

155
2

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

EDITED BY
SIR JOHN Y. W. MACALISTER
UNDER THE DIRECTION OF
THE EDITORIAL COMMITTEE

VOLUME THE SIXTEENTH

SESSION 1922-23

PART III

SECTIONS:—

OBSTETRICS AND GYNÆCOLOGY ODONTOLOGY OPHTHALMOLOGY
ORTHOPÆDICS OTOLOGY PATHOLOGY PSYCHIATRY SURGERY
THERAPEUTICS AND PHARMACOLOGY
TROPICAL DISEASES AND PARASITOLOGY UROLOGY WAR

INDEX



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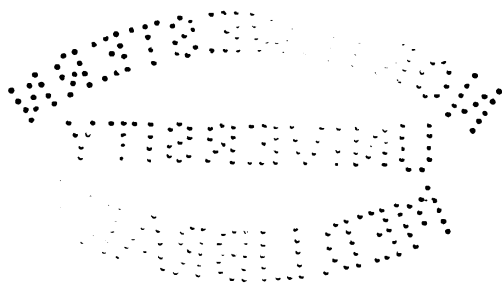
SESSION 1922-23

SECTION OF OBSTETRICS & GYNÆCOLOGY



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SECTION OF OBSTETRICS & GYNÆCOLOGY



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1923

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CONTENTS.

June 20, 1922.

	PAGE
Report of a Committee of the Section of Obstetrics and Gynæcology of the Royal Society of Medicine upon the Prognosis and Treatment of Eclampsia	1

October 5, 1922.

ARTHUR E. GILES, M.D., B.Sc., F.R.C.S.Ed.

(1) Large Fibroid of Cervix developing after Subtotal Hysterectomy ...	12
(2) Indications for and Results of Myomectomy for Uterine Fibroids ...	13

VICTOR BONNEY, M.S.

The Scope and Technique of Myomectomy (Abstract)	22
---	----

January 4, 1923.

Shown by EARDLEY HOLLAND, F.R.C.S.

A Specimen of Primary Carcinoma of the Vagina	25
--	----

Shown by THOMAS G. STEVENS, F.R.C.S.

Specimen of Squamous Epithelioma of the Vagina	26
---	----

HERBERT R. SPENCER, M.D.

Adenoma of the Vaginal Fornix simulating Cancer of the Cervix ...	27
---	----

A. H. RICHARDSON, F.R.C.S.

A Uterus removed for Carcinoma of the Cervix after Treatment by Radium	31
--	----

T. W. EDEN, M.D. (President), and AUBREY GOODWIN, M.D.

Two Cases of Cancer of the Cervix treated by Radium before Operation	32
--	----

ROBERT WISE, M.D.

Glycosuria, resulting in the Birth of a Dead Child, treated with success in a Subsequent Pregnancy	35
--	----

CLIFFORD WHITE, F.R.C.S.

Instruments left in the Peritoneal Cavity; The Effects and Results of this Accident as shown by an Analysis of Forty-four hitherto Un- published Cases	36
---	----

February 1, 1923.

Shown by J. S. FAIRBAIRN, B.M.	PAGE
(1) A Necrotic Fibro-adenoma in a Patient, aged 74, simulating Cancer of the Corpus Uteri	45
(2) A Cyst of the Uterine Cornu due to Dilatation of the Interstitial Portion of the Tube	45
BECKWITH WHITEHOUSE, .	
Adenomatosis Vaginae	46
W. R. WHITE-COOPER, M.B., B.S.Lond., and H. K. GRIFFITH, F.R.C.S.Eng.	
Inversion of the Uterus occurring in the Third Week of the Puerperium	48
SAMUEL J. CAMERON, M.B.	
The Technique of Cæsarean Section	50
BECKWITH WHITEHOUSE, M.S., F.R.C.S., and HENRY FEATHERSTONE, M.B.	
A Note on Two Cases of Cæsarean Section under Spinal Anæsthesia with Tropacocaine	55

March 1, 1923.

HENRY BRIGGS, F.R.C.S.	
(1) Angioma of the Vaginal Wall	61
(2) Section of Curettings	61
Shown by A. C. PALMER, F.R.C.S.	
A Mass of Secondary Leiomyosarcoma following Subtotal Hysterectomy	62
Shown by EARDLEY HOLLAND, F.R.C.S.	
A Leiomyosarcoma of a Fibromyoma removed by Subtotal Hysterectomy	64
Shown by J. D. BARRIS, F.R.C.S.	
Two Specimens of Sarcoma of the Uterus	65
Shown by S. GORDON LUKER, M.D.	
Chorion-epithelioma of the Uterus showing a very Extensive Growth in the Uterine Wall	67
SIDNEY FORSDIKE, M.D., F.R.C.S.	
The Treatment of Severe and Persistent Uterine Hæmorrhage by Radium, with a Report upon Forty-five Cases	69

May 3, 1923.

L. CARNAC RIVETT, F.R.C.S.	
(1) A Calcified Tumour of the Recto-vaginal Septum	81
(2) A Ruptured Hæmatoma of the Ovary, with Extensive Intra-peritoneal Hæmorrhage	81
ARCHIBALD DONALD, M.D., Ch.M.	
The Clinical Aspects of Adenomyomata of the Female Pelvic Organs ...	82
L. P. PUGH, B.Sc.Lond., F.R.C.V.S.	
Graves' Disease and Thyroid Instability in the Cow, and its Relation to Ovarian Disease	92

Contents

V

June 7, 1923.

HERBERT R. SPENCER, M.D.	PAGE
(1) Sarcoma in an Ovarian Dermoid Tumour	101
(2) Ruptured Unilateral Solid Cancer of Ovary; Ovariectomy; no Recurrence Six Years later	105
(8) Stretching of the Epithelium of the Tubal Rugæ by Blood effused into them in Torsion of Pedicle of Ovarian Tumour	106
HENRY RUSSELL ANDREWS, M.D.	
Carcinoma of a Prolapsed Cervix in a Woman, aged 77	109
LEONARD PHILLIPS, M.S., M.B., B.Sc.Lond., F.R.C.S.Eng.	
The Treatment of Dysmenorrhœa: An Analysis of 100 Cases	110

SECTIONS OF OBSTETRICS AND GYNÆCOLOGY, THERAPEUTICS AND PHARMACOLOGY.

(JOINT MEETING.)

December 7, 1922.

H. H. DALK, C.B.E., M.D., F.R.S.	PAGE
The Value of Ergot in Obstetrical and Gynæcological Practice; with Special Reference to its Present Position in the British Pharmacopœia	1
DISCUSSION: Sir NESTOR TIRARD, Dr. HERBERT SPENCER, Professor W. E. DIXON, F.R.S., Dr. T. W. EDEN, Mr. ALMCK BOURNE, Professor H. BRIGGS, Dr. DALK (reply), pp. 5-7.	

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Section of Obstetrics and Gynæcology.

President—Professor HENRY BRIGGS, F.R.C.S.

Report of a Committee of the Section of Obstetrics and Gynæcology of the Royal Society of Medicine upon the Prognosis and Treatment of Eclampsia.¹

THE Committee was appointed in October, 1921, and consisted of the following members: Mr. A. W. BOURNE, Dr. T. W. EDEN, Mr. EARDLEY HOLLAND, Professor LOUISE MCILROY, and Dr. HERBERT WILLIAMSON.

In order to assist them in the investigation the Committee subsequently co-opted the following: Mr. GORDON LEY (since deceased), Dr. A. J. MCNAIR, and Dr. EVERARD WILLIAMS.

The Committee desire to record their indebtedness to the three co-opted members for their very valuable assistance. The late Mr. Gordon Ley, with characteristic energy, undertook a large share of the very arduous work of analysing the returns of the 547 cases which were recorded, and the Committee would wish to make a special acknowledgment of their indebtedness to him, and of their deep regret that he did not live to see the completion of the work in which he had taken such a deep interest.

As the basis for a discussion upon the treatment of eclampsia it is necessary to obtain as accurate information as possible regarding the methods which have been adopted in recent years, the severity of the cases to which these methods have been applied and the results which have followed.

The Committee have therefore undertaken an examination of the whole of the cases admitted during a period of ten years to the following hospitals: St. Bartholomew's, Charing Cross, St. George's, Guy's, King's College, The London, St. Mary's, Middlesex, St. Thomas's, University College, Westminster, Royal Free, Queen Charlotte's Lying-in, City of London Maternity, General Lying-in.

We have included in our analysis those cases only in which convulsions occurred, and have endeavoured as far as possible to exclude cases of chronic renal disease complicated by uræmic fits in pregnant and parturient women.

The period selected, from 1911 to 1921, is an unfortunate one in some respects for it embraces the Great War during which, in many instances, the case-records were of necessity incomplete, and chemical and other investigations limited to those which were strictly essential.

A study of the material investigated shows that during the selected period 547 cases of eclampsia were admitted to these hospitals, and of these 427 patients recovered and 120 patients died. The maternal mortality is therefore 22·1 per cent.

Although our primary aim has been to ascertain the results of treatment, the analysis of the figures we have obtained has thrown light upon some

¹ Read at a meeting of the British Congress of Obstetrics and Gynæcology, Liverpool, June 30, 1922.

points in the natural history of the disease, and we have therefore included in our report such information on these points as appeared to be worth placing on record.

ÆTIOLOGICAL FACTORS.

Yearly Incidence.—From time to time the opinion has been advanced that the incidence of eclampsia is subject to wide yearly and seasonal variations. To test the truth of these opinions we have examined a series of 400 cases. On analysing these cases we find:—

In 1911	20	cases were admitted to hospital
„ 1912	26	„ „ „
„ 1913	22	„ „ „
„ 1914	53	„ „ „
„ 1915	43	„ „ „
„ 1916	34	„ „ „
„ 1917	45	„ „ „
„ 1918	40	„ „ „
„ 1919	56	„ „ „
„ 1920	61	„ „ „
				<hr/>
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The table shows that the largest number of admissions occurred in the years 1920, 1919, and 1914: the smallest number of admissions in the pre-war years 1913, 1912 and 1911; and that in the year 1920 the number of admissions was more than three times the number for 1911.

The figures do not support the view that eclampsia is an epidemic disease, nor would it be correct to draw the conclusion that eclampsia is on the increase. The larger number of admissions to hospital in recent years is more probably due to increased hospital accommodation and a wider recognition of the danger of the disease, so that there is an increasing tendency to transfer these patients to hospital rather than to treat them in their own homes.

Seasonal Incidence.—An examination of the same 400 cases shows that:—

In the month of January	28	cases were admitted
„ „ February	24	„ „
„ „ March	47	„ „
„ „ April	38	„ „
„ „ May	27	„ „
„ „ June	40	„ „
„ „ July	37	„ „
„ „ August	38	„ „
„ „ September	34	„ „
„ „ October	26	„ „
„ „ November	30	„ „
„ „ December	31	„ „
				<hr/>
				400

These figures suggest that the incidence of eclampsia is not liable to seasonal variations: thus, in this particular series the highest number of admissions took place in March and the lowest number in February: in the first half of the year 204 cases were admitted and in the second half 196.

Influence of Parity.—The necessary information is available in 488 cases. Of these patients, 341 were primigravidæ and 147 were multiparæ; giving a percentage of primigravidæ, 69·8 per cent.; multiparæ, 30·2 per cent.

Proportion of Twin Pregnancies.—The necessary information is available in 508 cases. Single births occurred in 467 (91 per cent.); twin births occurred in 41 (9 per cent.). If the average frequency of twins be taken as one in ninety deliveries, these figures indicate that twin pregnancy is much more liable to be complicated by eclampsia than is single pregnancy.

GROUPING OF CASES.

It is agreed that eclampsia is a disease which varies greatly in severity in different cases and although it is not possible to establish any fixed standard by which the severity of a case can be indicated in simple terms, yet it is clear that if we are to compare the effects of different forms of treatment some such standard is necessary, for if a particular treatment has been adopted in mild cases only, and another treatment in severe cases only, it is obvious that the results obtained in the two groups are in no way comparable. We have therefore endeavoured to ascertain what signs and symptoms are of grave import and to group the cases according to the presence or absence of these particular signs and symptoms.

With this object in view we have studied the effects upon prognosis of the following phenomena: Coma, the pulse-rate, the temperature, the number of fits, the amount of albumin in the urine, the degree of œdema, the blood-pressure.

Coma.—Three hundred and eighty-five cases have been analysed. The analyses were made by different members of the Committee and the results are not presented in precisely the same form. We have therefore to consider two series. *First*, a series of 262 cases in which three groups are distinguished, viz.: (1) Deep coma; (2) coma; (3) no coma, but drowsiness or restlessness. *Secondly*, a series of 123 cases in which two groups only are distinguished, viz.: (1) Cases with coma; (2) cases without coma.

Series I, 262 Cases.

Degree of coma	Number of cases	Recovered	Died	Mortality
Deep coma	60 (incidence 22·9 per cent.)	22 (36·6 per cent.)	38	63·4 per cent.
Coma	93 (incidence 35·5 per cent.)	74 (79·5 per cent.)	19	20·1 per cent.
No coma; drowsiness, or restlessness	109 (incidence 41·6 per cent.)	103 (94·5 per cent.)	6	5·4 per cent.

Series II, 123 Cases.

Group I.—Coma present: 75 cases, 21 deaths, mortality 28 per cent.

Group II.—Coma absent: 48 cases, 2 deaths, mortality 4·1 per cent.

Considering the two groups together we find that the mortality of cases with coma is 34·2 per cent., and of cases where the coma is described as "deep" 63·4 per cent., whereas the mortality of cases without coma is 5·09 per cent.

It appears therefore that coma is a grave symptom, and deep coma carries with it a bad prognosis.

The Pulse-rate.—Three hundred and forty-three cases have been examined and have been divided into three groups: Group I, where the pulse-rate is over 120; Group II, where the pulse-rate is between 120 and 90; Group III, where the pulse-rate is below 90.

Group	Number of cases	Recovered	Died	Mortality
I	110 (incidence 32·0 per cent.)	66	44	40·0 per cent.
II	159 (incidence 46·3 per cent.)	114	15	9·4 per cent.
III	74 (incidence 21·2 per cent.)	70	4	5·4 per cent.

A pulse-rate of over 120 is therefore of grave significance.

Temperature.—The temperature has been examined in 351 cases and the cases divided into three groups: Group I, temperature above 103° F.; Group

II, temperature between 103° and 100° F.; Group III, temperature below 100° F.

Group	Number of cases	Recovered	Died	Mortality
I ...	39 (incidence 11.1 per cent.)	10	29	74.3 per cent.
II ...	125 (incidence 35.6 per cent.)	111	14	11.2 per cent.
III ...	187 (incidence 53.2 per cent.)	176	11	5.9 per cent.

A temperature of over 103° F. is therefore of grave prognostic import.

Number of Fits.—The number of fits is recorded in 426 cases. A more detailed analysis of these will be made later. It will be sufficient to state here that the average number amongst those who recovered was 6.8, and amongst those who died 12.7. A larger number of fits than ten appears to be a grave sign.

Albuminuria.—The amount of albumin in the urine is recorded in 383 cases. We have divided the cases into four groups: Group I, albumin absent; Group II, a small amount; Group III, a large amount; Group IV, urine solid on boiling.

Group	Number of cases	Recovered	Died	Mortality
I ...	2 (incidence 0.5 per cent.)	2	—	—
II ...	38 (incidence 9.9 per cent.)	35	3	8.0 per cent.
III ...	213 (incidence 55.6 per cent.)	183	30	14.0 per cent.
IV ...	130 (incidence 34.2 per cent.)	97	33	25.3 per cent.

A urine which becomes solid on boiling is therefore a sign of grave danger.

Edema.—The absence of œdema or its degree when present is stated in 303 cases. It is difficult to establish a definite standard, but the cases fell into three groups: Group I, where a note is made that there was no œdema; Group II, where the œdema is described as "slight," moderate or local; Group III, where the œdema is described as "great" or "universal."

Group	Number of cases	Recovered	Died	Mortality
I ...	51 (incidence 16.8 per cent.)	36	15	29.4 per cent.
II ...	142 (incidence 46.8 per cent.)	120	22	15.4 per cent.
III ...	110 (incidence 36.3 per cent.)	92	18	16.3 per cent.

From these figures it appears that the absence of œdema is of grave significance, but the presence of widespread œdema is not.

Blood-pressure.—We find systematic records of blood-pressure in a series of 85 cases only. We have divided the cases in this series into three groups: Group I, below 140 mm. of mercury; Group II, between 140 and 200 mm. of mercury; Group III, above 200 mm. of mercury.

Group	Number of cases	Recovered	Died	Mortality
I ...	19 (incidence 22.3 per cent.)	15	4	21 per cent.
II ...	55 (incidence 64.7 per cent.)	44	11	20 per cent.
III ...	11 (incidence 13.0 per cent.)	7	4	36 per cent.

It appears, therefore, that a blood-pressure of above 200 is attended by a high mortality-rate.

From our study of these isolated symptoms we conclude that the following seven phenomena are signs of danger: Coma, a pulse-rate over 120, a temperature above 103° F., a number of fits greater than ten, a urine which becomes solid on boiling, the absence of œdema, a blood-pressure above 200 mm.

When a patient exhibited any two of the above phenomena the case has been grouped as a "severe" one, when these phenomena were absent, as a "mild" one.

Adopting this classification we find :—

Mild cases	264	...	60.1 per cent.
Severe cases	161	...	36.6 per cent.
Moribund	14	...	3.3 per cent.
				439		
Insufficient details for classification				108		
Total				547		

FURTHER STUDY OF THE MATERNAL MORTALITY.

The maternal mortality-rate has been further examined in relation to :—

- (1) Parity.
- (2) The period of gestation at which the eclamptic convulsions supervened.
- (3) The number of fits previous to treatment.
- (4) The incidence of fits before, during or after labour.
- (5) The sudden onset of convulsions without preceding symptoms.
- (6) The persistence of the fits after delivery.

Parity.—The number investigated was 458.

	Total	Recovered	Died
Primigravidae	332	269 (79.5 per cent.)	62 (20.5 per cent.)
Multiparæ	126	96 (76.4 per cent.)	30 (23.6 per cent.)

The mortality is therefore a little higher amongst multiparæ than amongst primigravidae.

The Period of Gestation at which Eclamptic Convulsions supervened.—The number of cases investigated is 413.

Period	Total	Recovered	Died	Mortality
Before 28th week	45 (incidence 10.8 per cent.)	34	11	24.4 per cent.
Between 28th and 32nd week	60 (incidence 14.4 per cent.)	48	12	20.0 per cent.
Between 32nd and 36th week	79 (incidence 19.1 per cent.)	61	18	22.7 per cent.
Between 36th week and full term	229 (incidence 55.4 per cent.)	188	41	17.9 per cent.

The mortality therefore amongst 184 cases before the thirty-sixth week is 22.2 per cent. Amongst 229 cases between the thirty-sixth week and full term 17.9 per cent. The mean mortality for the whole series of 547 cases is 22.1 per cent.

The Number of Fits previous to Treatment.—The number of cases investigated is 136: 109 patients who recovered had 513 fits before the commencement of treatment, an average of four fits each; 27 patients who died had 200 fits before treatment, an average of seven fits each.

The Incidence of Fits before, during or after Labour.—The number of cases investigated is 447.

Time	Total	Recovered	Died	Mortality
Before labour	287 (64.2 per cent.)	228	59	20.5 per cent.
During labour	84 (18.7 per cent.)	70	14	16.6 per cent.
After delivery	76 (17.1 per cent.)	55	21	27.6 per cent.

The mortality is therefore *greatest* when the onset of fits is post-partum and *least* when intra-partum.

The Sudden Onset of Fits without Preceding Symptoms.—The number of cases investigated is 386: It is recorded in 69 cases or 17.9 per cent. that no symptoms had been noticed before the fits; 59 of these patients recovered

and 10 died, mortality 14·5 per cent. We attach very little importance to these figures. Few of the cases had been under skilled observation and the history depended upon the statements of the patient or her friends. In none of them is there any record of urinary examinations.

Persistence of Fits after Delivery.—(Cases of post-partum eclampsia are not included.) The necessary information was available in 413 cases. Of this number the fits continued after delivery in 118 (incidence 28·5 per cent.) and ceased in 295. In cases classed as "mild" the incidence was about one-third lower than in those classed as "severe."

Of the 118 cases in which fits continued 26 died, 92 recovered, mortality 22·1 per cent.—this is exactly the same as the mean mortality of the whole series; 84 of the 118 cases were classified as either "mild" or "severe"; of these 36 mild cases had no mortality; the 48 severe cases had a mortality of 31 per cent.; the remaining cases were not classified.

Of the 295 cases in which fits ceased after delivery 64 died, 231 recovered, mortality 21·6 per cent.

From this it appears that fits may be expected to cease after delivery in about four out of five cases; further, the prognosis in those cases in which fits continue is not unfavourable since the "mild" cases all recovered and the "severe" cases showed a mortality very slightly higher than the mean mortality of all "severe" cases.

THE RESULTS OF TREATMENT.

In considering the results of treatment it must be recollected (1) that during a considerable part of the period of the Great War the hospitals were working under exceptional difficulties—e.g., the honorary staff were overworked and reduced in numbers, the resident staff were unusually inexperienced, and there was a shortage of drugs and appliances. (2) That the Report deals with the returns from fifteen separate hospitals in London; the methods of treatment employed were diverse, and the results very unequal in different hospitals. The results therefore represent the average over the whole of London, and in this respect they may be expected to compare unfavourably with the results obtained over the same period at individual hospitals.

The diversity of methods employed has made analysis of results of treatment very difficult; it is convenient to consider treatment under the headings of *Obstetric Treatment* including the method of delivery, and *Medical Treatment*.

In considering treatment we have taken only the cases which could be definitely classified as mild or severe; a certain small number were moribund on admission to hospital, and these we have excluded; a considerable number of records were lacking in the data necessary for classification, and they have also been excluded. There remain 425 cases, of which 264 were mild, and 161 severe. We attach importance to the classification of cases, because we believe that it will appear from the figures given below that regard should be had to the severity of the case in selecting methods of treatment.

The mean mortality of the 425 cases is as follows: Recovered, 356; died, 69; total, 425; mortality, 16·2 per cent.

If the cases are divided into mild and severe the figures are as follows:—Mild, 264—recovered, 247; died, 17; mortality, 6·4 per cent. Severe, 161—recovered, 109; died, 52; mortality, 32·4 per cent.

(A) *Obstetric Treatment.*

The cases fall into the following five groups:—

(I) *Natural Delivery*—i.e., spontaneous delivery without interference of any kind. Total 89, of which 11 died; mortality, 12·3 per cent. Mild, 50—recovered, 47; died, 3; mortality, 6 per cent. Severe, 39—recovered, 31; died, 8; mortality, 20·5 per cent.

(II) *Induction of Labour*, delivery thereafter being spontaneous. Total 83, of which 8 died; mortality, 9·6 per cent. Mild, 59—recovered, 56; died, 3; mortality, 5·1 per cent. Severe, 24—recovered, 19; died, 5; mortality, 20·8 per cent.

(III) *Assisted Delivery*.—These are mostly cases of low forceps, with a small number in which version was performed. Total 151, of which 22 died; mortality, 14·5 per cent. Mild, 100—recovered, 95; died, 5; mortality, 5 per cent. Severe, 51—recovered, 34; died, 17; mortality, 33·3 per cent.

(IV) *Cæsarean Section*.—Of the total six were vaginal operations, five recovered and one died. Total 88, of which 21 died; mortality, 23·8 per cent. Mild, 51—recovered, 46; died, 5; mortality, 9·8 per cent. Severe, 37—recovered, 21; died, 16; mortality, 43·2 per cent.

(V) *Accouchement Forcé*.—Total 14, of which 7 died; mortality, 50 per cent. Mild 4—recovered, 3; died, 1; mortality, 25 per cent. Severe, 10—recovered, 4; died, 6; mortality, 60 per cent.

In order to dissociate as far as possible the influence of the disease from the method of delivery, as factors in mortality, the results in the mild and severe cases may be compared. The mean mortality in mild cases is 6·4 per cent., and in severe cases 32·4 per cent., a ratio of exactly 1 to 5. If the mild cases delivered by the three simpler methods are compared with the severe cases delivered by the same methods the mortality-rates are 5·2 per cent. and 26·3 per cent. respectively—i.e., the ratio of 1 to 5 is almost exactly maintained. If we compare in the same manner the cases delivered by Cæsarean section we find that the mild cases showed a mortality of 9·8 per cent., the severe cases a mortality of 43·2 per cent., which also corresponds closely to the mean ratio of 1 to 5. The cases delivered by accouchement forcé are excluded, because the results are such as to condemn the use of this method under any circumstances. Hence it appears that cases which can be classified as severe may be expected to show a mortality-rate five times greater than those classified as mild, no matter what method of delivery is adopted.

It is also clear that the cases in which there was no obstetric interference, or in which simple methods only were adopted, show a much lower mortality than those delivered by Cæsarean section and accouchement forcé. The mean mortality of the various methods in order are as follows: Induction, 9·6 per cent.; natural delivery, 12·3 per cent.; assisted delivery, 14·5 per cent.; Cæsarean section, 23·8 per cent.; accouchement forcé, 50·0 per cent.

If now the results of the different methods as they appear in the mild cases only are compared with one another we get: Induction, 5·1 per cent.; assisted delivery, 5·0 per cent.; natural delivery, 6·0 per cent.; Cæsarean section, 9·8 per cent.

It appears therefore that in mild cases Cæsarean section increases the maternal risk to the extent of nearly two to one, and that cases delivered by the three simpler methods have much the better chance.

If now the results in the severe cases are compared we get: Natural

delivery, 20·5 per cent.; induction, 20·8 per cent.; assisted delivery, 33·3 per cent.; Cæsarean section, 43·2 per cent.

Here also the cases in which there was a minimum of obstetric interference show much the best results.

It must be pointed out that within the limits of the classes "mild" and "severe" there were no doubt gradations of severity, and the graver cases would perhaps be those in which expeditious delivery was regarded as necessary. Our perusal of the records however show that this was by no means always the case.

(B) *Medical Treatment.*

One or two general observations are called for in regard to the medical treatment carried out in this series of cases.

In the first place it is clear that medical treatment has seldom been carried out upon any definite plan. One thing after another has been done without method, and with such rapidity that in many instances the effects of one could hardly become apparent before the next had been begun. Secondly, in addition to being disorderly, medical treatment has also been, generally speaking, excessive. The patient has been subjected to a multiplicity of drugs, and to a succession of procedures such as venesection, saline transfusion, hot packs, and gastric and colonic lavage, which might be expected to reduce a parturient woman in good health almost to the point of death, and must have done serious harm to one suffering from a grave toxæmia.

An attempt has been made to glean from the records some information as to the value of medical treatment as a whole, and also as to the value of individual methods of treatment. For this purpose the various methods of medical treatment were grouped in the manner set forth below. The cases which terminated fatally were first selected for investigation by analysing the medical treatment adopted in each case. The task of analysing the medical treatment in the whole series of 547 cases was too formidable to be undertaken. In the next place *venesection* and *administration of morphia* were selected as representing individual methods of treatment, and a record made of *all the cases* in which these methods had been adopted, both those which recovered and those which died.

The information required for analysing the nature and extent of the medical treatment adopted in the fatal cases in this series was available in 71 cases. For convenience of analysis medical treatment has been divided into the following groups: (a) Elimination (including purgatives except croton oil, enemata, gastric and rectal lavage, rectal saline, &c.); (b) Venesection; (c) Intravenous saline; (d) Morphia and other sedatives; (e) Croton oil; (f) Hot packs; (g) Veratrine.

In the large majority of cases the treatment adopted ranged over three or more of the above groups. In only seven instances was simple treatment falling under one heading only adopted. Elimination only in 1 case, intravenous saline only in 2 cases, morphia only in 3 cases, veratrine only in 1 case. In 15 cases the treatment comprised two groups, elimination and morphia being the combination most often employed. In 24 cases treatment comprised three groups, elimination, morphia, and hot packs, or elimination, morphia and veratrine being the most commonly employed combination. In 23 cases complex and varied treatment comprising four or more groups was carried out. A few cases were subjected to treatment by elimination, venesection, intravenous saline, morphia, hot packs and veratrine. It is impossible

to avoid the conclusion that the majority of the fatal cases were over-treated, and that in a considerable number the excessive treatment must have been a contributory factor in bringing about the fatal results.

A further attempt has been made to arrive at an opinion as to the value of individual methods of treatment; in respect of only two methods were the cases numerous enough and the data sufficiently proved to warrant conclusions being drawn from them. These methods were venesection and the administration of morphia with or without other sedatives; cases in which less than one half a grain of morphia had been given were not included. The results are as follows:—

Venesection: 143 cases. Mild cases, total 60: Recovered, 53; died, 7; mortality, 11·6 per cent. Severe cases, total 83: Recovered, 44; died, 39; mortality, 47·0 per cent. The mean mortality of the mild cases in the whole series of 425 cases was 5·4 per cent. (*see* p. 6), the cases in which venesection was done showed double this mortality-rate. The mean mortality of the severe cases in the whole series was 34·3 per cent.; the severe cases treated by venesection showed a mortality of more than one third greater than this. So far as these results may be relied upon it therefore appears that venesection is a method from which little benefit is to be expected, and which may do harm. In one case 40 oz. of blood were taken by venesection and the patient died.

Morphia and other Sedatives: 69 cases. Mild cases, total 39: Recovered, 36; died, 3; mortality, 7·8 per cent. Severe cases, total 30: Recovered, 14; died, 16; mortality, 53·3 per cent. These results are no more encouraging than those of venesection.

It must however be recollected that in the case both of venesection and of morphia very few of these cases were treated by the above methods only; many other things were done to the patient as well, the effects of which cannot be disentangled from those of venesection or of morphia.

THE CAUSES OF DEATH IN THE FATAL CASES.

The proportion of cases in which a post-mortem examination was made was very small, and the cause of death specified is the clinical cause in the majority of cases. The notes are in many instances wanting in any indication as to the cause of death, and in only 87 of the 120 cases which terminated fatally was any information available, and that was often of a fragmentary nature.

Of the 87 cases, in 27 no reliable opinion as to the cause of death can be expressed: in 36, death was attributed to eclampsia, and in this group are a fair number of autopsies: in 5 cases death was due to cerebral hæmorrhage, these being all cases in which a post-mortem diagnosis could be made. In 10 cases pulmonary complications such as pneumonia, bronchitis, pulmonary embolism and œdema of the lungs were the cause of death: the other causes specified are uræmia (one case), suppression of urine (one case), heart failure, shock, chloroform poisoning, general peritonitis, and accidental hæmorrhage.

In one of the fatal cases, two pints of blood were taken by venesection, and the patient was delivered by accouchement forcé; this case was classified as "mild on admission," and it seems probable that any chance she had of recovery was destroyed by the severity of the remedial measures employed. In two cases there was post-mortem evidence of acute tracheitis and œdema of the lungs; in both croton oil had been administered, and it seems probable

that here also the methods of treatment employed were to a great extent responsible for the fatal result.

If the figures are looked at as a whole, we may take it that deaths attributed to eclampsia, to cerebral hæmorrhage, to uræmia or to suppression of urine are the direct outcome of the disease. These account for 47 cases out of 60 in which the cause of death could be determined, i.e., 71·5 per cent. In the remainder—viz., pulmonary complications, heart failure, shock, chloroform poisoning, general peritonitis, and accidental hæmorrhage, it is, at the least, an arguable proposition, that by more judicious management of the cases a great number of these fatalities could have been averted.

THE FŒTAL AND NEO-NATAL MORTALITIES.

In the following tables three groups of cases are considered :

(1) *Fœtal deaths*, including ante-partum, intra-partum, and post-partum death of the fœtus. Some of these fœtuses were born with the cord still pulsating but respiration was never established.

(2) *Neo-natal Deaths*.—In this group are included all children who died whilst the mother was still in hospital.

(3) *Survivals*.—This group includes all children who left the hospital alive.

Many of these children were premature, and in considering the fœtal mortality in relation to treatment a distinction has been made between those born before and those born after the thirty-fourth week of gestation, because in any case the probability of survival of a child born before the end of thirty-four weeks' gestation is small.

TOTAL MORTALITY.

Examination of 448 cases gives the following figures : Fœtal deaths, 156 (34·8 per cent.) ; neo-natal deaths, 50 (11·1 per cent.) ; survivals, 242 (54·0 per cent.). Thus of the children born of these cases of eclampsia rather more than half left hospital alive.

RELATION OF FŒTAL MORTALITY TO NUMBER OF FITS.

The number of cases analysed is 298 and the number of children born is 303. Of these 156 children were born alive and the mothers of these children had between them 726 fits—an average of 4·6 fits for each delivery. 147 children were born dead and the mothers had 1,234 fits—an average of 8·4 fits for each delivery.

RELATION OF FŒTAL MORTALITY TO THE TIME OF THE ONSET OF FITS.

Fits occurred *before the onset of labour* in 177 births : 74 children survived, 103 children died, mortality 58·2 per cent. Amongst these were many premature births.

The onset of fits was *during labour* in 33 cases : 24 children survived, 9 children died, mortality 27·3 per cent.

The onset of fits was *after labour* in 39 cases : 34 children survived, 5 children died, mortality 13 per cent.

THE EFFECTS OF METHODS OF DELIVERY UPON FŒTAL MORTALITY.

In looking into this matter we have excluded all cases in which delivery took place earlier than the thirty-fourth week; before this the chances of survival of the child are very small.

In 206 cases the necessary data were available and of these 137 survived—i.e., they left hospital alive, and 69 died (still-births and neo-natal deaths); the foetal mortality was accordingly 33·5 per cent. A comparison of the different methods of delivery gives the following results :—

	Total	Survived	Died	Mortality
Natural or assisted delivery ...	88	62	26	29·6 per cent.
Induction ...	41	18	23	56·1 per cent.
Cæsarean section ...	74	57	17	23·0 per cent.
Accouchement forcé ...	3	0	3	100·0 per cent.

The low foetal mortality of Cæsarean section in comparison with that of induction is remarkable. It is probably explained by the fact that an induced labour is nearly always prolonged, and the high foetal mortality is probably accounted for by the vulnerability of the child.

If the results in the cases born before the thirty-fourth week are examined separately, the foetal mortality is 80 per cent.

The fate of the child in eclampsia is necessarily subordinate entirely to the interests of the mother, and we do not wish to attach undue importance to the consideration of these figures.

Signed on behalf of the Committee,

June 15, 1922.

T. W. EDEN, CHAIRMAN.

Section of Obstetrics and Gynæcology.

President—Dr. T. W. EDEN.

Large Fibroid of Cervix developing after Subtotal Hysterectomy.

By ARTHUR E. GILES, M.D., B.Sc., F.R.C.S.Ed.

IN October, 1915, I saw a patient, aged 39, sent to me by Dr. (now Sir Bruce) Bruce-Porter. She was known to have had fibroids for four years, and had lately been suffering from increasing hæmorrhages, the menstrual periods being both too frequent and too free.

On examination, a large group of fibroids was found occupying the pelvis and rising up into the abdomen. Operation was decided upon, and a subtotal hysterectomy was done three weeks later. Owing to the presence of a right ovarian cyst, the appendages of the right side were removed. The left appendages, being matted down under adhesions to the floor of the pelvis, were not disturbed.

A year later the general health was good, but she complained of sacral pain. A swelling the size of a billiard ball was found on the left side, and a cyst of the left ovary was diagnosed.

In October, 1917, the pelvic condition felt the same on palpation, but as symptoms were not pronounced, I advised against operation. Five years of very good health followed, and the patient was able to live a very active and useful life.

In August, 1922, Sir Bruce Bruce-Porter asked me to see her again. She had been conscious of abdominal swelling and discomfort, and Sir Bruce had found a hard tumour above the pubes. On examination this hard tumour was found to be filling up the true pelvis, and rising also into the abdomen. Diagnosis was difficult. Sir Bruce had suggested a fibroid, but as I had never known a fibroid arise in the cervical stump after subtotal hysterectomy, I felt doubtful about this, and feared that the left ovarian cyst had developed a malignant growth. Early operation was urged, and was carried out shortly afterwards.

Extensive adhesions between the bowel and the bladder shut out the pelvic contents from view; after these had been separated, the tumour was reached, encompassed by adhesions on every side. It was carefully shelled out, and proved to be a large reniform tumour, attached to the top of the cervical stump by what would correspond to the hilus of a kidney. The tumour was removed and the cervical stump dissected out. Convalescence was temporarily complicated by a collection of blood in the pouch of Douglas, evidently arising from adhesion sites, and judging by the offensive smell, contaminated by *Bacillus coli*. This was evacuated by passing the finger through the divided upper end of the vagina, and after that recovery was satisfactory. The tumour was sent to Dr. Eastes' laboratory for examination, and the following report was received: "This large growth has the histological structure of fibromyoma,

[October 5, 1922.]

consisting of interlacing bundles of plain muscle tissue and fibrous tissue. It is innocent. A section was also cut of the cervix, but this presents no special pathological features."

There is no doubt that the development of a fibroid in the cervical stump after a subtotal hysterectomy is a very rare occurrence, hitherto unknown to me in an experience of nearly 1,000 hysterectomies for fibroids, the majority of which were subtotal. Sir Bruce Bruce-Porter tells me that he has met with another instance of it in another patient, but that and the case now recorded are the only cases within my knowledge.

Those who advocate total hysterectomy in every case of uterine fibroids would doubtless claim this occurrence as an argument in favour of their view. Personally, I prefer the subtotal operation in most cases; and I should not feel disposed to alter my practice on account of a possibility that may occur once in a thousand cases.

DISCUSSION.

Dr. HERBERT SPENCER said a considerable number of cases of development of fibroids in the cervical stump after amputation of the uterus had been published. It was only another instance of the advantage of total hysterectomy, which he had practised exclusively for the last twenty-two years, and had on many occasions recommended to the Section. The occurrence of fibroids in the stump was, of course, not so common as the occurrence of cancer, which was said by some to be "rare." Seeing that one American gynaecologist had collected 276 instances of the occurrence of cancer in the stump in the practice of American gynaecologists alone, and that William Mayo, of Rochester, U.S.A., had recently published his opinion that total hysterectomy was the better operation, he hoped that more Members of the Section would become convinced of its superiority over the partial operation. After total hysterectomy, leaving the vagina widely open for drainage prevented those exudates and accumulations of blood which were met with in Dr. Giles's case, and were not uncommon after subtotal hysterectomy. He asked how Dr. Giles treated the vagina.

Dr. GILES (in reply) said that his usual practice in the case of total hysterectomy was to suture the top of the vagina at the sides leaving the middle open for drainage.

Indications for and Results of Myomectomy for Uterine Fibroids.

By ARTHUR E. GILES, M.D., B.Sc.

IN recent discussions on the treatment of uterine fibroids, the alternatives of hysterectomy on the one hand and X-ray and radium treatment on the other have generally been dealt with as though they exhausted the possibilities of treatment. Myomectomy has been almost entirely overlooked. At the meeting of this Section in March, 1922, a paper was read by Dr. Fletcher Shaw, of Manchester, on the X-ray treatment of fibroids, as a sequel to a former paper by Dr. Eden and Mr. Provis.¹ I then felt that the case for myomectomy might well be presented, and this contribution is the result.

I hold the view that in the majority of cases of fibroids requiring surgical treatment hysterectomy is the more suitable and also the more satisfactory operation, and that the scope of myomectomy is restricted to the minority of cases. Hysterectomy requires no justification: it is generally accepted

¹ *Proceedings*, 1922, xv (Sect. Obst. and Gyn.), pp. 51-56.

as a sound procedure, surgically and clinically. But myomectomy needs to justify itself by its results, and to state reasons why it should be preferred; it must also acknowledge its limitations and define the conditions of its suitability.

The outstanding claims for myomectomy are first that the uterus is preserved for the important function of childbearing; and secondly that the patient is thereby spared the mental distress of feeling that an essential part of her womanhood has gone.

The broad-minded advocate of myomectomy will, I think, at once concede that in a woman who is past the child-bearing age the first claim disappears, and the second has only an academic existence; although, as I shall point out, there may be reasons for preserving the uterus that are not founded on its child-bearing function. But we may properly narrow the issue to the consideration of myomectomy *versus* hysterectomy in the child-bearing period.

The advocate of myomectomy is then, I think, called upon to show in the first place that the preserved uterus is, in fact as well as in theory, capable of child-bearing; and in the second place, that there is a reasonable prospect of a cure, both anatomically and symptomatically; in other words, that in a large majority of cases there will be no return of fibroids, and that the patient will menstruate normally.

The case for myomectomy will best be presented if we begin by examining the question whether it is justified by its results; and if the answer proves to be in the affirmative, we can then consider the scope and limitations of the operation.

A separate short section will be devoted to the interesting subject of myomectomy during pregnancy.

RESULTS OF MYOMECTOMY.

We may glance first at the immediate results, namely, the mortality, as compared with hysterectomy; for this purpose my own statistics will serve as well as any. The figures up to date are as follows: hysterectomy, for fibroids, 987 cases, seventeen deaths, mortality 1·72 per cent.; myomectomy, 167 cases, three deaths, mortality 1·8 per cent.

The above figures represent all cases from 1897. The present-day mortality is lower, and the figures from the end of 1910 to date are as follows: Hysterectomy, 684 cases, five deaths, mortality 0·73 per cent. Myomectomy, 107 cases, one death, mortality 0·93 per cent.

There is thus very little difference in the mortality of the two operations; certainly not enough to make this a deciding factor in the choice of operation.

Later Results.

In my work on the "After-results of Abdominal Operations," published in 1910, I was able to give particulars of forty cases traced out of fifty-one operated upon. Of the later 116 cases, I have particulars of fifty-three. I did not attempt to trace the histories of patients who were over 45 years of age at the time of operation.

I will take in order the three questions investigated in the previous cases:—

(1) *What is the Likelihood of a Recurrence of Fibroids?*—In the first series, of thirty-nine cases examined, the uterus was found to be of normal size, with no return of fibroids, in thirty-five cases, or 90 per cent.

In the present series, of forty-nine cases examined, or reported upon by their doctors, the uterus was normal in size without fibroids in forty-one cases; it was enlarged, without fibroids, in three cases; in all, forty-four cases without fibroids, or 90 per cent., as in the first series. In five cases fibroids were observed; the notes about them are the following:—

- (1) Fibroid in fundus: hysterectomy advised on account of bleeding.
- (2) Fibroid, size of pea on posterior wall.
- (3) Multiple fibroids, hysterectomy done nine years after myomectomy.
- (4) Fibroid, size of mulberry on anterior wall.
- (5) Fibroid, size of pea on anterior wall.

The conclusion is that the liability to return of fibroids may be represented as 10 per cent.

§(II) *How is the Menstrual Loss affected?*—In the first series, the menopause had occurred before the operation in three cases, and in one case there was no further loss after operation. Of the remaining thirty-six cases, menstruation was normal or moderate in thirty-one, or 84·5 per cent.; and free or profuse in five, or 15·5 per cent. In the present series we can exclude six cases in which the menopause preceded or synchronized with the operation. Of the remaining forty-seven cases menstruation was normal and moderate in thirty-six, or 75·1 per cent.; and in eleven cases it was profuse, either at first, or later on; or there was intermenstrual loss. Taking all the cases together, we find that of eighty-three cases, menstruation was normal in sixty-seven, or 80·7 per cent. The conclusion is that there is a liability to menorrhagia or metrorrhagia after myomectomy in about 20 per cent. of cases. This result is not altogether surprising; because it is clear that unless we open the uterine cavity every time we do a myomectomy, some small intra-uterine growth may easily be overlooked. In order to show that sufficient time had elapsed to enable one to judge of results, I may mention that the time between the operation and the report on the cases in the present series was as follows:—

1 to 2 years	4 cases	7 to 8 years	2 cases
2 to 3 "	8 "	8 to 9 "	6 "
3 to 4 "	2 "	9 to 10 "	7 "
4 to 5 "	5 "	10 to 11 "	3 "
5 to 6 "	1 "	13 years	1 "
6 to 7 "	12 "				

(III) *Is the Uterus serviceable for Child-bearing after Myomectomy?*—In the first series, there were fifteen married women under 45 years of age: of these, one was four months pregnant at the time of operation, went to term, and became pregnant again; two others became pregnant after the operation. In the present series I have after-histories of thirty-seven married women aged under 45, including two who were single at the time and were married afterwards. I have drawn up a table of these cases (see Table I, p. 16). One, who was pregnant at the time of operation, died of eclampsia at her confinement; and two did not live again with their husbands after operation. Of the remaining thirty-four, ten became pregnant after the operation, including two patients who were pregnant at the time, went to term, and became pregnant again. One patient had two children, and another had four. In all, therefore, of forty-nine who had the chance of conceiving after operation, thirteen became pregnant, or 26 per cent.

Now the question that we have to decide is this: are the results of myomectomy good enough to justify us in practising this operation in suitable cases? My answer is an unqualified "Yes," on the ground that the advantages far outweigh the disadvantages.

The advantages are represented by the occurrence of pregnancy in 25 per cent. of the women who had a chance of pregnancy, and by the satisfaction which patients derived from the consciousness that they remained after the operation "like other women." This satisfaction cannot be expressed in statistics, but it is nevertheless a very concrete advantage.

TABLE I.—RESULTS OF MYOMECTOMY IN RELATION TO PREGNANCY.

Number in present series	Age	Date of operation	Pregnancies after operation
2	38	November 11, 1909	None; husband has been in an asylum all the time
10	18	June 12, 1911	Married since; confinement, October 1921
13	(single)	March 14, 1912	None
14	30	April 15, 1912	Pregnancy at operation; went to term; none since
19	33	June 30, 1912	None
23	30	January 14, 1913	None
25	32	January 30, 1913	None
26	35	February 8, 1913	Married in 1914; confinements—April, 1916, and March, 1919
27	(single)	March 17, 1913	Pregnancy at operation; went to term; none since
32	41	September 25, 1913	None
33	31	October 6, 1913	Confinements—January, 1915, April, 1917, March, 1919, August, 1922; before operation had been married six years, without children
34	37	November 29, 1913	None
39	38	April 24, 1914	None; died in May, 1918
44	34	June 8, 1914	Confinement—October, 1917; none since
45	34	July 9, 1914	None
46	29	October 14, 1914	Pregnancy at operation; went to seven and half months; none since
47	42	October 22, 1914	None
49	30	November 6, 1914	Confinement since, date unknown
51	35	December 2, 1914	Confinement, June, 1916
58	34	October 7, 1915	Pregnancy at operation; went to term; another confinement, May, 1920
59	35	October 11, 1915	Pregnancy at operation; went to term; another confinement, January, 1919
61	35	November 11, 1915	None
62	32	January 3, 1916	None
63	37	April 17, 1916	Pregnancy at operation; went to term; none since
64	34	May 1, 1916	None; has not lived with her husband since operation
69	37	October 9, 1916	Pregnancy at operation; went to term; patient died of eclampsia
71	30	January 11, 1917	None; premature menopause, April, 1921
72	35	January 22, 1917	Confinement, February, 1918
81	35	November 26, 1917	None
82	33	February 6, 1918	None
88	40	December 30, 1918	Pregnancy at operation; went to term; none since
92	36	November 25, 1919	Pregnancy at operation; went to term; none since
97	36	February 5, 1920	Pregnancy at operation; went to term; none since
98	31	March 18, 1920	None
100	31	April 14, 1920	None
105	29	July 20, 1920	Confinement, February, 1922
106	38	July 21, 1920	None

The disadvantages are the possibility of the return of fibroids which is a 10 per cent. chance; and the possibility of further menstrual trouble, which is a 20 per cent. chance. Some women, doubtless, would prefer the assurance given by hysterectomy, that there will be no return of their troubles, and that the operation is therefore final. But I am persuaded that the great majority of women would prefer to take the risk which the figures indicate, for the possible chance of having a baby. The risk of a second operation being

necessary is not as great as 10 per cent. ; hysterectomy had been required in only five cases out of the total 167, that is, in just 3 per cent.

I submit therefore that, judged by the criterion of results, the advantages of myomectomy definitely outweigh its disadvantages in the surgical treatment of uterine fibroids, in a restricted class of cases. My own sense of this restriction is sufficiently expressed in the fact that of 1154 cases of uterine fibroids treated surgically hysterectomy was done in 987, or 85·5 per cent. The proportion of myomectomy to hysterectomy was thus 1 to 6.

We may now examine the scope and limitations of myomectomy.

INDICATIONS FOR MYOMECTOMY.

There is one important indication for myomectomy and there are several subsidiary ones. I will take the important one first :—

(1) *The Fact that the Patient is of Child-bearing Age.*—The younger the patient, the greater should be the effort to save the uterus, and I should say that, broadly speaking, myomectomy is the operation of choice in women up to the age of 40. In single women, the indication is not so great as it is in married women, especially after the age of 30 ; because after this age the expectation of child-bearing is necessarily less in the spinster from the fact that she has first to get married. The generalization of age in the case of married women may well be qualified by the question of previous pregnancies : when a woman has half-a-dozen living and healthy children we need not be so concerned to save the uterus as we should be in the case of a woman who has had no children and is ardently desirous of having one. This reflection is well illustrated in a case of myomectomy during pregnancy, to which I shall refer later. The influence of age and of the married state upon my own practice is set forth in the table given (Table II, p. 20) which shows the proportion of myomectomies to hysterectomies at different ages, in single women, in married women, and in the two groups taken together. In five-yearly periods the percentage of myomectomies, which stands at 56·6 for all cases under 30 in married women, progressively falls to 40·1, 26·7, 6·1 and 5·5 and rises a little to 10·0 in the case of women over 50. In single women, the percentage is 45·0 for all cases under 30, and falls to 21·3, 8·3, 8·6, and 4·0, and rises to 10·0 in those over 50. Taking all the cases together, the percentage is 52·0 in women under 30, and in successive five-yearly periods it is 34·3, 19·2, 6·9, and 5·1 ; and for women over 50, 10·0. It will be noticed that the proportion of myomectomy is distinctly higher in married women than in spinsters up to the age of 40 ; after that age there is not much difference. The chart herewith (p. 18) illustrates these figures in graphic form.

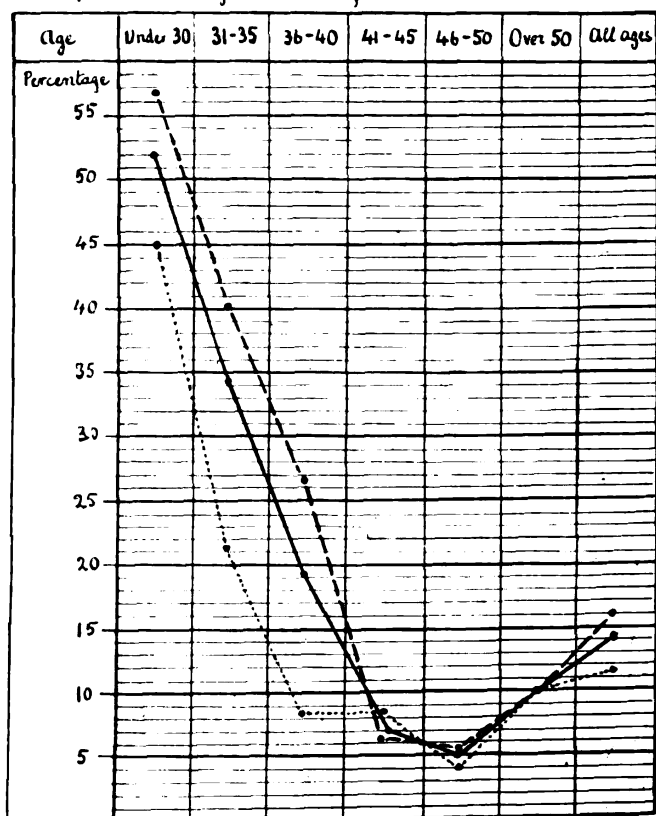
(2) *Association of Uterine Fibroids with Prolapse and Procidentia.*—The rise in percentage of myomectomies after the age of 50 is explained by the fact that a number of these patients had prolapse or procidentia. In the presence of this complication I consider it very important to preserve the uterus ; because this can be fixed up to the abdominal wall when myomectomy has been done, and thus play an essential part in the cure of the displacement. When the uterus is removed in conditions of procidentia there is a marked tendency to prolapse of the vaginal walls, which is apt to be very troublesome, and in severe cases to defy even extensive plastic operations on the vaginal walls and perineum.

(3) *A Deep-rooted Objection on the Patient's Part to Hysterectomy.*—In my experience, the great majority of women suffer no more than a passing regret for the loss of the uterus, especially when it is explained to them that the

presence of the tumours has rendered it incapable of serving its one function, that of child-bearing; that the uterus is not the essential organ of sex, and that its removal has no effect on the specially feminine characteristics and does not render a woman any the less able to live a married life. But there are a few women, swayed more by sentiment than by reason, to whom the loss of the womb seems a greater calamity than the loss of life itself: to them this organ appears to be the focus and essence of their personality as women, and if hysterectomy be done, it is apt to be followed by a profound and lasting depression which approaches the border-line of mental instability. In dealing

A Chart

showing the percentage of Myomectomies in Operations for Fibroids at different ages (Continuous line). Married women only, interrupted line. Single women only, dotted line.



with a patient of this kind, it is clear that a special effort should be made to save the uterus, even if the age of child-bearing be past, and myomectomy entails a rather higher operative risk and the possibility of a return of fibroids. But in such circumstances the surgeon may feel disposed to advise the adoption of X-ray treatment, rather than to undertake what seems to him to be an undesirable surgical procedure.

(4) *Certain Characters of the Tumours.*—When a tumour is solitary and pedunculated, and not associated with excessive bleeding, a myomectomy may properly be done, whatever may be the age of the patient, and the chances of

pregnancy. Even an interstitial tumour, unassociated with hæmorrhage, may be enucleated. In fact, when none of the previously considered factors have to be considered, the deciding question will probably be: "What is the simplest and safest operation?"

THE LIMITATIONS OF MYOMECTOMY.

We have so far considered conditions in which myomectomy should be done unless there is some strong reason to the contrary. We have now to enumerate those in which myomectomy should not be done unless there is some preponderating reason for it:—

(1) *The Age Factor.*—After the age of 40 or 45, I consider that hysterectomy should be the rule, especially when there are multiple fibroids and when there has been excessive hæmorrhage.

(2) *The Condition of the Uterine Appendages.*—When fibroids are associated with double tubal disease or with bilateral ovarian tumours, hysterectomy is the proper procedure.

(3) *Size and Position of the Tumours.*—Whatever be the position or number of the tumours, if myomectomy is going to leave a battered and useless organ, hysterectomy should be done. Cervix-fibroids almost invariably call for hysterectomy, though I have occasionally done a myomectomy even in these cases. A fibroid in the broad ligament may, if enucleated, leave the uterine vessels in a highly vulnerable condition: my first myomectomy was of this kind, and I had to re-open on account of hæmorrhage. The mere number of the tumours does not greatly matter, as long as a good and serviceable uterus is left.

(4) *Excessive Hæmorrhage.*—When a patient has had great losses and is seriously drained thereby, hysterectomy should be the rule. If urgent reasons call for an attempt at myomectomy, the uterine cavity must be opened, and any submucous or intra-uterine growth must be removed. Otherwise hæmorrhage will continue, and the operation will have been done in vain.

(5) *The Temperament of the Patient.*—Just as there are a few women whose temperament renders it important to save the uterus, so there are others whose temperament will turn the scale in favour of hysterectomy. They are the patients to whom an operation is such an ordeal that whilst they will face it bravely if they have the assurance that it is going to mean a definite cure, they could not tolerate the prospect that the ultimate result of the operation might be doubtful. With them even considerations of possible future pregnancy and of preservation of femininity have no weight compared with the all-important need for being completely and finally cured. With such a patient, if there be any doubt at all as to the issue of a myomectomy, hysterectomy should be done.

MYOMECTOMY DURING PREGNANCY.

Fibroids associated with pregnancy do not necessarily call for surgical treatment. When they are causing no symptoms, and do not seem likely to interfere with labour, it is best to leave them alone, always provided that the patient can be kept under observation so that operation can be undertaken if urgent symptoms arise, and that the medical attendant is on the look-out for trouble during labour. After the confinement, the fibroids can be dealt with on their merits. Surgical treatment may take the form either of myomectomy during pregnancy, or of Cæsarean section combined with myomectomy or hysterectomy at term. In exceptional cases, hysterectomy in the early

months of pregnancy may be required, but it should only be done if symptoms are urgent and there are cogent reasons against the adoption of conservative procedures.

Myomectomy during pregnancy is indicated in three groups of conditions. First, when the tumour or tumours appear to be increasing rapidly in size; secondly, when the patient is suffering from pain, pressure symptoms or indications of septic or degenerative changes in the tumours; thirdly, when the position of the fibroids makes it probable that labour will be obstructed, in which case myomectomy is done for the purpose of avoiding the necessity for Cæsarean section and of allowing the confinement to take place naturally.

In the second group, severe and persistent pain is of course a sufficient indication in itself. The effects of pressure may be exerted in relation to the bowel, the bladder, the diaphragm, the iliac vessels, or the nerves of the sacral plexus; and any one of these may call for interference. With regard to degenerative changes, it is well known that when fibroids are associated with

TABLE II.—SHOWING THE PROPORTION OF MYOMECTOMIES TO HYSTERECTOMIES AT DIFFERENT AGES.

Operation	30 and under		31-35		36-40		41-45		46-50		Over 50		Total	
	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Per cent.	No.	Per cent.
<i>Married women—</i>														
Myomectomy ...	17	56.6	37	40.1	36	26.7	13	6.1	11	5.5	11	10.0	125	16.0
Hysterectomy ...	13	43.4	53	59.9	103	73.3	201	93.9	190	94.5	99	90.0	659	84.0
Total ...	30	100.0	90	100.0	139	100.0	214	100.0	201	100.0	110	100.0	784	100.0
<i>Single women—</i>														
Myomectomy ...	9	45.0	10	21.3	7	8.3	8	8.6	3	4.0	4	10.0	41	11.4
Hysterectomy ...	11	55.0	37	78.7	78	91.7	85	91.4	72	96.0	36	90.0	319	88.6
Total ...	20	100.0	47	100.0	85	100.0	93	100.0	75	100.0	40	100.0	360	100.0
<i>All cases—</i>														
Myomectomy ...	26	52.0	47	34.3	43	19.2	21	6.9	14	5.1	15	10.0	166	14.5
Hysterectomy ...	24	48.0	90	65.7	181	80.8	286	93.1	262	94.9	135	90.0	978	85.5
Total ...	50	100.0	137	100.0	224	100.0	307	100.0	276	100.0	150	100.0	1144	100.0

pregnancy they are specially liable to undergo that form of necrobiosis known as "red degeneration"; and that the onset of this process is frequently marked by pain so acute and severe as to simulate the torsion of the pedicle of an ovarian tumour. In one of my cases operation was undertaken under the impression that the patient had an ovarian cyst with a twisted pedicle. In the other cases myomectomy was the intended operation.

In all, I have had thirteen cases of myomectomy during pregnancy, the details of which are of sufficient interest to be recorded in a tabular statement (*see* Table III). The period of gestation ranged from a few days to six months. In the case of "a few days," the fact of pregnancy was not known at the time; the tumour was a large subperitoneal tumour, and the first intimation I had of the pregnancy was two months after the operation, when the patient presented the signs and symptoms of a two months' pregnancy, and stated that coitus had taken place two or three days before the operation, and not since. In Case 11, the patient, aged 36, had been married ten years, and

this was her first pregnancy. The uterus was a mass of fibroids, and in other circumstances I should have had no hesitation in doing hysterectomy. But a child had been long desired, and this was obviously her only chance. I removed ten fibroids, several being the size of a fist, and one partly necrotic. Some were pedunculated, but several had to be enucleated from the substance of the uterine wall. She had a normal confinement at term. The results of the last two I have been unable to trace; in the remaining eleven pregnancy was not interfered with, and all went to full time, except one whose labour came on at seven and a half months, but the child was alive and survived. In Case 8, the mother developed eclampsia and died, and the child was stillborn. Two other children died a few days after birth from injuries during labour, one being in a case of contracted pelvis. Both mothers had the satisfaction of a living child subsequently; the one with the contracted pelvis being delivered by Cæsarean section.

It is evident that myomectomy during pregnancy is a most satisfactory operation.

TABLE III.—RESULTS OF MYOMECTOMY DURING PREGNANCY.

No.	Age	Date of operation	Period of gestation	Result
1	35	January 11, 1909	4 months	Normal confinement on June 8, 1909; mother and child alive
2	39	April 15, 1912	4½ months	Normal confinement, August 25, 1912; mother and child alive; this was the first pregnancy
3	34	March 17, 1913	3½ months	Normal confinement, September 8, 1913; mother and child alive
4	29	October 14, 1914	4½ months	Confinement at 7½ months, January, 1915; mother and child alive
5	34	October 7, 1915	3 weeks	Confinement, June 21, 1916; baby injured at birth and died three days later; confinement of a healthy child in May, 1920
6	35	October 11, 1915	2 months	Confinement, May 10, 1916; contracted pelvis, four days' labour; child died shortly after; became pregnant again, delivered successfully by Cæsarean section, January 16, 1919
7	37	April 17, 1916	6 months	Normal confinement, August 27, 1916; mother and child alive
8	37	October 9, 1916	3 months	Confinement at full time; mother had eclampsia and died; child stillborn
9	40	December 30, 1918	3 months	Confinement, June 29, 1919; instrumental delivery; mother and child alive
10	36	November 25, 1919	a few days	Normal confinement, August 12, 1920; mother and child alive
11	36	February 5, 1920	4 months	Normal confinement, July 5, 1920; mother and child alive
12	38	March 4, 1920	3 months	Unknown
13	43	April 12, 1920	6 weeks	Unknown

The Scope and Technique of Myomectomy.

By VICTOR BONNEY, M.S.

(ABSTRACT.)

[This paper is published in full in the *Lancet*, October 7, 1922, pp. 745-748.]

THE author said that myomectomy in principle fulfilled a higher surgical ideal than hysteromyomectomy. He recorded a series of 100 consecutive myomectomies to show that within wide limits neither the number or position of the tumours in the uterus, nor the presence of degeneration, nor the accompaniment of menorrhagia or pregnancy, were a bar to successful performance of the operation. The largest number of fibroids that he had removed from the uterus in a single case was thirty, but he did not regard this as the limit of possibility. Anterior tumours were more favourably placed for enucleation than posterior tumours, because they could all be got out through an anterior incision, but many posterior tumours could also be removed by the same route. Malignancy and sepsis, of course, barred the operation, whilst nævoid degeneration made the operation more formidable, but with these exceptions degeneration of the tumour or tumours had no bearing. In regard to the technique of the operation, Alexander of Liverpool laid down the right principle twenty-four years ago, i.e., an anterior incision in the uterus, and, if possible, all the tumours removed through that one incision.

The author then proceeded to show by diagrams the manner in which the enucleation cavities could be closed by suture. When fibroids were complicated by pregnancy, and an operation was required, myomectomy should be combined with Cæsarean section if the child was viable. If the child was not viable the ideal procedure was to remove the tumours and leave the pregnancy *in situ*; where many tumours were present, however, this was impossible, and the pregnancy should be removed through the same incision as the fibroids.

He had had two deaths in his series of 100 cases, all the other patients had done well, and none of them had returned to him with new fibroids, or a recurrence of the bleeding. Five patients had become pregnant subsequent to the operation; of these one had fourteen, another eight, and another five fibroids removed, and two had one fibroid each removed. Four of these patients had delivered themselves naturally. The remaining patient was delivered by Cæsarean section, but the condition of the uterus at this second operation showed this method of delivery to have been unnecessary. He hoped that in the future it would be common knowledge that fibroids did not as a rule necessitate hysterectomy, and that if the possessors of these tumours would only submit themselves to early surgical treatment their wombs need never be removed.

DISCUSSION.

Dr. T. W. EDEN (President) said that he was impressed by the excellence of the results Dr. Giles had been able to report, and he thought that no better series of cases had been placed on record. Apart from the question of mortality, it was very encouraging that in only 10 per cent. of his cases which were followed up had there been any reappearance of fibroids, and in only 20 per cent. recurrence of the menorrhagia which was present before the operation. His results in pregnancy were also remarkably good, for not a single instance of miscarriage had occurred in his thirteen

cases, although it was generally agreed that myomectomy in pregnancy carried a 20 per cent. to 25 per cent. risk of miscarriage. He agreed with the ideals expressed by Mr. Bonney. He had always thought and always taught that the removal of the uterus was surgically a very crude procedure to adopt in order to deal with a benign growth. The surgically ideal procedure would be to remove the neoplasm and conserve the organ in which it grew. His own practice had been to adopt myomectomy much more freely than Mr. Bonney seemed to think was the case with most gynæcologists, and he welcomed the efforts which were being made by the authors to call attention to its advantages. He could not but admire the almost uncanny ingenuity displayed by Mr. Bonney in removing fibroids of the posterior wall through an incision in the anterior wall. At the same time he thought Mr. Bonney's dread of an incision in the posterior wall was quite groundless. It did not matter where the incisions were, nor how many there were (within reason), so long as each one was properly closed with complete hæmostasis. There was no risk of bowel adhesions if the wound did not ooze and the suture material was not infected. He did not employ mattress sutures himself, but preferred to close the cavity left by enucleation of the fibroid in layers, beginning from below and ending with the peritoneo-muscular wall of the uterus. He had recently dealt with a large fibroid in pregnancy in this way, in which the decidua had been exposed at the base of the enucleation cavity, but no ill effects followed, and the patient carried her baby to term. He thought Mr. Bonney had been unfortunate in having been obliged to evacuate the uterus in all but one of the cases in which he had performed myomectomy in pregnancy. He felt strongly that this should never be done in early pregnancy from choice, but only if technical difficulties arose during the operation which rendered it unavoidable. The reason for performing myomectomy in the early months of pregnancy was to relieve the patient of her symptoms in order to allow pregnancy to continue to term.

Dr. HERBERT SPENCER thought the authors of the papers on enucleation of myoma had performed a useful service in calling attention to the value of the operation, which he supposed every gynæcologist practised, and preferred, in suitable cases, to hysterectomy. He was surprised, however, to find no reference in either paper to the most valuable form of enucleation, viz., through the cervical canal. The papers well illustrated the pitfalls of statistics, Dr. Giles (after careful inquiry in a proportion of his cases) giving the percentage of recurrence as 10, Mr. Bonney (without any adequate inquiry) giving it as 0. No doubt the percentage was somewhat greater than that found by Dr. Giles, and it constituted a serious objection to the operation, especially when carried out by the abdominal route and with the added risk of ventro-fixation. Mr. Bonney's method of enucleation was ingenious, but complicated and, in Dr. Spencer's opinion, unnecessary. One danger—the possible infected condition of the tumours had not been alluded to. He had seen in a virgin a case of fibroids containing abscesses infected with streptococci, successfully treated by total hysterectomy. The case, had enucleation been performed, would almost certainly have terminated fatally, as had happened in a case in which a conservative Cæsarean section was done for an obstructing fibroid infected with bacillus of gas gangrene. Although infection of fibroids which did not give rise to symptoms was rare, he thought it was important, particularly in cases complicating pregnancy, that a bacteriological examination of the tumour should be made before or at the time of the myomectomy. He agreed with the President's criticism of the frequent emptying of the pregnant uterus in performing myomectomy.

Professor BRIGGS had long ago recognized the increasing sphere of enucleation in the progressively earlier treatment of uterine fibroids: he appreciated the illustrative modern reports within the papers just read. Allusion had been made to Dr. Alexander's work in Liverpool before 1898. At that time he (Professor Briggs) had convinced himself by many laboratory tests of enucleation that the older fibroid growths, degenerated and adherent to their capsules, were physically unsuitable for enucleation. In his opinion enucleation was at its best when the scalpel could be thrust directly into the substance of the fibroid, cutting it into halves or less, reducing its bulk and providing for the insertion of an appropriately strong sharp hook for its extraction with

the minimum surgical interference with its capsular connexions. Fantastic uterine tunnelling was unnecessary. It had to be borne in mind that after the most thorough uterine palpation a fibroid growth might remain. He had only once in the enucleation of a fibroid, acutely degenerated and adherent to the bladder (acute abdomen), had to empty the pregnant uterus owing to the depth of the fibroid involving the placental tissue of a three months male fœtus. There could be no doubt that enucleation had asserted its rightful place: but the fact of its dangerous and inconvenient abuse had to be admitted.

Dr. LAPTORN SMITH said that he had done many myomectomies but they were the exception. He agreed with the President in his statement that myomectomies were the advanced stage of the operation for the removal of fibroid. Without the great experience and technical skill which Dr. Giles and Mr. Bonney had acquired through hundreds of operations of total hysterectomy they could never have obtained the results which they had reported for their myomectomies. He had been surprised, however, to hear that all Mr. Bonney's cases of myomectomy in pregnant women had involved the death of the child as in his own cases of myomectomy not one child was lost. First, because he had only done myomectomy during pregnancy when the symptoms were urgent; and secondly, because he had made liberal use of anodynes to keep the uterus quiet for several days after the operation. He had always managed to shell out the tumours without entering the uterine cavity. The presence of a fibroid in the uterus of a pregnant woman which was not causing suffering rarely called for interference until the child was viable. In his younger days the opinion was general in the profession that the presence of a fibroid, especially in the lower part of the uterus, meant a disastrous delivery. He remembered a whole morning at the International Congress at Washington which was spent in discussing whether to bring on a miscarriage or to remove the fibroid by hysterectomy as soon as discovered. It was now known that nature not only got the fibroid out of the way of the coming child, but that sometimes the tumour disappeared or at least became much smaller after the confinement.

Mr. L. C. RIVETT stated that he had assisted Mr. Bonney at a number of these operations, and that the actual hole in the uterus was not as complicated as appeared in the diagrams shown.

Section of Obstetrics and Gynæcology :
JOINT MEETING WITH THE
Section of Therapeutics and Pharmacology.

Chairman—Dr. T. W. EDEN (President of the Section of Obstetrics
and Gynæcology).

**The Value of Ergot in Obstetrical and Gynæcological Practice ;
with Special Reference to its Present Position in the
British Pharmacopœia.**

By H. H. DALE, C.B.E., M.D., F.R.S.

THE subject proposed for discussion is : "The Value of Ergot in Obstetrical and Gynæcological Practice, with Special Reference to its present position in the British Pharmacopœia." I think it is essential for a proper appreciation of the problem that we should have before us a clear conception of the pharmacology of the drug, and of the available information concerning the chemical properties of its active principles. It is, obviously, my principal function, in a discussion of this kind, to deal with this aspect of the matter.

The position of ergot in pharmacology and therapeutics is somewhat anomalous. Like many other drugs it owed its introduction into therapeutics to purely empirical observation of its effects. While no definite knowledge existed as to the nature of its active principles, the methods adopted for preparing the extracts, which have passed into official currency, were similarly settled largely by tradition and experience at best vaguely recorded. I cannot take time to-night to deal with the fascinating history of the subject. It is sufficient for our purpose to note that about the middle of the last century two types of preparation came into vogue. In one of these the extraction was begun with water, and to the watery extract a certain proportion of alcohol was added, which carried down what were regarded as impurities ; the other preparation was made by a preliminary extraction with alcohol—the alcohol being subsequently diluted with water, and the insoluble residue thereby precipitated being again discarded. These processes appear to have provided the basis for the two commonly used preparations in the British Pharmacopœia—the so-called *extractum ergotæ liquidum* and *extractum ergotæ* (or ergotine). I shall have something to say of these preparations later.

The first substance obtained from ergot in a pure condition, which could be regarded as having any probable relation to its pharmacological or therapeutic activity, was the "ergotinine" of Tanret, which was isolated and described as long ago as 1876. This beautifully crystalline alkaloid was associated with

a quantity of material appearing to Tanret to have an identical chemical composition, but differing from the crystalline ergotinine in resisting crystallization and in its low specific rotary power. Tanret regarded it as simply a physical modification, and described it as "amorphous ergotinine." A mixture of these crystalline and amorphous alkaloids passed into use under the name "ergotinine," and acquired some therapeutic reputation, particularly in France. Presumably the so-called "ergotinine citrate" is still obtainable and still prescribed. I think it is clear, however, that Tanret's conclusion that the two alkaloids were identical delayed for some years a clear understanding of the position. Subsequent workers, wishing to observe the pharmacological action of ergotinine, naturally chose the crystalline form, which could be obtained in a high state of purity, and found that it was practically inert. The position became much clearer when my former colleagues, Barger and Carr, succeeded in purifying the amorphous alkaloid by preparing crystalline salts from it, and were thus able to show that it was chemically different from the crystalline ergotinine closely related to it. My own pharmacological contribution to the joint investigation showed that the amorphous alkaloid yielding crystalline salts, now renamed "ergotoxine," was an intensely active substance possessing the already known toxic actions of ergot—producing gangrene and so forth—and being also the substance responsible for a new and specific action of ergot, which I had shortly before identified. So far as experiments in a laboratory enabled one to judge, ergotoxine might also have been regarded as the principle responsible for the therapeutic action of ergot, since it had a powerful stimulant effect on plain muscle—conspicuously of the arteries and of the uterus.

There were several difficulties, however, in the way of the conclusion that ergotoxine was the sole active therapeutic principle of ergot. I do not think that it can be said that it received thorough and systematic clinical trial, but such trials as were made at the time of its discovery did not arouse any enthusiasm for its therapeutic properties. Again, the majority of practitioners were using, in their obstetrical and gynecological practice, the preparations of the British Pharmacopœia. The methods indicated for preparing these were such as might almost lead to the suggestion that the aim of those devising them was to exclude ergotoxine, so far as possible, from the official preparations. No such aim, of course, existed. The methods were simply those long hallowed by tradition. But, whatever the origin of these extracts, the fact that they were so widely used, with apparent satisfaction, made it very difficult to suppose that the only important principle of ergot was this alkaloid, of which they contained, at best, very small traces. The argument, however, was not a very strong one. A preparation, such as the fluid extract of the United States Pharmacopœia—an extract made with acidulated alcohol—which contained a large proportion of the ergotoxine present in the ergot, was standardized by certain producers by its power of producing gangrene of the cock's comb—an ergotoxine effect—and was, apparently, used with as much satisfaction in America as the liquid extract of ergot of the British Pharmacopœia was used in England. There were other difficulties, however, created by the claims of other pharmacologists to have discovered methods of measuring the therapeutic value of an ergot extract by laboratory tests. My friend, Professor Dixon, for example, stated that the therapeutic value of a liquid extract of ergot could be measured by its power of raising the arterial blood pressure of the cat; while, somewhat later, Professor Kehrle made the rather plausible claim that the obvious way to measure the therapeutic activity of an

ergot preparation was by its stimulant action on isolated uterine muscle. Both these effects, indeed, are effects which ergotoxine produces, but the preparations in which they were being measured by these authorities owed their activities, in these two directions, not to ergotoxine at all—of which they contained hardly any—but to entirely different substances, which my colleague Barger and I were able to identify as the bases tyramine and histamine, which can be produced from the amino-acids tyrosin and histidin by the action of bacteria, which split off carbon dioxide.

An altogether new position was thus created. It was, of course, not difficult to suppose that bases such as these, which are produced by many bacteria, could also be produced by a fungus like ergot. So far as the evidence went, however, it was against the supposition that they were present in the drug itself before it was subjected to the process of extraction. The recent very careful investigations of Stoll, carried out with very fresh and carefully preserved specimens of ergot, confirm the view that these putrefactive bases are not present in any significant amount in good samples of ergot itself. This conclusion, however, by no means excludes the possibility that ergot preparations, such as those of our Pharmacopœia, might possess useful activity, due *not* to the truly specific ergot alkaloids, but to bases such as these, casually produced by the putrefaction, for which the officially prescribed process of preparation provides abundant opportunity. At the same time, there was difficulty in the way of supposing that bases like these, which are undoubtedly present in many articles of diet, and which, further, are certainly produced by bacterial action in the intestine, could have any great therapeutic importance when administered by the mouth in relatively small quantities.

During the last two years a further complication appeared to be introduced into this already tangled problem. Dr. Stoll, already mentioned, a Swiss chemist working on behalf of a Swiss firm, isolated from ergot what appeared, and still appears, to be a new alkaloid. Its properties are in many ways closely similar to those of ergotoxine, but its formula, according to present information, is slightly different, and it has the distinction of being susceptible of crystallization as a free base. Experiments were made on its pharmacological properties, by Professor Spiro of Basel; and a number of trials of its therapeutic properties have already been published in the Swiss and German literature. These, carried out far more systematically than any trials which ergotoxine has ever received, seem to have satisfied the clinical workers concerned that this alkaloid, named "ergotamine" by Stoll, has all the therapeutic effects of good ergot, and produces these effects with more certainty and regularity. On the other hand, Professor Spiro's preliminary investigation did not make at all clear the relation of the pharmacological action of ergotamine to that which I had, some fifteen years ago, described for ergotoxine. I suggested to him that we should exchange preparations, and jointly make the comparison, with the result that we were both perfectly satisfied that, whatever might be their chemical relationship, these two alkaloids were absolutely identical, both qualitatively and quantitatively, in their pharmacological effects.

So far as the subject of our discussion is concerned, we may, therefore, conveniently speak of these two known active alkaloids of ergot—ergotoxine and ergotamine—as "the specific ergot alkaloids"; and the problem reduces itself to this: Do the specific alkaloids of ergot give, in obstetrical or gynæcological practice, or in both, the effects which the clinical worker wishes to produce when he administers ergot? If so, ergot must be included in the

Pharmacopœia, because these specific alkaloids can be obtained from no other source. But, if ergot is to be retained in the Pharmacopœia on account of its specific alkaloids, the methods of preparing the official extracts ought to be so revised as to ensure that these alkaloids are present in the finished product, and not thrown away, as at present. If, on the other hand, it is the fact that, with the existing official extracts, the non-specific putrefactive bases which they contain possess all the remedial actions, in obstetrical or gynecological treatment, which ergot is supposed to produce, then it is equally clear that ergot has no proper place in the Pharmacopœia. These bases are much more easily and cheaply obtainable from other sources, and by other methods, and to provide them, for therapeutic use, by casual and unregulated putrefaction of an obscure and expensive fungus, is not a scientific or even a sensible procedure.

I do not suppose that a discussion such as the present is likely to produce all the necessary data for the final settlement of a question such as this, but I may be allowed, in conclusion, to indicate one or two directions in which the discussion might throw light on the problem. In the first place, it will possibly clarify the issue if I indicate the relation of these known active principles to certain non-official and proprietary preparations.

Ergotinine citrate is a preparation owing its activity entirely to the specific alkaloids. Ergotoxine and its salts, when they were obtainable, were also representatives of the specific alkaloids. The firm for whom Stoll carried out his recent investigations are issuing a solution of ergotamine salts with the name "gynergen" on the European continent, and "femergin" in this country. The action of these solutions again is, of course, purely that of the specific ergot alkaloids. Some years ago, Messrs. Burroughs Wellcome and Company issued, in this country, a preparation to which they gave the name "ernutin." This preparation was an attempt to avoid the difficulty of deciding between the specific alkaloid ergotoxine and one or other of the putrefactive bases, as the essential constituent of ergot, by including them all. During the war, and since then, the practical disappearance from the British market of the ergot from Russia, which was previously the source of ergotoxine, has, apparently, led to a revision of this policy, and a preparation has, if I am correctly informed, been produced in which the non-specific bases are retained, but the unobtainable ergotoxine has been omitted. There may be those present who have had the opportunity of testing both mixtures, and who may be able to say whether the mixture containing the non-specific bases only has any of the activity expected of ergot, and, if so, whether it has as much of that action as the preparation also containing the specific alkaloid—ergotoxine.

There is one other point which suggests itself to my ignorance as possibly worth consideration. Ergot used to be employed very largely in obstetrical work. I imagine that, in this direction, its use has largely been displaced by that of the pituitary extract. In so far as it is still used, it appears to me that there is just a possibility that the type of ergot action, and, therefore, the type of ergot preparation, which is desirable in obstetrical practice, may be different from those which are required in gynecological work, in which, I gather, ergot nowadays finds its principal application. I venture to suggest that the possibility of this distinction may with advantage be kept in view in our discussion.

It is, as I said, almost too much to hope that a discussion like this can finally settle a question of this kind. It will not be fruitless if it makes clear

the nature of the question which requires settlement. I feel very strongly that the present position of ergot and its official extracts, in our Pharmacopœia, is something of a reproach to a scientific profession. Possibly, from this discussion, some suggestion may emerge of an organized investigation by which the reproach may be removed.

DISCUSSION.

Sir NESTOR TIRARD said that since the value of ergot was to be discussed with special reference to its present position in the British Pharmacopœia, he felt it his duty, as one of the editors, to make a few remarks upon that subject. In particular he wished to refer to the statement about the methods of preparing the extracts being "those long hallowed by tradition." This appeared to suggest that very little trouble had been taken in the compilation. It might be well to say that before revising the "list of medicines and compounds and the manner of preparing them" (Medical Act of 1858) those responsible took many steps in their endeavour to ascertain the wishes of the medical profession. All the authorities—that is the examining or licensing bodies—were asked to make suggestions for additions, omissions or alterations. Replies were received from nineteen authorities and were tabulated. The detailed views of the Therapeutic Committee of the British Medical Association, the representations of a committee of wholesale firms, and an analysis of some forty-eight thousand recent prescriptions also supplied material of great value. It was found that in no single instance was there any desire to omit ergot or its preparations. One authority suggested the introduction of a stable active principle of ergot, another considered that the preparation of extractum ergotæ might be improved, and a third wished the liquid extract to be standardized physiologically. The last was an attractive proposal for preparations of several other drugs besides ergot. In the absence of a State laboratory to establish a standard there were obvious difficulties at the time the current Pharmacopœia was undergoing revision. He (Sir Nestor Tirard) expressed full appreciation of the valuable work of Dr. Dale upon ergot. He noticed, however, that Dr. Dale, in a recent report upon pituitary extracts,¹ had shown that claims of physiological standardization already made on behalf of some five different commercial samples included wide divergencies of activity. An authoritative standard and a State laboratory were essential before it would be possible to introduce pituitary extract into the British Pharmacopœia, even though pharmacologists and obstetricians might be agreed that it should replace ergot.

Dr. HERBERT SPENCER thought Dr. Dale's paper was of great value, not only in its pharmacological aspect, but also in emphasizing the importance of the collaboration of laboratory and clinical workers. He had had evidence of this at University College Hospital, where, in the laboratory of the medical unit, the discovery of the weakness of a supply of pituitrin had led to its abandonment and the substitution of an efficient brand. He said that Dr. Kellaway had also investigated a new preparation of ergot which showed a very powerful effect; but the uterus did not react to repeated doses. He (Dr. Spencer) had therefore decided not to use it clinically. There was no doubt of the power of liquid extract of ergot to cause the uterus to contract; but for gynæcological bleeding he found the ammoniated tincture much more efficient and had used it exclusively for thirty years. He had therefore been interested to hear Dr. Dale's opinion as to its pharmacological superiority.

Professor W. E. DIXON, F.R.S., regarded as one of the most important advances of the last few years the discovery that the substances that worked the human body were not far removed from the crystalline alkaloids that could be isolated. It appeared likely that the normal stimulus to the uterus was the secretion of the pituitary gland into the cerebro-spinal fluid. An interesting point was that the injection of ovarian extract alone among organic extracts led to the appearance of pituitary secretion in the cerebro-spinal fluid. Corpus luteum did not cause this phenomenon, but the substance

¹ See *Lancet*, November 25, 1922, p. 1134.

of the ovary itself did so even after it had been boiled. It would seem to follow that uterine contraction was a physiological process brought about by pituitary extract. It was therefore interesting that obstetricians apparently preferred pituitary extract to any other drug for the purpose of stimulating uterine muscle. In Germany in 1915 a number of experiments had been performed in which the movements of the human uterus were recorded. A comparison of the effect of ergot with that of pituitary extract showed that after the administration of the latter the pains became quicker, but no difference in tone was detectable, whereas after ergot the pains increased and the tone also increased. With regard to the British Pharmacopœia, pharmacologists were clear that the American liquid extract was far superior to the British liquid extract. Sir Nestor Tirard had intimated that the editors of the British Pharmacopœia were out to produce standards of those drugs which were commonly used—i.e., that they were prepared to follow in the wake of, but did not attempt to lead the profession. How, then, should we advance? Practitioners tended to follow the British Pharmacopœia as an authority guiding them as to which preparations of drugs they should use. In this way a circle was established, from which there appeared to be no escape.

Dr. T. W. EDEN (Chairman) said that they greatly appreciated Dr. Dale for his very clear and interesting presentation of the subject. Although Dr. Dale had spoken with modesty of his own share in the work which had been done, there was no doubt that he had done a great deal to clear up the confusion which had existed with regard to the active principles of ergot. He (the Chairman) thought that although they now had, in pituitary extract, a reliable substitute for ergot, it would be very unfortunate if ergot disappeared from the Pharmacopœia because it was a much more suitable drug than pituitary extract to place in the hands of midwives. Pituitary extract occasionally produced rather alarming toxic effects, and its potency called for great care when using it, whereas ergot was free from any risk of producing unfavourable general effects in obstetric practice. The reluctance of clinical workers to discuss the subject was perhaps explained by the deep distrust which they all felt for clinical impressions of the action of drugs. There were so many possible sources of error that very great care was called for in attributing clinical phenomena to drugs which had been administered. In the case of ergot he had always felt that it ought to be possible to devise a method of graphing the uterine contraction in labour by means of a bag placed in the cervix, and if the pharmacologist could help them to work out such a method, the action of both ergot and pituitary could be subjected to an accurate clinical test.

Mr. ALECK BOURNE said that he was now engaged in an attempt to measure accurately and graphically the changes of intra-uterine pressure induced by the injection of pituitary extract, on behalf of the Committee appointed by the Council of the Section of Obstetrics and Gynæcology for the Investigation of Pituitary Extract in Labour. The apparatus which was made and supervised by Dr. H. H. Dale and Dr. Burn had so far worked very well, and it was hoped that the report of the Committee, when published, would refer to results of real value obtained by this direct method. He further mentioned this work on pituitary extract to suggest a method by which some scientifically accurate evidence of the value of ergot and its derivatives on the human uterus might be obtained, as he had very little confidence in the scientific value of the impressions gained by the clinical administration of the drug. He feared the effects of bias in favour of, or against, the use of a particular drug were too strong—perhaps unconsciously so—to enable the observer to arrive at a scientifically accurate opinion from clinical evidence alone.

Professor HENRY BRIGGS appreciated Dr. Dale's interesting summary of the past of ergot, also his laboratory search for the best in ergot. The drug had attained a varied reputation, and established clinical opinions against its efficacy were readily recalled. Wider co-operation in laboratory and bedside work was rightly advocated by Dr. Dale. Professor Briggs stated that during the past five years he himself had often officially referred to the singular practice in medical schools of issuing daily from the theatre long lists of common and rare operations and of not issuing similar ward notices of the medical cases with an indication of the main remedy or drug in use in

Sections of Obstetrics and Gynæcology and Therapeutics 7

each. In any medical school, the issue of such medical ward notices was little more than a bare duty to the laboratory of pharmacology; the notices would be welcomed as guides to students and visitors.

Dr. DALE (in reply) said that the position Sir Nestor Tirard had taken up as to the functions of the British Pharmacopœia, was quite intelligible but rather hopeless. The function of the British Pharmacopœia was not to lead but to follow the profession. There would never be progress on those lines, for the profession was apt to regard the British Pharmacopœia as an authority not only as to what was, but as to what should be, used. In the 1914 British Pharmacopœia the extract of ergot had been changed from an alcoholic to a watery extract.

Sir NESTOR TIRARD at this point reminded Dr. Dale that the British Pharmacopœia was controlled by an international agreement whereby it was compelled to make all liquid extracts watery extracts, alcohol being added subsequently. In America alone, in spite of this agreement, had the alcoholic extract been retained.

Dr. DALE said he wished that the American disobedience had prevailed in this country also, because by other methods the specific active substances in ergot were thrown away and the adventitious substances—he might almost say impurities—were retained. The Chairman (Dr. Eden) had spoken of the advantages of extracting all the active principles of ergot. This would limit the preparation to one containing the alkaloids, since the bases practically did not exist in fresh, clean ergot. Mr. Bourne had been pessimistic as to the help available from clinicians, and Dr. Dale agreed that a Committee reporting to the Section of Obstetrics and Gynæcology would probably be the best method of eliciting any information of value. Presumably the findings of such a committee of obstetricians would carry some weight with the editors of the British Pharmacopœia.

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Section of Obstetrics and Gynæcology.

President—Dr. T. W. EDEN.

A Specimen of Primary Carcinoma of the Vagina.

Shown by EARDLEY HOLLAND, F.R.C.S.

THE patient was aged 46, had four children, and still menstruated regularly; for five months she had had an offensive, watery, blood-stained vaginal discharge; for the last two months there had been a good deal of hæmorrhage, but no pain. On examination a hard, circular, raised, rough, friable growth was found on the upper third of the posterior vaginal wall. The upper edge of the growth was separated from the cervix by half an inch of healthy vaginal mucous membrane. The cervix looked and felt normal, the uterus was normal in size and position, and there was no palpable evidence of other pelvic disease. Rectal examination did not reveal any infiltration of the anterior rectal wall, which could be moved freely over the posterior surface of the growth.

The operation was performed under stovaine spinal anæsthesia and open ether on October 20, 1922, the whole of the vagina and uterus, together with the pelvic cellular tissue and iliac lymphatic glands being removed. The patient was first placed in the lithotomy position and an incision was made all round the lower end of the vagina, which was dissected up from its attachments, partly by blunt dissection and partly with scissors; no difficulty was experienced with this dissection, and there was remarkably little bleeding. There was no adhesion between the vaginal wall at the site of the growth and the rectum, and there was no evidence whatever of infiltration of the anterior rectal wall. After the vagina had been separated as high up as the cervix, its lower end was inverted and sewn up. The vulva was then packed with gauze, the patient was placed in the Trendelenburg position, and the usual stages of Wertheim's hysterectomy were proceeded with. The only difficulty was encountered when the upper end of the vagina was being separated from the bladder. At this stage it was apparent that the plane of separation found was different from that found during the separation of the vagina from below; it was evident that, whereas from below the separation was mesial to the vesico-vaginal fascia, in the abdominal part of the operation the plane of separation was outside this fascia. The correct plane was soon found, and there was no difficulty in completing the operation and removing the uterus, together with the whole of the already separated vagina, through the abdominal wound. After the peritoneum had been sewn over the pelvic floor in the usual way, the abdomen was closed and the patient again placed in the lithotomy position. Upon removal of the pack from the cavity left by the excision of the vagina,

very free oozing was found to be going on from the upper end of this cavity; this had been caused, no doubt, by the separation of the upper end of the vagina through a plane different from that used in the vaginal part of the operation. As control of this oozing with forceps and ligatures proved troublesome and lengthy, the cavity was packed with gauze soaked in flavine solution, and the patient was put to bed in very fair condition. Convalescence ran a smooth course, and was complicated only by suppuration in the lower part of the abdominal wound. Histologically the growth proved to be solid, trabecular, squamous and horny-celled carcinoma of the vagina.

Owing to the close proximity of the posterior vaginal wall to the rectum, and to the very small amount of intervening cellular tissue, it seems almost certain that in these cases permeation of the carcinoma cells into the anterior rectal wall must take place at a very early stage of the growth. The question therefore arises as to whether it is not also advisable to remove the lower part of the rectum, although the operation would then become an extremely severe one.

I should like the opinion of the Section on this point.

Specimen of Squamous Epithelioma of the Vagina.

Shown by THOMAS G. STEVENS, F.R.C.S.

THE patient, Mrs. M., aged 53, had noticed a coloured discharge for two months previously to June 1, 1922, when she was first seen. The periods had almost ceased, only a small loss on one day each month for three months having occurred. There was pain in the pelvis, there was also great sense of weight and pressure, with pains down the legs. For some years she had been wearing a large Hodge pessary for prolapse, but this had been discontinued for a year or more.

On examination a large flat growth was found on the posterior vaginal wall, which at first was thought to be an extension from a cervical growth, but this was found at the subsequent operation not to be the case.

The uterus and upper half of the vagina with the whole growth were removed by abdominal pan-hysterectomy, after isolation of the ureters. The operation was not particularly difficult, the growth apparently not having involved the rectal wall in any way. No secondary glandular deposits were found. The patient made an uninterrupted recovery and eight weeks after the operation received a large dose of X-rays with the object of preventing a recurrence. Up to the present the patient remains quite well.

The growth which, roughly speaking, is the size of a five-shilling piece, occupies the posterior vaginal wall, and is separated from the cervix by about half an inch of unaffected vaginal tissue. The external os uteri shows some redness and roughening of the surface, the result of infection and inflammation, but does not present any evidence of malignant growth. The body of the uterus contains one fibroid.

The growth proves to be a squamous epithelioma histologically.

DISCUSSION.

Dr. H. RUSSELL ANDREWS said that he was interested in the remarks of Mr. Holland and Mr. Stevens as to the best method of removing the vagina in such cases. He said that in March, 1909, he showed a specimen of primary carcinoma of the vagina before this Section.¹ The patient, a multipara, aged 62, had complained for six months of a blood-stained watery discharge and loss of flesh. There was a carcinomatous ulcer, the size of a two-shilling piece, high up on the posterior vaginal wall. The cervix was not involved. He removed the whole vagina and uterus from below. As soon as the lower part of the vagina had been freed, a large curved clamp was put on, which converted the vagina into a closed bag; this was done with the idea of preventing any scattering of carcinoma cells. The operation was performed in September, 1907, over fifteen years ago, and there had been no recurrence. The patient, who was now 77, had been seen by Dr. Andrews on January 8, 1923, and found to be in excellent health. Dr. Andrews considered that if the rectum was involved the case was too far advanced for operation. He did not think that routine removal of the rectum was justifiable in cases of carcinoma of the vagina.

Mr. CLIFFORD WHITE said he thought that the question raised by Mr. Holland of excising the rectum as well as the uterus and vagina in advanced cases was important. He had done this operation on one occasion by an extension of the ordinary abdomino-perineal method for excision of the rectum. The operation itself was not especially difficult, but he had experienced the greatest difficulty in covering in the large cavity left, owing to the deficiency in peritoneal flaps. In spite of the presence of a large plug inserted from below, a loop of small gut prolapsed, and the patient died with symptoms of intestinal obstruction on the fifth or sixth day.

Adenoma of the Vaginal Fornix simulating Cancer of the Cervix.

By HERBERT R. SPENCER, M.D.

E. F., AGED 54, admitted to University College Hospital on June 30, 1920, had had two children and one miscarriage, the last pregnancy having occurred twenty-seven years ago. She had been a widow for twenty-five years. She had suffered from intermittent hæmorrhages from the vagina since September, 1919. The blood was very dark and clotted, and was followed by a slightly coloured discharge. Sometimes the patient had gone for a month without any discharge at all. Pain was absent except when the clots were being passed. The patient had been getting thinner during the last month. There was no history of cancer or tumour in the family. Menstruation began at the age of 15, had always been irregular (at intervals of four to six weeks), lasted three to four days and required five to six diapers. The menopause occurred six years ago (at the age of 48) and there had been no bleeding afterwards until nine months ago. There was no trouble with micturition, beyond slight frequency in the daytime; the urine was normal, except for a deposit of phosphates; the bowels were confined; there was no history of the performance of any vaginal operation with the exception of forceps deliveries.

The patient, a grey-haired, moderately nourished woman, with a well-marked moustache, looked unhealthy and somewhat cachectic. Nothing abnormal was to be felt in the abdomen. The perineum had been torn in one

¹ *Proceedings*, 1909, ii (Obst. and Gyn. Sect.), p. 248.

of her confinements, and stitched. There were some scars in the vagina extending from the perineum (and, after removal of the tumour, a triradiate scar was seen in the posterior fornix). On passing the finger into the vagina a brittle growth as big as a large duck's egg was found nearly filling the vagina. It bled very freely, and prevented examination of the upper vagina. The tumour was irregular on the surface and exactly resembled a proliferating carcinoma of the cervix, and I had no doubt that it was of that nature.

My purpose to remove the vaginal mass as a preliminary to an extended abdominal hysterectomy was easily effected by breaking away the growth in pieces with the fingers. Having done this, on passing a speculum, I was surprised to find that the growth, which had been completely removed by the fingers, had been attached to the fornix of the vagina to the left and front of the cervix. The cervix itself was normal, except for two minute mucous polypi, which were snipped off. The raw surface where the tumour had been attached measured $1\frac{1}{4}$ in. in length and $\frac{3}{8}$ in. at the broadest part (in front) as shown in the sketch taken at the time (fig. 1); it resembled a superficial tear in the vagina. The shallow wound was slightly infiltrated with blood; but there was no induration at its base or in its neighbourhood, and the uterus itself was



FIG. 1.—Showing area of attachment of the adenoma.

normal and freely movable. As the wound oozed slightly a plug of iodoform gauze was applied and a piece of the growth was hardened and cut; the rest of the tumour was not kept. Although it appeared to be a benign tumour, on July 3, 90 mg. of radium emanation were applied to the raw surface for twenty-five hours: on July 10 the surface was only $\frac{5}{8}$ in. in length, quite smooth and pink in colour. The patient left the hospital on July 15, and was examined on August 15, when the wound had cicatrized. I examined the patient on April 4, 1922, and found her quite well, and the vagina and uterus healthy. A letter was received from her on December 14, 1922, stating that she remained in good health, nearly two and a half years after the operation.

Microscopical Examination.—What appears to be the surface is covered with a single layer of columnar epithelium, in places thrown into papillae. The tumour consists of glands lined with a single layer of columnar epithelial cells (with large well-stained nuclei) set in a fibro-muscular stroma which forms well-defined bands and areas in some parts of the tumour, and in others is so scanty that the glands lie closely apposed. There is slight small-cell infiltration of the stroma, and in parts hæmorrhage has occurred, probably owing to the

trauma of the operation. The glands are sometimes simple tubes, in other places are nearly filled with papillary projections and have a markedly convoluted appearance (fig. 2). There is no sign of penetration of the basement membrane, and the epithelium is everywhere in a single layer and of simple aspect. Only in the neighbourhood of the hæmorrhages is the epithelium a little swollen,



FIG. 2.—Illustrating Dr. Spencer's case of adenoma of vaginal fornix.

and apparently stained by imbibition of blood. At one part of the section a long channel is seen lined by columnar epithelium, and the ducts of other glands can be seen opening into the channel (fig. 3). The growth appears to be a benign adenoma.

Remarks.—The occurrence of a large pedunculated benign adenoma of the

vaginal fornix simulating cancer of the cervix must, I think, be very rare; I have not come across the record of a similar case. It points to the value of the removal of redundant growths before resorting to the extended abdominal hysterectomy. It is remarkable that such a large growth should have had a narrow band-like pedicle, and that removal of the growth with the fingers was sufficient to effect a cure; for I do not think the radium can be credited with the result. An interesting subject for speculation is the origin of the growth.



FIG. 3.—Illustrating Dr. Spencer's case of adenoma of vaginal fornix.

The position suggests that it may have originated in an isolated portion of Gartner's duct; or perhaps it may have taken its origin in the crypts which are sometimes found in the vaginal fornix.

Dr. HERBERT SPENCER said that Mr. Bonney's case, which had been referred to by a speaker, was one of disseminated, inflammatory, sessile glandular structures, quite unlike the large pedunculated cancer-like adenoma he had shown. He agreed with the criticism that it was unlikely that it originated in Gartner's duct.

A Uterus removed for Carcinoma of the Cervix after Treatment by Radium.

By A. H. RICHARDSON, F.R.C.S.

THIS is a specimen of a uterus which was originally the site of an extensive cervical carcinoma in which, first, a laparotomy was done and the growth judged to be inoperable; secondly, two applications of radium were made at an interval of three months, followed by apparent disappearance of the growth; and thirdly, the abdomen was again opened and a radical hysterectomy was done without much difficulty.

The patient first came under observation in June, 1921, as a case of carcinoma of the cervical canal, with slight extension to the vaginal portion. She complained of continuous hæmorrhage, offensive discharge, and backache. She was nulliparous and aged 51. The menopause had taken place eighteen months previously. Symptoms had been present for nine months. She was admitted to hospital and an attempt was made to remove the uterus, but this was given up on account of the fact that in the act of separating the bladder from the cervix a portion of growth was actually opened up.

Three weeks later the first application of radium was made. A quantity of typical friable growth was scraped out of the cervical canal, so that a cavity remained, into which the terminal joint of the index finger easily passed. In this cavity two tubes of radium of 50 mg. each were placed, each screened with a silver filter of 2 mm. in thickness, and rubber tubing of 1 mm. in thickness. These were left in position for thirty-six hours.

Three weeks later the patient was seen at the out-patient department and reported that all her symptoms—hæmorrhage, discharge, and pain—were much alleviated. She did not appear again for about a month, and then said that the hæmorrhage had recommenced though it was not so severe as before. She was readmitted and another application of radium was made. It was now found that one tube of radium could only be passed with difficulty—and that, after some use of a sharp spoon—into the cavity which before had been a large hole easily excavated out of the cheesy growth. This tube, as previously, contained 50 mg. of radium, and was similarly screened with 2 mm. of silver filter and 1 mm. rubber tubing. It was left in position for thirty-six hours. A second tube was placed transversely across the vaginal vault and held in position with a gauze pack. This was removed in twenty-four hours.

The first operation took place in June, 1921; the first radium application was made in July, 1921, and the second in October, 1921.

The patient next appeared in early December, 1921, when she said that there had been no further hæmorrhage nor discharge, but that she had had a good deal of pain in the back (sacral pain). On bimanual and vaginal examination it was found impossible to say whether there had ever been any growth, so it was thought that now was the time to remove the uterus. She agreed to this and a card was sent to her just before Christmas, 1921, telling her to come into hospital. She failed to appear and wrote to the Sister saying that her husband was ill and that she had to nurse him. After this we completely lost sight of her until six months later, June, 1922, when she reappeared in the out-patient's department one day, saying that she had not been able to come before as her husband had been ill and had died very recently. The condition was exactly the same as in December, 1921. There

was no demonstrable growth. She was admitted and a radical hysterectomy was performed. Where, before, growth had been encountered in separating the bladder from the cervix, there was now an area of dense cicatricial fibrous tissue which presented the only, but rather formidable, difficulty of the operation. Once past this cicatricial area the bladder was separated from the upper part of the vagina with ease; the ureters were isolated without difficulty and were not involved in any sort of pathological tissue, fibrous or otherwise. There were no enlarged glands to be seen nor felt in the pelvis. She left the hospital five weeks later and went to a convalescent home.

Dr. Dudgeon examined the uterus and reported that, macroscopically, he could see no growth. He cut sections from four different parts of the cervix and only in one did he find a small focus of carcinoma, and that was very atypical. To put it in his own words, "the carcinoma cells are very shrunken and look as if they had been boiled."

There is a great deal of fibrosis of the whole cervix and the mucosa has largely disappeared.

Two Cases of Cancer of the Cervix treated by Radium before Operation.

By T. W. EDEN, M.D. (President), and AUBREY GOODWIN, M.D.

Case I.—A. D., a multipara, aged 53, was admitted to the Chelsea Hospital for Women on January 17, 1922, complaining of vaginal discharge and pain in the back and left groin of three months' duration. Previously to the onset of the symptoms menstruation had been regular and normal, and her general health was good. Examination showed an extensive, friable growth covering the whole cervix, and extending over the left and posterior vaginal walls for an area of about 2 cm. in diameter. The mobility of the uterus was a good deal impaired. On January 26, scraping the growth with a sharp spoon showed that considerable invasion of the cervical tissues had occurred, and into the large cavity thus formed a tube containing 200 mgm. of radium bromide was introduced, and left there for twenty-four hours. The patient was not examined again until February 21, nearly a month later: no trace of the growth could then be found on examination with the finger or by inspection through a speculum. The cervix was smooth and the external os of normal size; the vaginal walls were smooth and apparently healthy in the position previously occupied by the growth. Wertheim's hysterectomy was performed on February 23. No difficulty was experienced in freeing the ureters, and there were no enlarged glands found. Convalescence was uneventful.

PATHOLOGICAL REPORT BY DR. AUBREY GOODWIN.

Macroscopical.—A uterus, 9 by 6 by 3.5 cm., removed by total hysterectomy together with the appendages of both sides and a cuff of vagina 0.5 cm. in length. The peritoneal surface of the uterus is smooth. On the posterior wall of the vagina there is a healing granulating surface. The vaginal portion of the cervix is excavated but its surface is smooth. On section the endocervix has been found to be occupied by a fungating granular growth which spreads upwards towards the body of the uterus and downwards towards the vaginal

portion of the surface. The walls of the uterus are up to 1·8 cm. thick, the endometrium is 0·5 cm. thick, pale and villous. The tubes and ovaries are atrophic.

Microscopical.—There is an extensive spheroidal and columnar-celled adenocarcinoma of the upper part of the cervix. The vaginal portion of the cervix and the posterior wall of the vaginal cuff show a healing granulating surface with no malignant cells to be seen. The endometrium shows a type of villous endometritis but in the zone near to the internal os the cells are beginning to show definite malignant changes. The ovaries show senile fibrotic changes.

Case II.—L. S., multipara, aged 47, admitted to the Chelsea Hospital for Women on October 23, 1922, complaining of a blood-stained vaginal discharge of five months' duration. Previously to this, menstruation had been regular but profuse. The general condition was good and she had had no pain. Examination showed a large irregular friable growth of the cervix which practically filled the upper part of the vaginal canal; the growth extended upon the vaginal wall on the right side for a short distance. The body of the uterus contained a hard fibroid the size of a large apple. The mobility of the uterus was not much impaired. On October 25, after the friable growth had been scraped, 160 mg. of radium bromide were introduced in three tubes, and left for twenty-four hours. The pathological report on the scrapings was "columnar-celled papillary cancer of the cervix." The patient was examined again on November 9, i.e., fifteen days later, and no trace of growth could be found. The walls of the cervix appeared smooth and healthy. The uterus was removed by Wertheim's hysterectomy on November 9; the operation was simple and easy and there were no enlarged glands.

PATHOLOGICAL REPORT BY DR. AUBREY GOODWIN.

Macroscopical.—Uterus, 10 by 8 by 12 cm., removed by total hysterectomy together with the appendages on both sides and a cuff of vagina 3 cm. in depth. In the posterior wall of the uterus is a single large fibromyoma 8 cm. in diameter. Projecting into the cavity are two small polypoid projections of endometrium 0·8 cm. long. The cervix is hypertrophied and tough, with several hard whitish areas in its substance. The ovaries are tough and fibrous.

Microscopical.—Three sections: (1) Wall of uterus and polypus; (2) tough area in cervix; (3) cervix and vagina. Section (1): Benign glandular hyperplasia and hypertrophy of endometrium—"polypoid endometritis." Sections (2) and (3): Columnar-celled adenocarcinoma of cervix undergoing hyaline degeneration. The fibro-muscular tissue of the cervix is also showing hyaline changes. There is an extensive round-celled infiltration of the growth.

REMARKS BY DR. T. W. EDEN (PRESIDENT).

I think that in future malignant disease will be treated by a combination of wide surgical removal of the growth with free use of methods of radiation. In the case of cancer of the cervix there are certain notable advantages in the use of radium before operation. In the first place a proliferating growth can be almost entirely got rid of by a single application of a sufficiently large amount of radium: this greatly simplifies the subsequent operation, and may even render clamping of the vagina below the level of the growth unnecessary.

In the second place the risk of cancer implantation occurring during the operation is greatly reduced, for the sections show that the cancer cells which remain in the zone of irradiation are much degenerated, and are probably incapable of being grafted successfully upon fresh tissues. In the case of a widespread growth such as that found in Case I, the strictly local action of the radium is well shown, the portion of the cancer most distant from the external os being little affected. On this account as much as possible of the growth should be removed with the sharp spoon, so that a cavity in which the radium tube can be buried may be obtained and thus brought in contact with the distant parts of the growth.

DISCUSSION.

Mr. MALCOLM DONALDSON said that this subject was being investigated by Dr. Canti and himself on behalf of the Gynæcological Department at St. Bartholomew's Hospital. So far fifty-five cases had been treated and the majority had been benefited for a time. In his opinion it was much too early to discuss the ultimate value of the treatment. It was quite obvious that the carcinoma could be destroyed locally in the cervix, but the problem remained how to free the parametric tissues and glands. At present he was trying two methods: (1) Deep radiation by the operation devised by Dr. Frans Daels (of Ghent); (2) by X-ray treatment. The most urgent need at present was a scientific basis for dosage. He was very doubtful whether the biological effect was the same if the amount of radium was doubled and the duration of exposure halved. He said that a chart of the histological findings drawn up by Dr. Canti very strongly suggested that the time-factor and distribution of the radium were perhaps more important than the quantity of radium.

Mr. SIDNEY FORSDIKE said that the authors had mentioned the weight of radium and the length of time for which it was employed, but that information was of small value unless they also gave some account of the screens utilized. This was most important, for a paper, published by Wood and Prime in the *Annals of Surgery* some years ago, showed conclusively that the hard Beta-ray plus Gamma-ray was approximately eight times as potent as the Gamma-ray when used alone. There was no question of the immediate dramatic improvement in the most advanced cases of carcinoma when treated by radium, and it might be said that as a palliative it was the treatment of choice. With apparent cures one must always regard indurated tissues with suspicion, for they too frequently contained a potential neoplasin. As to the Belgian operation for treating the pelvic glands and cellular tissues by radium, it was retrogressive in principle and added another terror to the disease; opening the abdomen and implanting radium into the growth had obvious limitations, and the only method of dealing with such deposits was by means of the hard X-ray treatment. Recasens had been treating carcinoma of the cervix with radium applied locally and X-rays applied to the rest of the pelvis, and he claimed improved results upon treatment with radium alone. The question arose whether radium should not be used at an earlier stage of the disease; Burrows (Manchester) only treated advanced cases referred to him by surgeons, and reported 12 per cent. of them as remaining free of the disease for from three to four and half years, but concluded that this was the best he hoped for with the advanced cases with which he dealt. Three early operable cases had been referred to him (Mr. Forsdike) owing to the physical condition of the patients; one died within a month of the first exposure, with an acute toxæmia, and there was reason to suspect the selenium, with which she was also being treated, as the cause of it. One of the others had had three exposures and remained free from signs and symptoms one and half years afterwards. The third had four exposures of radium and subsequently hard X-rays to the pelvic tissues; she remained well eighteen months afterwards. No conclusion as to the efficacy of this treatment could be drawn from the result in three cases only, but the benefit derived from it in suitable cases was encouraging.

Mr. GORDON LUKER said that for the past two years he had been employing radium previous to operation in all cases. He had been inserting 50 mg. of radium into the

cervical canal for thirty-six hours and the application had been made from one to seven days before the operation. His idea was that by this means the dissection was made through "radiumized" tissue and that recurrence was thereby prevented. In nine cases followed up from nine months to two years there had, so far, been no local recurrence. In a few cases another application of radium had been given three months after the operation. He was not so enthusiastic about the use of radium in bad inoperable cases, but in doubtful cases he had seen the growth almost disappear, so that operation could be easily performed later. In one such case of a year ago, operation five months after the application of radium had been very easy and no recurrence had taken place.

Dr. H. WILLIAMSON said that every case of carcinoma of the cervix admitted to St. Bartholomew's Hospital during the last eighteen months had been treated by radium before operation. Some details of these cases had already been given by Mr. Donaldson¹ and although it was too early to speak of results, facts of importance had been demonstrated. First, it had been shown that in early cases in which the irradiation was distributed over the cervix by means of a number of needles in addition to a tube inserted into the cervical canal, sections taken from many parts of the cervix after Wertheim's operation failed to reveal the presence of cancer cells. Secondly, it had been shown that cases originally considered inoperable in some instances became operable after three or four irradiations. Thirdly, it had been shown that it was dangerous to treat septic growths by radium: in two cases in which the growth was in a sloughing and necrotic condition a fatal pyæmia had followed the application of radium.

Glycosuria, resulting in the Birth of a Dead Child, treated with success in a Subsequent Pregnancy.

By ROBERT WISE, M.D.

MRS. M., married, aged 29, multipara, a slightly stout and lethargic lady, gave birth to a dead child during the eighth month of the first pregnancy, having previously suffered from polyuria, thirst, weakness, and loss of flesh.

She had the following symptoms during the second pregnancy: Slight nausea and vomiting, syncopal attacks, weakness, intense general pruritus, marked polyuria, and very great thirst. She used to rise seven or eight times every night to micturate, and each time drank a large tumbler full of water. She rarely drank less than half a gallon of water daily, besides other fluids. She took no alcoholic drinks. She was often very drowsy, but had no coma.

The urine was usually pale, acid, with deposit of mucus; no urates, no albumin or blood; specific gravity 1045, rising to 1050. It had a sweet odour, and on being heated deposited burnt sugar. It quickly gave a brilliant yellow, turning into a brick-red colour on boiling with Fehling's solution, and usually contained 10 to 15 per cent. of glucose.

Prognosis was varied, and very bad during the eighth month of her pregnancy.

Treatment was by diet and drugs. At first sugar was stopped in her diet for one week, but there was no improvement in her symptoms, nor any decrease of the sugar in her urine. The withdrawal of all starchy food reduced the specific gravity of her urine 5° in two days. Her diet was altered, at intervals, to suit the condition present. Sugar, milk and potatoes were given, along with

¹ *Proceedings*, 1922, xv (Sect. Obst. and Gyn.), pp. 66-70.

large doses of soda bicarb. and nux vomica, when diacetic acid was found in the urine, and when great drowsiness existed. Coma never occurred. She had many prescriptions, but very few drugs, amongst which were pil. hydrarg., saline aperients, ammonium bromid., and magnesium carb., which cured the sickness; salicylates, potassium tartrate, Glauber's salts, soda and nux vomica, &c. She had no endocrine treatment.

The foetal heart varied in rate and sound, and also the foetal movements, according to the condition of the mother; and the study of this relation led me to understand better the cause of death of the first child in her previous pregnancy.

Result.—She gradually improved, and by the beginning of the ninth month her symptoms had all disappeared and very little sugar was left in her urine. About ten days before her delivery, she told me that "her womb had dropped," and examination showed the head of the child in the pelvis, and the urine, for the first time, was free from sugar, the specific gravity being 1015. The rapid change in the urine during the last week of treatment, coincident with dropping of the child's head into the pelvis, suggested a possible relief of some pressure, perhaps releasing some intestinal villi for their absorption work, or allowing more pancreatic secretion to pass in some cases.

The child was delivered at full time, alive, and weighing 9½ lb. Mother and child were both well a short time ago. The urine of the mother was free from sugar.

Instruments left in the Peritoneal Cavity : The Effects and Results of this Accident as shown by an Analysis of Forty-four hitherto Unpublished Cases.

By CLIFFORD WHITE, F.R.C.S.

MY interest in cases in which solid objects have been unintentionally left in the peritoneal cavity was aroused by having on two occasions to operate for their removal. I may state that in both these cases the solid body in question had been inserted into the abdomen by someone else, and that, to the best of my knowledge, I have never yet left an instrument in a patient's peritoneal cavity.

The notes of the two cases mentioned are the following:—

(1) *Spontaneous Partial Extrusion of a Pair of Hæmostatic Forceps from the Peritoneal Cavity through the Cervix.*—A patient, aged 50, was sent to me because her doctor had found a sharp foreign body in the canal of the cervix. She had consulted him on account of constant pelvic pain. The history given was that nineteen years before she had had a laparotomy performed in the country for a "tumour," but no details could be obtained. Seven years after this she was delivered of a full-time child without difficulty, and had had moderate health, except for one attack of severe abdomino-pelvic pain which subsided under treatment with hot fomentations. The abdominal pain, with nausea and vomiting, gradually increased in severity for several years, and for these symptoms she had had a second operation performed upon her in a county hospital eighteen months before I saw her. She was informed that the adhesions in the lower abdomen were so dense that nothing could be done, therefore the operation was abandoned and the abdomen closed. The pain and sickness increased and six months later an offensive vaginal discharge commenced. The bowels were relaxed, with frequent desire to defæcate, and there was frequency of micturition.

When I first saw the patient she looked toxæmic and wasted. The abdomen was distended and there was a tender mass below, and to the left of, the umbilicus. There was a hernia of the old laparotomy scar. On vaginal examination, a pointed metallic instrument could be felt protruding through the cervix for 4 cm. from the external os. The body of the uterus was bulky, and situated in the middle line. There was a mass on the left of the uterus and above it, continuous with the tender area on the abdominal wall. The X-ray photograph showed a Spencer Wells hæmostatic forceps, with the handles towards the left iliac fossa and the points in the cavity of the pelvis. It was obvious that any attempt to remove it by traction on the pointed end would lacerate the uterus severely, and possibly also damage the ureters. I therefore opened the abdomen, and was faced by a mass of very dense adhesions. With difficulty I reached the forceps; the points had eroded the uterus 5 mm. above the level of the bladder reflection; the handles had eroded the wall of the pelvic colon, and were lying inside the lumen of the gut. The forceps was removed, and a pan-hysterectomy performed, in order to get rid of the infected uterus, and to give free drainage to the fæculent abscess in which the forceps was lying. Owing to the large deficiency in the gut wall, and the infiltration of the surrounding tissues, it was difficult to suture the hole in the colon, but finally the edges were united, and it was not thought necessary to do a colotomy. Large drainage tubes were inserted through the vagina, and through the abdominal incision.

The patient stood the operation well, but a fæcal fistula formed at the end of a week, and was still discharging six weeks later. She then began to suffer from peculiar attacks of collapse with cyanosis, loss of consciousness, and vomiting. The first attack came on after eating eggs, which had been sent to the hospital, and she stated that she had never been able to eat eggs, as they had previously affected her in a similar manner. It was suggested that the attacks were anaphylactic in origin. In one of these attacks the patient died some six weeks after the operation.

The specimen shows a uterus 11 cm. long. Perforating the peritoneal covering of its left anterior surface, is the pointed end of a Spencer Wells forceps, which emerges into the canal of the cervix and then protrudes 4 cm. into the vagina. The whole forceps measures 11 cm. in length and is of the usual pattern now in use.

This case is a rare one, because of the extrusion of the forceps through a tough, thick-walled organ like the uterus. I have found no record of any other case in which this happened. It is also uncommon because of the long period that the foreign body remained in the peritoneum. It must have been left there either one and a half or nineteen years before, and the longer time is suggested by the history that the pain persisted and increased after the first operation, and was unchanged by the second. The existence of adhesions so dense at the second operation that it had soon to be abandoned also suggests that the inflammatory focus was already there. Also, supposing the forceps to have been left in at the second operation, it should have been finally found above the adhesions that the second operator did not separate, and not lay down. It is a question that cannot be settled, but it seems probable that the instrument remained in the peritoneum for nineteen years.

(2) *Removal of a Bone Penholder from the Epigastric Region.*—This foreign body, which had been passed into the vagina by a patient who was unable to withdraw it, remained in the peritoneal cavity from 8 a.m. on January 2, 1915, till 4 p.m. on January 5, 1915. Even under anæsthesia no scar could be seen in the vaginal vault, or on the fundus of the uterus, when the abdomen was opened. The penholder, with the ink stains still showing well, was found entirely wrapped up in omentum. I ligatured off the omentum containing the penholder, and closed the abdomen. The patient made an uninterrupted recovery.

In discussing these two cases with my colleagues, I found that few of them had had any experience of this accident, nor could I find much written about it except one very valuable article by Crossen, and the records of a few cases that had been published because they had given rise to legal proceedings.

In considering a question like the present, where negligence on someone's

part comes in, it is obvious that the great majority of cases that are not the subject of legal action will never find their way into the literature at all. To get details of the unpublished cases, I sent inquiries to surgeons in all parts of Great Britain. No names were asked for, but in order to obtain full, although anonymous, records, I posted printed inquiry forms with printed reply envelopes.

I wished primarily to find out what risk there is to a patient if an instrument is allowed to remain, say five or six days, in the peritoneal cavity. Such a question may be of importance (*a*) if the loss of the instrument is not noted till the patient has recovered consciousness; (*b*) if already suffering from such a degree of shock from the operation that undoing the laparotomy incision may be attended by grave risk; and (*c*) if it is desired to confirm the diagnosis and locate the instrument by radiography before proceeding to open up the incision. Secondly, after about what interval of time does the foreign body usually commence to cause symptoms, if its loss is not noted? Thirdly, what is its usual effect on surrounding viscera?

Crossen¹ gives a short tabular summary of fifty cases that he collected from the literature from 1880 to 1907. In these fifty cases the articles left behind were: Forceps, forty-one; drainage tubes, three; finger-rings, two; Nélaton catheter, glass irrigator, scissors, "piece of instrument," and pair of spectacles, one each. The total of fifty-one instruments in fifty cases is explained by two pairs of hæmostatic forceps being left in one patient by Kosinski.

Many of the cases abstracted by Crossen are incomplete, and essential details are lacking, but an examination of his collected cases shows that some twenty-four certainly, or probably, lived, thirteen died, and in thirteen cases the details are inconclusive. The time in the peritoneal cavity in the cases that recovered varied from a few hours to ten and a half years, but it is of interest that in only seven out of twenty-four was the period over one year. The foreign body was removed by a subsequent laparotomy in ten cases; removed through a sinus or abscess in seven cases; passed spontaneously *per rectum* in four cases; removed by colpotomy in two cases, and once was found in the bladder. In four of the non-fatal cases and two others it is stated that serious damage resulted to surrounding viscera.

NEW CASES.

Including a few cases in which foreign bodies had been passed into the peritoneal cavity either *per vaginam* or *per rectum*, I received details of thirty-nine cases; there are three specimens bearing on the subject in the Museum of the Royal College of Surgeons, making forty-four with the two cases recorded in detail above (*see* tabulated analysis, pp. 40-42).

The foreign bodies found were twenty-nine artery forceps, two retractor blades, two glass rods, two bone knitting-needles, two hairpins, and drainage tube, towel clip, uterine dilator, piece of needle, pin, bone penholder and stone, one each.

Of the forty-four patients, eleven died, giving a mortality of 25 per cent. In these eleven fatal cases the foreign body is stated to have been present: several years, three cases; seven years, one case; two years, one case; one year, one case; five months, one case; three weeks, one case; and twice "unknown." In all cases, except those where the details are wanting, severe ulceration or erosion of the surrounding viscera was present.

¹ *Amer. Journ. Obstet.*, 1909, lix, pp. 58, 250; and also "Operative Gynecology," 1915, p.589.

Of the thirty-three patients who recovered, cases are given in which the foreign body remained in the peritoneal cavity seven, twelve, fifteen and (?) nineteen years. Of these thirty-three cases, twenty-six were treated by a second operation, and in the remaining seven the instrument was passed through a sinus, or *per rectum*. The operation was performed within a few hours, five cases; within forty-eight hours, four cases; within a few weeks, eight cases; and the remaining nine during periods ranging from six months to many years.

A consideration of the details of the cases seems to show that, as would be expected, the best result is obtained by an immediate removal of the foreign body, if the patient's general condition permits of a second operation. But if the loss of the instrument is not noted at once, or if the patient's general condition contra-indicates an immediate second operation, there does not seem to be any grave risk in leaving it inside for a few days. A solid metal instrument does not seem to cause the onset of peritonitis as rapidly as a blood-soaked gauze sponge, and a consideration of the cases does not indicate that the viscera suffer any severe damage within a few days.

A striking fact is the protective action of the omentum in surrounding the foreign body and shutting it off from the rest of the peritoneal cavity. This is well shown in the second specimen exhibited to-night.

The frequency with which this accident occurs came as a surprise to me. I received details of thirty-nine fresh cases by post, and fifty-one forms were not returned to me at all, in spite of the fact that I especially asked for the form to be returned with a negative on it and sent a stamped addressed envelope to ensure it remaining anonymous.

It seems reasonable to think that a large proportion of those fifty-one unreturned forms must have given details of fresh cases if their recipients had returned them at all. Again, it should be noted that only surgeons on the larger hospitals were circularized, and if the records of cases operated on in cottage hospitals, naval and military hospitals, &c., could be obtained, it is probable that my numbers would be again largely increased.

Most of the cases in which any date was given occurred within the last fifteen years, so that my inquiries show that, quite roughly, at least forty of these accidents have occurred in one hundred and eight months, or, in other words, once in every four and a half months a patient is exposed to this unnecessary risk in Great Britain alone.

It is therefore desirable to consider what can be done to diminish the risk of this accident.

I have deliberately avoided so far all reference to sponges and swabs, and definitely asked that these should not be included in my inquiry form. But it is worth while emphasizing the protection given by the use of six or twelve yard rolls of gauze for packing off intestines and sponging, instead of using large numbers of small pieces of gauze. In counting a large number of swabs by an assistant or nurse, the usual margin of human error exists, and this can be avoided and time saved by the use of rolls of gauze as continuous sponges.

Regarding instruments, the same principle can be applied and the numbers of instruments in use, especially of Spencer Wells forceps, reduced to a minimum, an extra supply in a separate package being kept in the operator's bag for emergencies. Also any instrument brought near the abdomen while the peritoneum is open should measure 6 in. in length—an exception to this rule must be made in the case of needles.

ANALYSIS OF FORTY-FOUR HITHERTO UNPUBLISHED CASES OF INSTRUMENTS LEFT IN THE PERITONEAL CAVITY.

In the following list, the details are given as far as possible in the words used by the reporters of the case.

Case	Instrument and position	Nature and date of first operation	Symptoms	Treatment adopted and condition of viscera disclosed	Notes
1	Short Spencer Wells forceps; among small gut	Hysterectomy, several years before	?	Enterostomy; gut ulcerated	Whole length of forceps lying in cavity of gut; no external sinus present; death
2	Short Spencer Wells forceps; among small gut	?; several years before	?	Enterectomy and anastomosis; gut ulcerated	Whole of forceps lying in cavity of gut; no external sinus present; death
3	Small Spencer Wells forceps; among intestines	Ruptured gastric ulcer; many weeks	Faecal fistula in caecal region	Extraction through fistula; ulceration of ascending colon	Death from peritonitis
4	Small Spencer Wells forceps; partly in peritoneal cavity and partly in bladder	Ovariectomy; five months	Like stone in bladder	Laparotomy; bladder perforated and very dense adhesions present	Point of instrument seen during cystoscopy; no external sinus; death from peritonitis
5	6-in. Spencer Wells forceps; among small gut	?; several years	Acute obstruction	Resection of gut; two or three adjacent coils of small gut ulcerated	Ring carcinoma of pelvic colon also found and so colostomy done at same time; no external sinus; death
6	5-in. artery forceps; among small gut	Gastro-enterostomy; two years	Pain, increased by taking food	Double resection of small gut; gut perforated?	Instrument detected by radiogram; no external sinus; death
7	Spencer Wells forceps; ?	Appendectomy?	Fever, pain; mass in pouch of Douglas	Resection of small gut; small gut "damaged"	? no sinus present; death
8	5-in. Spencer Wells forceps; among small gut	Ruptured ectopic; three weeks	Fever, emaciation; discharge from faecal fistula in left iliac fossa for six months	Sinus explored and instrument removed	No external sinus; death
9	4-in. Spencer Wells forceps; in left half of pelvis; handles pointing downwards	Ovariectomy; seven years			A portion of one handle of the forceps could not be found; death
10	Towel-clip 3 in. long; ?	?; one year	?	?	Found at autopsy; Royal College of Surgeons Museum 2398 I
11	Hairpin; lower abdomen	?; passed <i>per vaginam</i> ?	Pelvic peritonitis	Laparotomy for purulent peritonitis; no obvious damage to viscera	Royal College of Surgeons Museum 2349
12	3-in. Spencer Wells forceps; below umbilicus in middle line	Ovariectomy; six weeks	Point of forceps palpable through scar	Removal by laparotomy; no damage to viscera	Forceps found wrapped in omentum; no external sinus; uneventful recovery
13	Small Spencer Wells forceps; ?	Removal of tumour, probably renal; eighteen months	Rectal pain and sinus	Passed spontaneously, <i>per rectum</i>	Recovery
14	Spencer Wells forceps; upper abdomen	Ovariectomy; several weeks	Hard mass in upper abdomen	Laparotomy; adhesions but no erosion of viscera	No external sinus; recovery
15	5-in. Spencer Wells forceps; pelvis	Salpingo-oöphorectomy; several years	Abdominal and rectal pain	Passed spontaneously <i>per rectum</i> ; ulceration of rectum	Recovery
16	Small Spencer Wells forceps; pelvis	Pan-hysterectomy; a few hours	None	Removal <i>per vaginam</i> ; no damage to viscera	Radiogram taken; recovery

	5 in. artery forceps; among intestines	Cæsarean section; forty-eight hours	None	Removal; no damage to viscera	Radiogram taken because instrument missed after operation; no external sinus; recovery
17					
18	7-in. Spencer Wells forceps; upper abdomen	Radical cure of ventral hernia; four months	?	Enterectomy; ulceration of "bowel"	No external sinus; recovery
19	Artery forceps; pouch of Douglas	Ovariectomy; six hours	None	Removal; no damage to viscera	Instrument missed after operation; palpated by bimanual examination; recovery
20	9-in. Spencer Wells forceps; pouch of Douglas	Sub-total hysterectomy; seven years	Pelvic pain	Extraction <i>per vaginam</i> ; ulceration of vagina	Points of forceps palpable on vaginal examination; recovery; no external sinus
21	Small Spencer Wells forceps; pelvis	Ileo-colostomy; six months	Left iliac pain	Passed spontaneously <i>per rectum</i>	Complete recovery; no external sinus
22	Spencer Wells forceps; attached to omentum	Ovariectomy; twenty-four hours	None	Removal; no damage to viscera	Loss noticed soon after abdomen closed; recovery uneventful
23	Kocher's artery forceps; among small gut	For perforated duodenal ulcer; three weeks	None	Removal and suture of bowel; 2 in. of instrument lay inside lumen of gut	Instrument only discovered by accident during radiogram of bismuth meal; complete recovery; no sinus
24	8-in. Spencer Wells forceps; lower abdomen	Hysterectomy; twelve days	None	Removal by laparotomy; no damage to viscera	Forceps missed after operation, but the patient was too ill for immediate search to be made; no sinus; radiogram on tenth day; uneventful recovery
25	6-in. artery forceps; pelvis	Ovariectomy; nine months	Febrile convalescence; recurrent abdominal pain; rectal pain and discharge	Passed spontaneously <i>per rectum</i> ; rectum ulcerated	No external sinus; recovery
26	Small Spencer Wells forceps; right iliac region	Removal of broad ligament cyst; ten days	None	Removal; no damage to viscera	Forceps missed three hours after operation; radiogram taken; recovery; no sinus
27	8-in. Spencer Wells forceps; pelvis	For pyosalpinx; four years	Indefinite	Removal; severe adhesions present but no erosion of viscera	Abdomen examined on account of hæmatemes and forceps felt through wall of lower abdomen; radiogram taken; no external sinus; recovery
28	5½-in. artery forceps; pelvis	Hysterectomy; a few hours	None	Laparotomy; no damage to viscera	Forceps missed two hours after operation; recovery
29	4½-in. artery forceps; lower abdomen	Ovariectomy; four hours	None	Laparotomy; no damage to viscera	Forceps missed three hours after operation; recovery
30	Flange of a Berkeley retractor; lower abdomen	Hysterectomy; twenty-four hours	None	Removal by laparotomy; no damage to viscera	Instrument missed after operation; radiogram taken; recovery
31	Blade of retractor 3 in. long; loin	Hysterectomy; fifty-four hours	None	Removal by laparotomy; no damage to viscera	Radiogram taken; recovery
32	Artery forceps; lower abdomen	?; twenty-four hours	None	Removal by laparotomy; no damage to viscera	Recovery
33	Drainage tube 24 in. long; partly in bladder and partly in peritoneal cavity	Hysterectomy; twelve years	Leucorrhœa for twelve years; cystitis three weeks	Removal by suprapubic cystotomy; ulceration of bladder	No external sinus; recovery

ANALYSIS OF FORTY-FOUR HITHERTO UNPUBLISHED CASES OF INSTRUMENTS LEFT IN THE PERITONEAL CAVITY.

Case	Instrument and position	Nature and date of first operation	Symptoms	Treatment adopted and condition of viscera disclosed	Notes
34	Glass rod 4 in. long; rectovesical pouch	Colostomy; four and a half months	All symptoms were referred to a large wound in the buttock (ventral hernia)	Removal; dense adhesions but no serious erosion of viscera	The rod was found and removed during an operation for removal of the rectum; recovery
35	Flanged glass tube 2 in. long; under right rectus muscle	For relief of ascites; fifteen years before	None	None	The tube is still there and the patient in good health; the ascites is cured; no external sinus present
36	Hairpin (straightened); passing from rectum and transfixing the Fallopian tube	? Passed into rectum; ? two months	Dysmenorrhea; rectal pain	Removal by laparotomy; peritonitis round tube; ulcer in rectum	No external sinus present; recovery
37	5-in. bone knitting needle; left iliac fossa	Pushed through uterus; fifteen months	Abdominal pain which was increased by movement	Removal by laparotomy; needle surrounded by omentum; no damage to viscera	No external sinus present; radiogram taken; recovery
38	Uterine dilator 8 in. long; below liver	Pushed through uterus; four days	None	Removal by laparotomy; no damage to viscera	No external sinus present; radiogram taken; recovery
39	Bone knitting needle 10 in. long; pouch of Douglas	? Pushed through vagina; ?	Intestinal obstruction	Removal by laparotomy; no damage to viscera	No external sinus present; recovery
40	Piece of steel needle 1 cm. long; near caecum	? Appendectomy; two years	Persistent sinus	Sinus opened up and needle removed; no damage to viscera	Recovery
41	Pin; back of broad ligament	None; ? swallowed	None	Removed during ovariectomy for ovarian cyst; no damage to viscera; slight adhesions present	No external sinus present; recovery
42	Stone; lower abdomen	No operation; three days	Pain	Removal by laparotomy; perforation of rectum	The stone was accidentally forced through the rectum; Royal College of Surgeons Museum 2310 I; recovery
43	4½-in. artery forceps; left iliac fossa perforating uterus	Two operations; ? eighteen months; ? nineteen years	Pain; diarrhoea; leucorrhoea	Abdominal hysterectomy and suture of colon; ulceration of colon	No abdominal sinus present; radiogram taken; sudden death six weeks later; case recorded in detail above (see p. 36); Royal College of Surgeons Museum
44	Bone penholder 5 in. long; above umbilicus, wrapped in the omentum	Pushed through vagina; three days	Pain	Removal by laparotomy; no damage to viscera	Recovery; University College Hospital Museum

To conform as far as possible with the ideal I never use towel clips—it is quite easy to fix all towels by two stitches, one at the pubes and one at the upper end of the incision. Again, it is quite easy to fix efficiently the rubber sheeting, towel, or whatever is used to protect the edges of the abdominal wall by pieces of sheet lead, 3 in. broad and 12 in. long, which can be bent round the edge of the wound. This method has long been used by Oldfield and other members of the Leeds school. The use of a Reverdin needle not only saves time but largely reduces the number of small needles in use. A Reverdin needle of suitable shape can be used satisfactorily except at the bottom of a deep wound.

In the Trendelenburg position, instruments tend to roll down towards the incision, and the use of an instrument tray (such as that suggested by Bonney) with a large flange will prevent this.

While operating, all hæmostatic forceps should be replaced by ligatures as soon as possible, instead of their being left hanging on to the bleeding points at the edges of the wound or within the peritoneal cavity.

The use of any instrument that has loose parts that are liable to become detached should be avoided.

DISCUSSION.

MR. ARTHUR GILES said that one of the cases quoted in the table had occurred in his (Mr. Giles's) own practice though he was not responsible for the foreign body. It was the case in which a pin was found embedded in the right broad ligament. It was discovered in the course of doing an ovariectomy. He could only suppose that the patient at some time must have swallowed the pin, and that this worked its way out through the bowel wall. The important practical side of the paper was the question as to how surgeons in the future could guard against such an accident as leaving an instrument in the abdomen. In his opinion the two principal points were that as few instruments should be used as possible, instead of the formidable array which some surgeons seemed to like to see displayed; and that operations should be done in such an orderly and methodical manner that the surgeon should know what his instruments were doing. As the question of swabs had been mentioned in the paper, he might say that the use of long rolls of gauze did not appeal to him at all. The gauze quickly worked up into strings which failed completely in the essential purpose of keeping the bowel out of the way. He preferred a large gauze and Gamgee pad, 10 by 12 in. square, sewn round the edges. Only by gross carelessness could such a pad be overlooked. Further, he considered that the plan long in use at the Chelsea Hospital for Women reduced the chances of error in the counting of swabs to its minimum. Only two large swabs and six or eight smaller ones, about 6 in. square, were put out. When a swab had been used it was placed in a bowl of sterilized water and washed out and used again. The sole duty of the ward sister was the charge of the washing of the swabs. In other hospitals he had seen operations at which eight, ten, or twelve dozen small swabs were used: this method enormously increased the chances of a miscount.

DR. H. RUSSELL ANDREWS said that his own experience was limited to one case in which first a celluloid bougie and later a metal dilator were pushed through the wall of the uterus in attempts to induce abortion. Dr. Andrews had removed the bougie through a small incision in the posterior fornix, and, after an X-ray photograph, removed the metal dilator, 8½ in. long, through a median abdominal incision about 1 in. long. The upper end lay just below the liver. The patient, who was not pregnant, had suffered no inconvenience from the presence of the foreign bodies, and made an uninterrupted recovery. The bougie had been in the abdomen for six weeks; the metal dilator for two or three days. The original operator said that he had not worried about the bougie as he "thought that it would be absorbed," but, after a thorough search under the bed had convinced him that the metal dilator had not fallen on the

floor, he was considerably alarmed, as he felt sure that the dilator was inside the uterus which he considered to be three months pregnant. Though this was the only case in which he (Dr. Andrews) knew of instruments being left in the abdominal cavity he could relate a case in which disaster occurred from a still more unexpected accident—viz., a tumour being left in the abdomen after separation of all its attachments. He saw only the beginning of the operation, as he was interested in the diagnosis. An operator of great experience opened the abdomen and showed Dr. Andrews a large hydrosalpinx on each side. As the removal of these tumours did not seem to promise anything of much interest he did not stay to see the operation completed. The patient developed peritonitis and died. A post-mortem examination was performed and a large necrotic hydrosalpinx was found loose in the abdomen, all the attachments having been severed. He did not know what was the explanation—possibly sudden hæmorrhage made it necessary to pack off the intestines to expose the bleeding point and the tumour was pushed out of the way and forgotten. It was extraordinary that neither the operator nor his assistant noticed that only one trophy was to be seen after the operation instead of two. This must be a very rare occurrence, otherwise it would be necessary to add a new rule to the old one which warned the operator to count his swabs and his pressure forceps—namely, “count your tumours.”

Mr. BECKWITH WHITEHOUSE showed a specimen, consisting of half a bone knitting needle, which was removed from a married woman who had introduced the same through the uterine cavity for the purpose of procuring an abortion. The patient's history was as follows: The catamenia, previously regular, ceased in August, 1917. Thinking that pregnancy had occurred she introduced a bone needle into the uterus on October 26, 1917. Upon withdrawal of the instrument it was noticed that half had been retained. Considerable pain occurred and she consulted her medical attendant, who explored the uterus at once and removed a two months' pregnancy, but failed to find any foreign body. No immediate complication occurred and the patient made a sound recovery. The catamenia were resumed five weeks after the curetting. At the end of December, 1918, the patient noticed abdominal pain, especially on bending, and she was referred by her medical attendant for further examination. A radiograph revealed the shadow of an elongated foreign body lying within the abdominal cavity. Laparotomy was performed upon January 18, 1919, and the missing half of the needle removed from the peritoneal cavity. The foreign body was completely enveloped by omentum, but no wound of the viscera was noted. A scar was present at the fundus of the uterus, marking the place of perforation. The patient made an uneventful recovery.

Dr. L. MARTINDALE alluded to a patient of hers, a woman, aged 25, who seven weeks ago had swallowed six needles, one pin, one hairpin, and part of a tortoise-shell comb. She had operated last week and had found only one needle in the peritoneal cavity, the others being in the stomach and jejunum (a gastro-jejunostomy had been performed some years previously). As in Mr. Giles's case there had been no actual symptoms arising from the presence of these foreign bodies.

Mr. EARDLEY HOLLAND said that for several years he had adopted the rule of never using a short instrument in abdominal operations; the shortest instrument he used was a dissecting forceps measuring 7 in. and his artery forceps, needle holder, scissors and other instruments all measured 8½ in. or more. This did not ensure absolute safety, but there was much less chance of leaving an instrument in the abdominal cavity if it was of such length that one end would probably protrude from the wound.

Section of Obstetrics and Gynæcology.

President—Dr. T. W. EDEN.

A Necrotic Fibro-adenoma in a Patient, aged 74, simulating Cancer of the Corpus Uteri.

Shown by J. S. FAIRBAIRN, B.M.

THIS specimen shows a necrotic tumour attached to the inner surface of the uterus, but distinct from it. It is a cystic fibro-adenoma and the vessels in the uterine wall are distinctly degenerated. The reason for its being shown is because of its clinical interest; it was removed from a multipara, aged 74, who gave a history of slight uterine hæmorrhage with offensive discharge for two months. She was stout with a very thick abdominal wall, which made bimanual examination difficult. The cervix was small and atrophic and showed no sign of disease, but the uterus appeared to be somewhat enlarged. She was not a favourable patient for a radical abdominal operation, and before removing the uterus it was decided to explore the cavity and only perform hysterectomy if the condition was clearly malignant. Portions of necrotic material came away as the dilators were withdrawn, and this appeared to be sufficiently certain evidence of malignancy to make it unnecessary to go any further; the cervical canal was, therefore, sewn up and the uterus removed *per abdomen*. On examination of the specimen it is perfectly clear that the tumour could have been removed *per vaginam*, and the innocent nature of the growth shows that this would have been quite satisfactory. It is polypoid and could have been easily detached from the uterine wall by the finger. The section under the microscope shows the growth to be made up of cystic spaces lined by endometrial glandular tissue with a little fibrous tissue. Professor Dudgeon described it as a cystic fibro-adenoma. A simple growth of this type arising from an atrophic endometrium is outside my experience and this specimen is brought before the Section in order to learn whether others present have met with similar tumours at this age.

A Cyst of the Uterine Cornu due to Dilatation of the Interstitial Portion of the Tube.

Shown by J. S. FAIRBAIRN, B.M.

THIS specimen shows a cystic swelling in the right cornu of the uterus in a patient, aged 42, who was operated upon for an inflamed ovarian cyst with both Fallopian tubes distended with blood-stained fluid. An incision was made into the cystic swelling in the cornu, which was about the size of a

46 Fairbairn: *Cyst of Cornu*; Whitehouse: *Adenomatosis Vaginæ*

walnut, as it was thought to be a soft fibromyoma which could be enucleated, but fluid similar to that contained in the tubes escaped. The body of the uterus was removed as the nature of the cyst was uncertain and at the time it was thought to be of adenomyomatous origin; on subsequent examination it proved to be a distension of the tube within the uterine wall. Dr. Dyke, Pathologist to the Surgical Unit of St. Thomas's Hospital, kindly examined the specimen and, as the slides under the microscope show, reported that the wall of the cyst in the uterus had a mucous membrane similar to that in the tube, from which it seems to have arisen by blocking up of the lumen by inflammation. Sections of the wall of the tube show chronic inflammation with hæmorrhage into its tissues. This condition is one I have never met with before and I have brought it forward in order to ascertain if others have seen anything like it.

DISCUSSION.

Dr. HERBERT SPENCER said he had on one occasion diagnosed cancer of the body in an elderly patient with a foul and hæmorrhagic discharge. It was operated on by another gynaecologist through the cervix and found to be a sloughing fibroid. Dr. Fairbairn's case of adeno-fibroma resembled one shown by the late Dr. Maxwell as a case of cancer of the body.

Dr. T. W. EDEN (President) said that he thought a fibro-adenomatous growth of the endometrium so large as the specimen shown by Dr. Fairbairn was very uncommon. Incidentally the case illustrated the fact that benign uterine tumours were a source of danger even in advanced age, from the risk of necrotic changes followed by sloughing. He thought that this case, illustrated by the second specimen shown by Dr. Fairbairn, was almost unique; he could not recollect having seen or read of a cyst formed from dilatation of the interstitial portion of the tube.

Adenomatosis Vaginæ.

By BECKWITH WHITEHOUSE, M.S.

THE patient from whom the specimens were obtained was a 2-para, aged 40, who was referred to me on November 27, 1922, by Dr. Dick, of Alvechurch, Worcestershire. She gave a history of constant muco-purulent vaginal discharge of some years' duration, and unrelieved by the usual methods of vaginal douching, tamponage and various applications to the cervix uteri. The last pregnancy had been terminated by forceps five years previously and the discharge had gradually increased from this date. Throughout the year 1922 it had been particularly troublesome and the patient was willing to submit to any treatment for its relief.

Examination showed a very unusual condition of the vaginal vault. The cervix was deeply lacerated, fibrosed and everted. It was the seat of numerous small cysts of the typical Nabothian type. The interesting feature of the case, however, was the extension of the cystic condition to the upper half of the vagina. Both anterior and posterior walls of the vagina were completely studded with small cysts varying in size from that of a pin's head to that of a small pea. The right vaginal fornix was packed with these cysts and they extended on the right side of the vagina in a chain almost to the vulva. Some of the cysts appeared translucent and resembled sago granules. Others

occupied a subepithelial position and could only be palpated as nodules. A few in proximity to the cervix were involved in granulation tissue, but in general terms the vaginal epithelium appeared to be healthy and intact, without any inflammatory reaction. A considerable amount of mucopurulent discharge occupied the upper fourth of the vagina but this appeared to be chiefly cervical in origin.

The clinical appearance resembled the condition described and figured by Bonney and Glendinning¹ as adenomatosis vaginæ and this diagnosis was made.

A further point of interest in the case is that the patient's uterus was found to be of the bicornis unicollis type. This was not appreciated until an examination under anæsthesia was made, when a sound passed easily into each cornu.

Treatment.—The cervix was amputated by Schroeder's method, several of the vaginal cysts were excised for microscopical purposes and the remainder as far as possible were obliterated by means of the cautery. Owing to the number of cysts present this proved to be rather a tedious proceeding. The vagina was subsequently packed with bismuth gauze.

A bacteriological examination of the cervical canal and vaginal discharge failed to show any point of interest. So far it is too early to say anything definite about the subsequent history. I am informed that up to date the patient has reported herself to be comfortable and free from discharge.

As regards the pathology of the vaginal cysts, the first report received stated that the cavities were lined by a single layer of flattened cells and that the cysts were presumably lymphatic in origin. Apparently this report was based upon an investigation of the larger cysts only. Subsequently a second statement was made to the effect that the epithelial lining of the small cavities was cubical and in places columnar. A similar appearance was seen in the cysts present on the cervix, and this points to the fact that they are all probably adenomatous in origin. The cysts are embedded in a fibrous matrix, and with regard to the vaginal tissues no evidence of recent inflammation is present.

The question naturally arises as to whether the condition is the result of embryological or of inflammatory factors. The co-existence of a uterus bicornis unicollis is evidence in favour of embryological agency. On the other hand, the distribution, clinical history and similarity between the cervical and vaginal conditions appear to point to an extension of chronic inflammatory reaction from the cervix to the vagina. Small isolated cystic glands appear to be not uncommon about the vaginal vault in association with chronic inflammatory lesions of the cervix uteri. I have frequently met with them under conditions calling for amputation or plastic repair of the latter. The case here described appears to be a very exaggerated instance of the same, and is reported as such, rather than as an example of displaced Wolffian or Müllerian rudiments.

Dr. T. W. EDEN (President) thought that the origin of these adenomatous growths of the vaginal wall was a difficult point to decide. It might well be that they arose from embryonic rests of Müllerian epithelium, and the occasional occurrence of simple tubular glands in the vaginal wall which had been demonstrated by several observers might be explained in the same way.

¹ *Proceedings*, 1910-11, iv (Sect. Obst. and Gyn.), pp. 18-25.

Inversion of the Uterus occurring in the Third Week of the Puerperium.

By W. R. WHITE-COOPER, M.B., B.S.Lond., and
H. K. GRIFFITH, F.R.C.S.Eng.

THE patient was a Mrs. K., aged 26, primipara, under the care of W. R. W.-C. Full time labour began on March 14, 1922, the contractions being slow and scarcely painful. At the end of ten hours the head was in the pelvis, position R.O.A., the vertex presenting and the os fully dilated. Three hours later the head had descended on to the perineum, but the pains were few and hardly felt, and the patient was spending the time playing cards. After several hours waiting the pains did not become any stronger, so 0.5 c.c. of pituitary extract was given subcutaneously. Chloroform anæsthesia was induced, and the forceps applied, and with one good pain the child was born alive.

One hour later there had been only a few feeble uterine contractions, so half a drachm of extract ergot liq. was given by mouth, and a further 0.5 c.c. of pituitary extract given subcutaneously. Later the placenta was found in the vagina with the membranes still adherent; the membranes were peeled off intact and the uterus massaged. There was severe post-partum hæmorrhage controlled by bimanual compression of the uterus and hot douches. The patient was collapsed and practically pulseless, but slowly rallied under the administration of rectal salines, hypodermic injections of strychnine and Curschmann's solution. The perineum, which had been torn back into the rectum, was sutured.

During the next fourteen days the patient slowly improved in general condition but remained very anæmic, and the uterus contracted down slowly. The temperature varied between 99° F. and 101° F., the pulse-rate from 96 to 118. The lochia were offensive, but ceased to be blood-stained about the tenth day.

On March 26 when the sutures were removed from the perineum the wound fell open, showing no signs of attempt at union. On vaginal examination it was noticed that both the external and internal os were dilated and patulous. There was a uterine discharge, small in amount and slightly offensive.

On March 29 the patient sneezed several times, and this was followed by bright hæmorrhage from the vagina, about 2 to 4 oz. in amount. Very slight hæmorrhage occurred on one or two occasions subsequently. Three days later she had a rigor in which the temperature rose to 105.2° F. and the pulse-rate to 140; 20 c.c. of polyvalent streptococcal serum were then given. On the next day she had two more rigors. When seen at 6 p.m. by H. K. G. the patient looked very ill and anxious. There was marked anæmia, the temperature was 101° F., the pulse 130, volume poor. Nothing abnormal was discovered in the chest. The abdomen was retracted with slight tenderness over the lower third, the uterus not being palpable. *Per vaginam* the perineum was completely torn into the rectum, with no signs of any attempt at granulation on the raw surfaces. The cervix was widely dilated, and protruding through it was a smooth round tumour, about 4½ in. in diameter, which bled easily on touch. Bimanually the body of the uterus could not be identified

above the cervix, and in place of it a dimple could be felt. The diagnosis was thus made of inversion of the uterus with possibly septicæmia.

Under a general anæsthetic a hot vaginal douche was given, and the body of the uterus grasped and slowly compressed, so that after about five minutes it began to re-invert, the fundus slipping back quickly through the cervix. Hot intra-uterine douches were given, but in spite of this the uterine wall remained flabby, and was dented in by the pressure of the intestines above, so the cavity was packed with gauze soaked in flavine. The perineum and rectum were rapidly sutured with two layers of catgut and silkworm gut, the sphincter being left unsutured; 500 c.c. of gum saline were given intravenously at the end of the operation. She had a rigor, the temperature rising to 106° F., on coming round from the anæsthetic. No pituitary extract or ergot were given owing to fear of irregular uterine contraction. The patient had no after-pains; the plugging was slowly withdrawn, beginning twenty-four hours after the operation, and it was completely removed by the end of the third day. It was very offensive. During the first three days the patient had several rigors, the highest temperature being 106·2° F. and pulse 120.

Other treatment consisted in daily subcutaneous injections of cacodylate of iron and strychnine and 20 c.c. of polyvalent streptococcal serum and douches. The patient's condition slowly improved, the rigors gradually diminishing in severity and frequency, but at the end of a week thrombosis of the right leg took place. At the end of the second week the catgut in the perineum gave way and the whole wound gaped, showing no attempt at healing. At the end of three months the patient was convalescent and able to get up. She has since resumed her work as a school teacher, and apparently suffers from no inconvenience from the torn sphincter and perineum.

The interest in the case lies in the peculiar inertia of the uterus which was present throughout the first stage of labour; it practically caused no inconvenience to the patient, but it almost completely prevented further progress. After the child's birth there was very poor power of retraction, and this resulted in the inversion, which possibly began with the patient's sneezing attack. The intense anæmia undoubtedly assisted in the subinvolution as well.

DISCUSSION.

Dr. HERBERT SPENCER referred to his paper read before the Section on nine cases of inversion of the uterus,¹ in which he had pointed out the danger, which the late Dr. Maxwell had emphasized, of replacing a septic inverted uterus, and the advantages of irrigation before reducing the inversion with Aveling's repositor. He thought it was futile and dangerous to stitch up a torn perineum in the presence of a septic condition of the parts.

Dr. J. D. BARRIS congratulated the authors on the successful termination of an extremely serious and anxious case. He said that Dr. Donaldson and himself had reported before this Section (November 4, 1920),² a case somewhat similar from the fact that the inversion occurred apparently a considerable time after labour, and was complicated by sepsis. The patient, aged 24, was in her third labour, which was managed by a midwife. Labour was uncomplicated until the expulsion of the placenta, which was delayed for more than two hours, and after that time the midwife delivered it by pressure on the abdomen and traction on the cord; the patient then became collapsed and bled freely. On her admission to hospital she was in a condition of shock. The fundus could be

¹ *Proceedings*, 1919-20, xiii (Sect. Obst. and Gyn.), pp. 20-37.

² *Proceedings*, 1920-21, xiv (Sect. Obst. and Gyn.), p. 207.

felt protruding slightly through the cervix. She was treated first for shock and hæmorrhage by blood transfusion. It was decided to delay reposition of the fundus. Sepsis was evident by the third day of the puerperium, but was controlled by means of continuous irrigation through Carrel's tubes. On the sixth day of the puerperium, while the patient was micturating, the fundus became completely inverted. He (Dr. Barris) would make two comments upon the case just reported. The first was with regard to the time at which the inversion occurred. He gathered that in the opinion of the authors the inversion did not take place until the fourteenth day of the puerperium. He thought it more probable that it really began immediately after labour, for the placenta was manually removed and they found the membranes adherent at a time when they also noticed that the uterus was relaxed. Moreover, in his own case, although the inversion became complete on the sixth day, it had been noted on admission that it had already really begun. His second comment was with regard to immediate replacement of the fundus. Shortly before his own case occurred he had heard Dr. Spencer's paper. Late reposition was tried partly for this reason, and also not to subject the patient to the shock of manipulations and an anæsthetic. He found that the uterus involuted well, and did not bleed while still inverted. Reposition was not carried out until seven weeks after the delivery. The fundus was replaced by means of Aveling's repositor without anæsthesia in nineteen hours, and the repositor was removed easily at the end of this time after five minutes' further traction. He wished to endorse Dr. Spencer's remarks on this point, and to emphasize the safety and value of late reposition as opposed to immediate replacement of the inverted fundus.

Dr. W. R. WHITE-COOPER and Mr. H. K. GRIFFITH also read a short communication on "A Case of Obstructed Labour."

The Technique of Cæsarean Section.

By SAMUEL J. CAMERON, M.B.

MY object in making this communication is to relate briefly the technique which has enabled me in 107 successive cases of Cæsarean section in rachitic subjects to bring the mortality in my practice to under 1 per cent., and also to record a few observations which have interested me in connexion with the operation.

Although many of the patients in the series were admitted to hospital in labour, it is a decided advantage to have the patients under observation for some days before the operation takes place, as minor ailments can be treated and thorough preparations can be made. For example, special attention should be given to patients with "colds," as it has been my experience that rachitic patients are peculiarly susceptible to pulmonary complications after operation, and it will be found that many of them are troubled with chronic bronchitis and emphysema. Consequently the choice of an anæsthetic seems to me to be important. Chloroform should be used in preference to ether as the chloroform has a less chilling effect on the lung; (in my gynæcological practice I generally employ ether). Precautions should also be taken to guard against exposure. I have frequently seen patients kept naked on the table for many minutes after the completion of the operation, while nurses with lotions were assiduously removing every trace of staining from the skin. As soon as I have secured the anchor sutures which retain the dressing, the patient is wrapped up and her head covered with a blanket while she is being removed to a warm room.

The patient in the one fatal case in my series died from broncho-pneumonia ten days after operation; ether alone was administered during the section, and since then I have always used chloroform. So far I have not tried spinal anæsthesia and gas in Cæsarean section, but it is doubtful whether this method of anæsthesia will lessen the incidence of bronchitis. Whenever bronchitis proves troublesome after operation I immediately place the patient in a tent with a steam kettle. Invariably she experiences great relief and in cases in which the heart exhibits signs of flagging camphor in oil proves an effective stimulant.

The next danger to be considered is the important one of sepsis. At the outset of my surgical career I performed Cæsarean section even when the membranes had been ruptured for many hours, and the fact that the forceps had been applied by the practitioner did not deter me. As a rule the patients recovered, but a few died and others had a prolonged convalescence due to septic infection. The loss of life was regrettable and accordingly I determined to operate only in cases which were presumably free from contamination. I therefore now perform craniotomy in most cases in which the patients have been repeatedly subjected to vaginal examinations before admission to hospital, and in all such cases if the membranes have been ruptured for longer than twelve hours. Even if a patient had not been examined vaginally I found that the mortality was great if labour had been allowed to proceed until exhaustion occurred after rupture of the membranes. Cases of this description I now treat by craniotomy instead of Cæsarean section. Other important factors in the elimination of sepsis will be mentioned as I describe the operative technique which I adopt.

For many years I have operated on my gynæcological cases either through an incision in the rectus sheath or through a transverse wound. Within recent years I have adopted the incision through the rectus sheath in cases of Cæsarean section owing to the fact that a weak wound is liable to be obtained by a middle line incision as the abdominal wall in this situation is attenuated by the large gravid uterus. Since I adopted this incision ventral hernia has been abolished from my obstetrical practice. Owing to stretching of muscle and fascia the wound should be made at a considerable distance from the middle line. It is a matter of some importance as to whether the right or left side is chosen. The incision ought usually to be made on the right side as the uterus almost always lies towards the right, and therefore the left margin of the organ approaches the middle line of the abdomen. Some time ago I was performing the operation in a country establishment where the lighting equipment was miserable, and the uterus was exposed through an incision in the left rectus sheath. As soon as the uterine incision was made profuse hæmorrhage occurred from the lower angle of the wound and it was found that some large veins in the left broad ligament had been severed. Difficulty was experienced in arresting the hæmorrhage. The left border of the uterus in this case was situated near the middle line, and if the incision had been made on the right side of the abdomen the above unpleasant complication would have been avoided.

Every surgeon who has had a large experience in dealing with cases of repeated Cæsarean section has probably been hampered on several occasions by numerous and dense adhesions in the region of the former scar or scars. The attachment of the uterus to the parietes can often be diagnosed before the abdomen is opened, owing to movements communicated to the uterus being accompanied by in-dragging of the abdominal wall. In consequence of the

intimate fusion which often exists between uterus, parietal peritoneum and intestine, the operation may be dangerously prolonged if the usual technique of dealing with adherent cases is followed. I have known an accomplished obstetric surgeon lose a patient from shock after a tedious operation of this description. In my opinion destruction of existing adhesions is inadvisable, as their obliteration would probably be followed by the formation of fresh bands and in some instances intestinal loops might adhere to a denuded portion of the uterine surface which was formerly in contact with parietal peritoneum.

In treating adherent cases I take this opportunity of strongly advocating my practice of evacuating the uterine contents through transverse incisions in the abdominal and uterine walls. By so doing the surgeon can avoid the labyrinth of adhesions, and as the uterus is usually tightly fixed to the parietal peritoneum the organ remains there in a state of ventro-suspension. It is probable that the attachment of the uterus to the parietes favours incomplete retraction, as on several occasions I have observed profuse post-partum hæmorrhage in these cases. Pituitary extract injected into the uterine tissue will prevent this complication. As a rule I make the transverse incision a short distance above the upper extremity of the old scar and this will usually be situated above the level of the umbilicus and near the fundus of the uterus. Upon the abdomen being opened, the free portion of uterine wall near the fundus is severed transversely, and then the child and placenta are extracted. By this procedure I avoid adhesions and the operation is almost as easily performed as in an uncomplicated case.

Some obstetricians still omit to place gauze between the parietal peritoneum and the uterus before the uterus is incised. This is a mistake, as blood and liquor amnii obtain access to the peritoneal cavity. Should the case be infected, such contamination may prove fatal. Before opening the uterus I insert four large swabs, so that two are situated laterally and the other two at the upper and lower angles of the abdominal wound. The swabs are not removed until the wound in the uterine wall has been closed.

Some years ago it occurred to me that lives were lost owing to a single knife being used throughout the operation. Many urgent cases of Cæsarean section are subjected to a hurried preparation and it seems to me that the knife which severs an infected skin may carry organisms into the wall of the gravid uterus with fatal consequences. Accordingly I altered my technique and used two knives, one for the abdominal and the other for the uterine wall. It may be merely a coincidence but nevertheless my results improved immediately.

When the uterus has been opened I always deliver the child as a breech, and as the limb is often greasy the nurse who is attending to the dressings keeps a large swab in readiness to pass on to me to prevent the hand slipping during extraction. Immediately the child is delivered the uterus should be drawn through the parietal wound on to the abdominal wall. There the organ should be turned inside out, so that the placenta and membranes can be stripped from their attachment. For this purpose a large pledget of gauze should be used. In most instances the membranes in the region of the cervix are the most difficult to detach and it is highly desirable that they should be entirely removed, as even a small portion overlying the os may prevent the lochia from escaping for several days. From information supplied by Dr. Murdoch Cameron and many of his former pupils and house surgeons we may conclude that the extremely useful procedure of inverting the uterus

was first practised by him, but he employed it only in cases where the membranes were adherent to the lower segment. Gradually the practice became an invariable one at the Glasgow Maternity as it ensured thorough emptying of the cavity. When the extraction of the placenta and membranes has been completed the inner wall is thrust back into position in order that the uterine wound may be closed. Many years ago I accomplished this by means of several mattress sutures and one continuous suture, but I have discarded this method as I found that in some instances there was morbidity in the puerperium. Doubtless this may have been due to excessive compression of tissues by the mattress sutures. For this reason also, I never place mattress sutures in the cervical stump after the operation of supravaginal hysterectomy, as on one occasion, in which the patient was an anæmic woman, a suture of this description caused localized necrosis. After abandoning the mattress suture I inserted three interrupted sutures of silk: one suture was placed in the middle of the wound and the other two midway between the first suture and the upper and lower angles of the wound. Each suture passed through the entire depth of the uterine wall, with the exception of the inner layer. The remainder of the wound was closed with interrupted sutures of catgut. Finally a continuous suture of catgut was used throughout the entire length of the wound. The three sutures of silk were a safeguard in case the catgut became absorbed too rapidly. Care was taken to cut the ends of the sutures level with the surface of the uterus. A sinus did not develop in any case. Despite the warning given by Mr. Eardley Holland in his valuable communication¹ as to the dangers arising from the use of catgut, I sometimes rely entirely on this material as when properly prepared it holds the tissues in apposition as efficiently as silk, and moreover it has the advantage of disappearing within three weeks. If union of the various elements has not taken place by that time it never will. My experience has been that all interrupted sutures lie loosely in tissues after a few days and so silk does not keep the tissues more firmly lashed together than catgut. It seems to me that the free use of silk within the abdomen is unjustifiable, as the presence of this durable material may readily lead to the formation of dense adhesions, and, moreover, it may be the cause of great misery if it becomes infected, as a persistent sinus forms. This event is of common occurrence and on two occasions patients were sent to me for the closure of uterine fistulæ which had resulted from infected silk sutures. Each month the unhappy women menstruated on to the abdominal wall. This evening I also show you vesical calculi, which originated from silk sutures in the uterus having made their way through the wall of the bladder.

Adhesions within the abdomen are usually undesirable, but I have met with cases of repeated Cæsarean section in which their presence seemed advantageous, since they caused firm fusion between the uterine scar and the parietal peritoneum, thus diminishing the tendency to rupture. Despite various modifications in the method of closing the uterine wound, the danger of rupture in subsequent pregnancies still exists. Personally I have met with four cases. From my observations on these cases I have come to the following conclusions: The gravity of this complication depends to a great extent on whether the uterus manages to expel its contents completely or not. (1) If there be only partial extrusion from the cavity, the uterus cannot retract effectively and death may rapidly ensue from hæmorrhage. (2) On the other hand, if the entire contents are quickly ejected into the peritoneal

cavity, and if rupture be unaccompanied by severe shock, I believe that the patient may actually walk into hospital and live for some days without operation. In such instances little blood will be found in the belly cavity, and the empty uterus remains in a state of firm retraction. (3) I am also of opinion that in cases in which the uterine wound has been infected there is less loss of blood when rupture occurs, as the margins of the wound have never remained in apposition and the opposed surfaces are non-vascular.

Frank's procedure of opening the uterus in its lower segment has been more extensively practised during recent years in Germany and America with the object of abolishing rupture. Modifications of Frank's technique have been introduced from time to time, and the liability to rupture certainly seems to have been diminished, but it should be remembered that the cervical incision is seldom employed in comparison with that in the uterine body. An additional advantage which appeals to me in the cervical operation is that adhesions will be less apt to form. On the other hand it is an operation for the specialist rather than the general practitioner, and this view should not be lost sight of as small maternity homes staffed by practitioners are being opened in all parts of the country. During extraction of the head I have known alarming hæmorrhage result from extensive tearing of the tissues where a transverse incision had been made. The transverse wound should also be avoided because the muscular fibres in this area are for the most part arranged longitudinally. Another objection to the low incision is that operation must be delayed until the patient is well on in labour so as to permit sufficient stretching of the lower segment.

Although compression and massage of the uterus between hot swabs is often practised in the course of the Cæsarean operation, this manœuvre was not adopted in any of the cases in this series.

As soon as the wound in the uterus is closed the four large swabs are withdrawn and the abdominal incision is united in layers. For many years I have used anchor sutures to support the wound and to keep the small gauze dressing in position. No other covering is applied to the wound. Before the patient leaves the operating table the surgeon should observe whether blood is escaping from the vagina or not; if it is not he should grasp the uterus and compress it through the abdominal wall. Should a trickle of blood fail to appear, the probability is that a retained portion of membrane is occluding the os. The external parts should therefore be bathed with an antiseptic lotion and afterwards the vaginal walls should be swabbed with a similar solution which is carried into the vagina on gauze attached to a sponge holder. The gloved fingers are next passed into the vagina and the index finger is forced up the cervical canal to break down the obstruction. In a few instances the cervical canal may be so narrow and rigid that the cervix has to be seized with volsella in order that a few Hegar's dilators may be passed, but it is almost unnecessary to state that the less frequently such manœuvres are required and adopted, the less likely will sepsis ensue. In my surgical practice I have for many years allowed my patients every liberty of movement as soon as they become conscious after the operation. They are encouraged to lie on their side and sit upright in bed in Fowler's position. By so doing drainage is promoted, flatulence is diminished, convalescence is shortened and the liability to thrombosis is lessened. In conclusion I may state that the only other death, which I have had in my Cæsarean sections in recent years occurred in a case which was complicated by pronounced toxæmia and placenta prævia. Death was due to suppression of urine. My impression is that Cæsarean section should never be performed in toxæmia of pregnancy.

A Note on Two Cases of Cæsarean Section under Spinal Anæsthesia with Tropacocaine.

By BECKWITH WHITEHOUSE, M.S., F.R.C.S., and
HENRY FEATHERSTONE, M.B.

IN March, 1914, Dr. J. D. Barris [5] recorded before this Section a successful case of Cæsarean section under spinal anæsthesia with stovaine, the indication being pregnancy complicated by severe cardiac disease. The author of this communication was able to collect records of three other instances in which spinal anæsthesia had been used in this country for Cæsarean section, by Walls [4], Fairbairn and Stabb. The literature, however, both here and abroad, is scanty and within the past ten years but five references directly bearing upon the subject can be traced.

At a meeting of the Section of Anæsthetics on November 3, 1922,¹ in which Members of this Section took part, the anæsthetization of patients for classical Cæsarean section was under discussion. Again but a brief reference was made to spinal anæsthesia. Dr. Herbert Spencer, in his opening remarks, stated that "spinal anæsthesia is undoubtedly more dangerous than inhalation anæsthesia, and has drawbacks from which inhalation is free." Dr. Hadfield had found, however, that "in the half dozen or so cases in which he had used spinal anæsthesia, the results had all been most satisfactory."

Two instances recently occurred in the Obstetric Department of the General Hospital, Birmingham, in which the Cæsarean operation was performed under spinal anæsthesia with entirely favourable results. In fact, certain advantages attached to this method so impressed us that we have ventured to place the facts before this Section.

CASE I.

A. F., a 2-para, aged 29, was admitted to the General Hospital on October 11, 1922. She gave a past history of chronic nephritis, and each of her previous pregnancies had been complicated by generalized œdema. Pregnancy dated from about March 17, and its progress had been marked by progressive wasting, headache, vomiting, œdema of the lower extremities, and much pruritus and swelling of the vulva. The patient was much emaciated on admission and the vulva presented the typical lesions of diabetic vulvitis. The urine contained much albumin, numerous epithelial "casts," acetone, diacetic acid and glucose, 1·3 per cent. The urinary diastase number was 10.

The patient was placed under the care of one of our medical colleagues, Dr. A. P. Thomson, and at first under a rigid dietetic régime improvement was noted, both in the general condition and also as shown by the renal function tests. The glucose content of the blood, however, showed no improvement, and on November 17 Dr. Thomson advised termination of the pregnancy. The percentage of blood-sugar on this date was 0·2 per cent. one hour after food.

Cæsarean section was performed on November 18, the abdominal route being selected owing to the risk of sepsis attached to induction; the vulva and perineum being the seat of much solid œdema and furunculosis. Spinal

¹ *Ibid*, 1922-23, xvi (Sect. Anæsth.), pp. 1-4.

anæsthesia was employed in order to avoid the possibility of degeneration of the hepatic cells from inhalation narcosis. The operation taking place one month before term, a further indication existed in the interests of the child. Half an hour before operation a hypodermic injection of morphia $\frac{1}{4}$ gr. was administered. In the anæsthetic room the patient was turned on to her left side and requested to flex her neck and her hips. This is a more convenient position for these cases than the sitting posture. Barker's spinal needle was introduced between the second and third lumbar vertebræ, care being taken to see by the flow of cerebro-spinal fluid that the spinal theca was actually entered; 1.5 c.c. of a 5 per cent. solution of tropacocaine (Allen and Hanbury) dissolved in normal saline solution was drawn into the syringe, and after about 10 c.c. of cerebro-spinal fluid had escaped from the needle the syringe was connected and the piston withdrawn very slightly to ensure that the point of the needle had not been inadvertently displaced. The contents of the syringe were slowly injected, the needle withdrawn, and the puncture wound covered with collodion. The patient was laid on her back with the head on a pillow and the body inclined, shoulders slightly downwards. Full anæsthesia was present in five minutes. A screen was interposed between the patient's face and the seat of operation, and steps were taken to cut off all visual and auditory sensations. The operation was completed in twenty minutes and no inhalation either of a narcotic or oxygen was required throughout.

During the operation three points of interest call for special mention:—

(1) *The Condition of the Child.*—The infant was very vigorous, cried lustily as soon as extracted, and presented a very different appearance from that often seen when inhalation narcosis is employed.

(2) *The Tone of the Uterus.*—This was very evident from the moment of incision into the organ and even before. As soon as the child was extracted the uterus contracted firmly and partially expelled the placenta through the uterine incision.

(3) *The bloodlessness of the whole operation*, owing, of course, to the tone of the uterus. Only four dry abdominal mops were used throughout.

There was no vomiting, and the after-history of the case was uneventful. The pulse-rate of the patient on leaving the theatre registered 104. Within half an hour it fell to 80, and subsequently varied between this rate and 96. Sensation returned slightly within one hour, and in two hours the patient complained of abdominal pain. This apparently was never severe. No serious hæmorrhage occurred from the placental site, and both pituitrin and ergot were withheld throughout for purposes of observation.

The following day some flatulent distension of the abdomen was present, but this was relieved by the ordinary nursing measures. Urine was passed naturally at the end of twenty-four hours after operation. The patient was somewhat drowsy for the first few days, and slept a good part of each twenty-four hours. By November 30, however, glucose had disappeared from the urine, and only a trace of albumin was present. She was discharged to a convalescent home on December 8, 1922, and we have been recently informed that both she and her baby are in good health.

CASE II.

R. L., a 2-para, aged 29, was admitted to the obstetric department of the General Hospital, Birmingham, on November 9, 1922, complaining of ante-partum hæmorrhage. There was doubt as to the exact period of pregnancy, as

the patient's last menstruation occurred at the beginning of May, 1922, and on admission the size of her uterus corresponded to a gestation of seven months. In June, hæmorrhage occurred with the passage of some clots, and, with slight intermissions, bleeding had continued irregularly until the date of admission to hospital. Periodical attacks of uterine pain from August onward had suggested that premature labour might be imminent, but with the aid of absolute rest and hypodermic injections of morphia the pregnancy continued.

On admission, the height of the fundus uteri above the pubes measured 12 in. The foetus occupied a right occipito-posterior position, and on vaginal examination the lower uterine segment appeared to be filled with an inelastic swelling. No foetal part could be palpated *per vaginam*. The foetal heart was audible in the right lumbar region. A diagnosis of placenta prævia was made, and the patient confined to bed until it was thought that the foetus was viable. For reasons which need not be discussed on this occasion, delivery by Cæsarean section was decided upon. Further, in the interests of the premature foetus, spinal anæsthesia was selected.

Operation: The operation was performed on December 21, 1922, at 9.30 a.m., the same technique being employed as in the previous case. The spinal theca was tapped, and 1.5 c.c. of a 5 per cent. solution of tropacocaine introduced through the first lumbar space. Anæsthesia had extended to the middle of the sternum in five minutes. The operation followed the usual lines and the patient left the theatre under half an hour.

Before the operation the systolic blood-pressure was 98, and the diastolic 75 mm. Hg, with pulse-rate of 100. Upon the patient's return to the ward the systolic blood-pressure was still 98, the diastolic having fallen to 70 mm. The pulse-rate was now only 88.

The patient herself was very much pleased at having the operation carried out "without an anæsthetic," as, in her own words, she "had been dreading it so." Apparently she did not object to the technique required by the spinal method.

After the extraction of the child she expressed a desire for a drink, and swallowed a cup of hot black coffee. As in the previous case, the baby showed no signs of cyanosis, and progressed very satisfactorily from the moment of birth. No artificial stimulation was required. Contraction of the uterus was again a feature of the case, and made the actual operation almost bloodless. Owing to the central position of the placenta over the os internum and the rapid contraction and retraction of the uterus a slight difficulty was met with in delivering the placenta.

The subsequent history of the patient differed but slightly from the first case. Vomiting (2 oz.) occurred fifteen minutes after her taking the cup of coffee, and again (4 oz.) after taking a cup of weak tea in the afternoon following the operation. Apart from these occasions no vomiting took place. Sensation returned at the end of one hour, and the presence of somewhat severe abdominal pain called for an injection of morphia ($\frac{1}{4}$ gr.). The lochia were more profuse in this case than in the preceding, but were never alarming. No pituitary extract was used, but ergot was administered hypodermically four hours after operation and subsequently in mixture. The increased hæmorrhage occurred after the effect of the tropacocaine had passed off, and was attributed to the low placental site. Urine was voided naturally five and a half hours after anæsthesia. Flatulent abdominal distension was not a feature of this case. The patient made a non-febrile convalescence, and was discharged to a convalescent home on January 8, 1923.

The two cases here recorded tend to confirm the previous observations of Polano [1], Barris [5], Walls [4], Wyatt [3], Marsh [6] and others in proving that Cæsarean section under spinal anæsthesia is a safe and reliable proceeding both to mother and child. Further, under certain conditions, e.g., morbus cordis, diabetes, &c., it appears to offer special advantages to the mother over inhalation narcosis. As to the effect upon the child, in our experience there is no comparison between the results of spinal and inhalation anæsthesia, however little chloroform or ether are employed.

To effect a surgical degree of anæsthesia sufficient for the purpose of abdominal Cæsarean section, an amount of volatile narcotic must be absorbed which may prove deleterious to a premature infant. In cases of Cæsarean section performed *before "term"* we venture to suggest, therefore, that spinal anæsthesia may find an application in the interests of the child, without increasing the mortality or morbidity of the mother.

Concerning the disadvantages of the method, they are those which have been put forward with regard to spinal anæsthesia in general, and are not influenced by the more pressing obstetric indications. Certainly looking through the small number of cases hitherto recorded, they appear to be more theoretical than real. At the same time, until more experience has accumulated, we think that the method should be confined to institutional practice.

In conclusion, may we draw attention once more to the increased tone of the uterine muscle under spinal anæsthesia with tropacocaine? In all the recorded cases it is noted that the uterus contracted well, and certainly in both our patients contraction and retraction were in excess of normal. What is the cause of this phenomenon? In labour occurring in paralytics, as also in bitches after experimental division or destruction of the lumbar cord, the expulsive power of the uterus is undiminished, in fact, labour is stated to be rapid under such conditions. Such facts appear to point to the thesis that the uterus is under the control of both vago-tropic and sympathico-tropic fibres, and that when inhibitory impulses from the lumbar cord are cut off the sympathetic tonic stimuli have full play. If such is the case, may not a remedy for the atonic uterus lie in the intrathecal injection of stovaine or tropacocaine?

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DISCUSSION.

Mr. FARDLEY HOLLAND said there was no doubt that Dr. Cameron's results were outstanding, for his remarkable mortality of under 1 per cent. for such a long series of cases was only about half the average mortality as revealed by published statistics. His own careful technique and the admirable conditions at the Glasgow Maternity Hospital were contributory factors in his achievement, but there seemed no doubt that the most important factor was his careful selection of cases. He took no risks as regards the mother, and the number of craniotomies in cases under his care must be very high; the question seemed to be—how many babies equal one mother? Mr. Beckwith Whitehouse had discovered the virtues of spinal anæsthesia for Cæsarean

section somewhat late in the day, as it must have been used many times during the past few years by most obstetric surgeons. He (Mr. Holland) had personally used it in about fifteen cases in most of which there was a special indication, such as eclampsia, cardiac disease, concealed accidental hæmorrhage or Graves' disease; but in two cases the method had been used for ordinary cases of contracted pelvis at the patients' own request. The effect on the uterus was indeed remarkable, for it remained in a state of intense contraction and anæmia, as described by Mr. Whitehouse. This was a great advantage for the closure of the uterine incision, for the thicker the walls of the uterus at the time of the suture, the more muscle was gripped by the suture, and the stronger would be the resulting scar. Further, if the sides of the incision were drawn into apposition when the uterus was contracted, how much more would they remain in apposition during relaxation, when the volume of the uterus became more bulky and the sides of the incision tended to become relatively expanded?

Mr. MILES PHILLIPS said he had greatly appreciated Dr. Cameron's paper, chiefly for the large number of technical points it explained: some of these were new to him, others he had already found to be of value. Dr. Cameron's wonderfully low mortality, he thought, was chiefly due to the wise rules he had formulated for the selection of cases suitable for Cæsarean section. The frequency of pulmonary complications in these rickety patients had been greatly lessened, in his experience, by the use of warmed ether. With regard to spinal anæsthesia he would ask Mr. Whitehouse whether there was likely to be any special difficulty in inserting the needle in patients much deformed by rickets. When he had failed to remove the membranes satisfactorily from the lower segment of the uterus, he pushed a swab, soaked in antiseptic lotion, from above into the vagina. He had also on several occasions been faced with the difficult task of tracking and removing silk sutures (always thick twisted silk) which had wandered from the uterine wall and were ulcerating into abdominal viscera. Personally, he used catgut, and he always waited until the uterus had retracted and its wall had become as thick as possible before beginning to insert the sutures.

Dr. J. D. BARRIS congratulated Dr. Cameron on his successful result. He had adopted many of his methods and had found these of great value. He had never had occasion nor had he felt it necessary to employ the instrument shown by Mr. Beckwith Whitehouse for dilating the cervical canal. Although he now performed Cæsarean section without waiting for the onset of labour he found that the cervical canal dilated well and allowed for drainage. He thought Cæsarean section under spinal anæsthesia a good method under certain conditions such as severe morbis cordis, diabetes, and pulmonary complications; the uterus retracted well, hæmorrhage was not great, and the child was born crying. But he took a modified view as to the use as a routine measure of spinal anæsthesia for Cæsarean section. It differed from spinal anæsthesia as employed for other operations from this fact that morphia or scopolamine were excluded for the sake of the child. In routine cases he preferred to perform the operation under a general anæsthetic and he found that gas and oxygen anæsthesia, with a little ether, gave satisfactory results.

Dr. HERBERT SPENCER said he thought it was regrettable that selected series of cases of Cæsarean section should be published without stating the whole of the authors' experience of the operation. Dr. Amand Routh's paper¹ was of the greatest value as it gave the whole of the experience of the author up to 1910. He hoped that Dr. Samuel Cameron would publish the whole of his experience so that a basis for comparison with the work of others would be available. Professor Bar, of Paris (who used silk ligatures to sew up the uterus), had published a series of ninety-seven selected cases without a death, a better series even than Dr. Cameron's; but he (Dr. Spencer) thought that both these obstetricians made such a selection as must lead to the unnecessary sacrifice of many children's lives. He (Dr. Spencer) saw no reason for refusing to perform Cæsarean section when the patient had been examined and the membranes ruptured for twelve hours. He had often operated under these circumstances and even after the application of forceps, and so far had only lost one case of Cæsarean section for

¹ *Proceedings*, 1911, iv (Sect. Obst. and Gyn.), p. 161.

contracted pelvis, a case already infected at the time of operation, which should have been dealt with by total abdominal hysterectomy—an alternative to craniotomy not mentioned by Dr. Cameron.

With regard to technique: he (Dr. Spencer) had published a case in which the uterus had been inverted to separate the membranes; but he did not think this necessary or advisable: it did not permit the inversion of the internal os where membranes were apt to be adherent: in that case all that was necessary was to perforate the membrane by forceps passed through the cervical canal, no dilatation being necessary. He did not agree with Dr. Cameron's remarks on silk sutures. He had only once had a sinus from silk used to sew up the uterus, and, with regard to adhesions, his experience of repeated Cæsarean sections showed that adhesions did not occur, if the uterus was properly sewn up with silk. He agreed with Dr. Cameron in condemning mattress sutures.

Dr. Cameron had not stated in his paper whether he sterilized any patient after Cæsarean section for contracted pelvis. It would be interesting to know how many craniotomies were necessitated by his method of selection and how many inductions of premature labour had been performed. He (Dr. Spencer) had been supplied by the Obstetric Registrar with the statistics for contracted pelvis at University College Hospital for the last three years—total number of cases of labour, 5,647; induction of premature labour, 113; all the mothers and 101 children surviving: Cæsarean section, 33, all the mothers and 30 children surviving; craniotomies, 4, all the mothers surviving; the operation was done twice for hydrocephalus, twice for dead children; one of the mothers had previously had twelve inductions of premature labour.

Section of Obstetrics and Gynæcology.

President—Dr. T. W. EDEN.

Angioma of the Vaginal Wall.

By HENRY BRIGGS, F.R.C.S.

To this tumour clinical and pathological interest, if not surprise, is attached. It was, without any note of local injury, found on July 28, 1922, in a primigravida, aged 24, at the thirty-sixth week of gestation, partially procident through the vaginal orifice and during the succeeding week under observation its increase was slight but appreciable. There was no abnormal varicosity of vulval or other veins. A sharp line of demarcation of the apparently normal vaginal wall around the base of the reddish-brown tumour is more consistently that of a new growth than of a progressive hæmatoma. There was a reasonable reservation or doubt in diagnosis. Neither an angioma nor a melanotic sarcoma could on available evidence be substantiated and a hæmatoma also was to be doubtfully assumed. As a circumscribed polypoid formation its size, $1\frac{3}{4}$ in. by $\frac{3}{4}$ in. by $\frac{3}{4}$ in., is unusually large for its superficial site within the limited tissue range of the vaginal wall itself. Its free surface, from epithelial erosion, was minutely porous and slightly bleeding. Its sessile base was sharply defined on the normal vaginal wall and the lowest basal edge was $\frac{1}{2}$ in. above the fourchette. In treatment a ring or encircling excision on August 7, 1922, was the most eligible means of securing a rapid primary union with a very trivial sacrifice of normal structure.

The following is Dr. R. A. Hendry's histological report: "The mass is covered by a thinned out and, now incomplete, covering of stratified epithelium—probably surface layer rubbed off during operation, &c. Underneath this, for about half the depth of the "tumour," it is composed chiefly of blood, some in more or less regular areas suggesting grossly distended vessels, the remainder irregularly extravasated. Deep to this is a mass of loose œdematous (?) tissue containing extravasated blood cells and dilated vessels."

Verification by Professor Beattie was obtained.

The tumour and the microscopical section shown will, I believe, adequately confirm the description given.

Section of Curettings.

Shown by HENRY BRIGGS, F.R.C.S.

A MICROSCOPICAL section of curettings from a moderately abundant group, obtained by an exploratory curettage on August 29, 1922, is under the microscope: beyond excess of interstitial endometrial hæmorrhage—an excess

for which the curette may or may not be alone responsible—there is the welcome negative testimony excluding any new growth in a normally sized uterus of a patient aged 48 with a healthy family of three at ages ranging from 25 to 17 years. Before assuming that a menopausal functional disturbance is the probable explanation the absence of chorionic villi from the section of curettings is also recorded. The curettage was undertaken after an exhausting three-weeks' metrorrhagia following a ten-weeks' suppression. The curettage was followed by a slightly longer period of suppression, this time for eleven weeks and three days. Then on November 17, 1922, an apparently normal period, to begin with, repeated its blood-clots and excessive loss once more. As this possibility had been anticipated and fourteen days earlier a letter of inquiry had been posted, the patient came early for local treatment by a gauze wick saturated with turpentine and lodged within the body of the uterus on November 24, 28, 30, and December 4, a total of four dressings coincident with the gradual arrest of the bleeding.

Three more precautionary dressings were applied on December 6, 13 and January 10.

On February 13 a ten-weeks' menstrual suppression ended in a normal period—February 13 to 16, 1923.

In the choice of local remedies an intra-uterine dressing of turpentine in my experience holds a foremost place in the treatment of hæmorrhage usually attributed to functional causes.

A Mass of Secondary Leiomyosarcoma following Subtotal Hysterectomy.

Shown by A. C. PALMER, F.R.C.S.

M. L., SINGLE woman, aged 60. The menopause had occurred at the age of 55, the periods having previously been normal. For three months the patient had suffered from vague abdominal pains and a feeling of tiredness; for three weeks she had noticed a slight blood-stained vaginal discharge. Ten days before admission to the hospital she consulted her doctor who discovered an abdominal tumour. The patient was of moderate height (5 ft. 6 in.) with a considerable amount of fat in the subcutis of the abdominal wall.

A solid, slightly mobile, lobulated abdominal tumour was found rising out of the pelvis, and reaching well above the umbilicus. The vagina was shrunken. On examination under an anæsthetic, on May 5, 1921, the cervix was found to be atrophic, but otherwise healthy. The original diagnosis of multiple fibroids was thought to be correct, and a sub-total hysterectomy was performed, a portion of the left ovary being preserved. Numerous adhesions were encountered, and the removal of the large lobulated tumour was made somewhat more difficult from the fact that one lobule rising from the upper part of the supravaginal portion of the cervix had grown well out into the left broad ligament.

Recovery was uneventful. The patient was provided with an abdominal belt, and was discharged well.

NAKED-EYE DESCRIPTION OF SPECIMEN REMOVED.

The uterus is greatly distorted by many large masses in its wall, the whole measuring 27 cm. by 21 cm. by 13 cm. The peritoneum, for the most part

smooth, has a few fine fibrous tags attached, and shows numerous areas of dark red, subperitoneal hæmorrhage. The cavity is 8 cm. long, up to 3 cm. wide at fundus, and lined by smooth pink endometrium, up to 0·3 cm. thick. The masses in the wall (up to 14 cm. in diameter) have a bulging, smooth, pinkish-grey and white, whorled cut surface, with, in the case of the largest at the fundus, numerous areas (up to 4 cm. by 2 cm.) flecked with dark red hæmorrhage. The smaller nodules are free from hæmorrhage. A mass (6·5 cm. by 6 cm.) projects from the left side of the uterus. It is devoid of peritoneum; its cut surface is similar to that of the larger nodule and shows many thin-walled vessels.

Histology.—A section taken from the projecting nodule showed the histological appearance of a fibromyoma, while a section from the largest nodule in the fundus showed the appearance of a leiomyosarcoma infiltrating the muscularis of the uterus.

The question of further operation for removal of the cervical stump was considered, and decided against. The patient was not seen again until March 25, 1922, almost eleven months after operation. She came for advice, largely on account of constipation which had been particularly troublesome for ten days. For the first three days of the ten, there had been aching pain in the left iliac region. This had passed, to be followed three days later by similar pain in the epigastric region. During the preceding three months, the patient had noticed her abdomen getting stouter and the abdominal belt had become uncomfortable. On examination, a subcutaneous lump, the size of a tangerine orange, was found in the lower end of the abdominal scar; the left hypochondrium and part of the epigastrium were occupied by a large, firm, somewhat tender, fixed mass. A similar firm, fixed mass filled the lower abdomen.

The patient was again admitted to hospital. She died suddenly from pulmonary embolism, on the morning after admission.

The post-mortem examination was made by Dr. W. W. Woods, Assistant Director of the Pathological Institute of the London Hospital. The following are extracts from the report:—

Pulmonary Embolism and Recurrent Leiomyosarcoma; Old Operation, Subtotal Hysterectomy.—"A well defined rounded mass of sarcoma (17 cm. diameter) attached to the vault of a greatly stretched vagina. Two discrete masses (the larger 8 cm. by 6 cm. by 4 cm.) infiltrating the peritoneum of the iliac colon. A discrete nodule (3 cm. in diameter) in the cave of Retzius; a discrete hard nodule (5 cm. diameter) in the subcutis of the abdominal wall, in the lower end of the scar. A lobulated discrete mass (25 cm. by 18 cm. by 10 cm.) in the parietal peritoneum of the anterior abdominal wall at the umbilicus, projecting into the left hypochondrium and epigastrium, and attached to the omentum. No secondaries in soft inguinal, iliac or lumbar glands. Red marrow in the lumbar spine. Slight distension of large intestines. Great dilatation of urinary bladder, with injection of its mucosa, in the lower part of posterior wall where stretched over and fused with the growth. The lower end of the right ureter completely surrounded by growth; great distension of right ureter (4 cm. circumference) above encircling growth, and hydronephrosis (size of an orange), with considerable fibrotic atrophy of right kidney. Moderate hydronephrosis of left kidney. Five masses of externally laminated clot (up to 8 cm. by 1·5 cm.) in bifurcation of, and in main trunks of, pulmonary artery. Fluid blood in femoral and iliac veins. Slight œdema of emphysematous lungs."

Description of Specimen after Removal.—The specimen weighs 11 lb. 11½ oz. and consists of the secondary growth, pelvic organs, omentum and part of the abdominal wall; it has been divided into two parts. One part consists of the lobulated mass (25 cm. by 18 cm. by 10 cm.) attached to the abdominal wall at the umbilicus, and adherent to the great omentum. Its cut surface shows large areas of opaque, spongy hæmorrhagic necrotic tissue; in other areas it is white, cedematous, slightly whorled, and shows numerous small cystic spaces. In the omentum are a few nodules (up to 4 cm.) with a spongy cut surface. In the other part, the stretched vagina has been laid open; adherent to its upper part is a mass (17 cm. diameter), the cut surface of which is swollen, white, "rubbery" in some areas; in others the tissue is opaque, spongy and infiltrated with blood. The bladder is adherent to, and spread out over, the mass. The left ureter passes through most of the mass. The cervix is represented by a dimple in the vault of the vagina, and a probe entering the dimple passes up the stretched cervical canal in the centre of the mass of growth. The nodule (8 cm. diameter) infiltrating the peritoneum of the iliac colon; the nodule (5 cm. diameter) in the subcutis of the abdominal wall near the scar, and that (3 cm. diameter) in the muscles of the abdomen, are hard; their cut surface is white and has a distinctly whorled appearance. Sections from the two large masses, and the three smaller nodules mentioned, show the histological appearance of leiomyosarcoma. Sections of six iliac, inguinal and lumbar glands show congestion and œdema only.

A Leiomyosarcoma of a Fibromyoma removed by Subtotal Hysterectomy.

Shown by EARDLEY HOLLAND, F.R.C.S.

THIS case is similar to that just reported by Mr. Palmer in that the uterus was removed by subtotal hysterectomy for a fibromyoma which subsequent examination showed to be undergoing sarcomatous changes. In the case of Mr. Palmer's patient the sarcoma recurred in eleven months; in my own case the operation was performed only four months ago and the patient, so far shows no sign of recurrence. The patient is aged 42, and has two children, the younger 10 years old; no miscarriages; menstruation regular and not excessive; no abnormal vaginal discharge. The chief symptom was difficulty with micturition for the past two months, and a few weeks ago there was retention of urine, for the relief of which a catheter was necessary. Examination showed the uterus enlarged to the size of an eighteen weeks' pregnancy and of a semicystic consistence. A diagnosis was made of fibromyoma, probably undergoing cystic changes, and the uterus was removed on October 30, 1922, by subtotal hysterectomy. The patient made a smooth recovery and when examined a fortnight ago presented no signs or symptoms of recurrence.

The specimen consists of the body of the uterus and a single cystic tumour with thick walls, both together forming an ovoid mass about 13 cm. by 11 cm. by 11 cm. in size. The tumour has originated from the anterior surface and left border of the uterus. Bisection of the tumour reveals an irregular cystic space 6 cm. by 5 cm. lined by soft, smooth, yellow and pink tissue. The uterine muscle can be traced entirely covering the tumour, although in places too thin for measurement. The encapsulating muscle is everywhere easily separable

from the tumour, the wall of which has, for the greater part, a whorled appearance, and is composed of dense white tissue, save for numerous cystic spaces which are scattered throughout, some of which are lined by pale yellow tissue similar to that described above. In the uterine wall is a small seedling fibroid. The endometrium is smooth and pink. Microscopic sections were taken from four areas of the wall of the tumour, and all proved to be leiomyosarcoma.

About ten days after the operation, when I received the report on the specimen from the Pathological Institute at the London Hospital, I considered whether it would be wise to re-operate for removal of the cervical stump; but I decided against this course, arguing that if the sarcoma cells had permeated as far as the cervix they would also have permeated the blood-vessels and tissues of the broad ligament and beyond.

DISCUSSION.

Dr. G. F. BLACKER, discussing Mr. Eardley Holland's specimen, said he thought that in any case in which subtotal hysterectomy was practised the tumour should be opened before the operation was completed and that if the tumour showed signs of breaking down, other than simple cystic degeneration, the cervix should be removed.

Mr. L. C. RIVETT mentioned a case of cystic fibroid, containing one large cyst weighing 14 lb., removed by operation at the Chelsea Hospital for Women.

Mr. L. PROVIS thought that if the growth in Mr. Palmer's case had extended into the broad ligament there would be no point in doing a total hysterectomy.

Dr. H. RUSSELL ANDREWS said that his own experience of sarcoma of the uterus was a gloomy one, the growth usually recurring within a short time of the operation. In two cases at least secondary growths had been present when the patient was first seen. He did not agree with Dr. Blacker that cystic change in a fibroid was usually due to sarcoma. In several cases in which he had removed large cystic tumours from the uterus the microscope had shown no evidence of malignant disease and the patients remained well.

Professor HENRY BRIGGS agreed with the experience of the previous speakers; sarcoma of the uterus recurred so speedily after the widest possible operations that he thought Mr. Eardley Holland had adopted the most correct reasoning in his case. In a few non-recurrent cases Professor Briggs believed that a cellular fibroma and not a sarcoma was the truer description of the tumour: malignancy was not always frank in its histology.

Two Specimens of Sarcoma of the Uterus.

Shown by J. D. BARRIS, F.R.C.S.

CASE I.

THE specimen is composed of the uterus with its appendages together with the vagina, bladder and rectum, which have been displayed by sagittal section and the left half preserved.

The interior of the uterus in the recent state was occupied by a vascular, spongy, friable growth, the greatest length of which measured 12.5 cm. and the greatest width 10 cm. In its upper and anterior portions the growth can be seen to have invaded the uterine muscle, which is thinned and nowhere measured more than 0.6 cm. in thickness; the invasion has in some places reached the peritoneum but has not penetrated it. In its lower portion the growth is more vascular, is necrotic and hangs down as a tongue-like process,

the tip of which reaches to the level of the os internum. The cervix appears healthy and the cervical canal is not dilated. The outer surface of the uterus was in the recent state pink in colour, smooth in outline and covered by numerous recent adhesions.

Secondary masses appear in three situations:—

(a) In the anterior vaginal wall, separate from the cervix and completely surrounding the urethra, through which a probe has been passed, there is a whitish, firm, rounded nodule, which measures 5 cm. in length and 3.75 cm. in width. The mass projects into the vagina but the vaginal mucosa appears healthy. The bladder is much dilated and its walls are fasciculated owing to the retention of urine due to the urethral growth.

(b) In the rectovaginal septum there is another mass similar in appearance, which measures 2.5 cm. by 2.5 cm. Neither the mucous membrane of the rectum nor that of the vagina is involved.

(c) In the position of the perineal body there is a third mass again of similar appearance but considerably smaller, being about the size of a pea.

Microscopic Report.—Microscopic sections have been prepared from the uterine growth, the vaginal walls and the secondary masses. The walls of the vagina between the growths did not show any evidence of invasion by malignant tissue.

I have shown the sections to Sir Bernard Spilsbury and he has kindly furnished me with the following opinion: "The sections show the presence of a malignant growth having the characters of a sarcoma. The tumour consists of masses of cells with fibrous and muscular septa separating the masses. Most of the tumour cells are spherical or oval and show some differences in size, being on the average rather larger than the cells of a small round-celled sarcoma. Where the cells have a loose arrangement they are seen to have fine cytoplasmic processes which connect neighbouring cells; there are also elongated and fusiform cells and a few larger cells having the characters of small giant-cells. Mitotic nuclear figures in fairly large numbers point to a rapidly growing tumour. There are areas of degeneration and necrosis in the centres of the larger masses of tumour cells, with hæmorrhage into necrotic areas. The tumour infiltrates widely the uterine wall and masses of the tumour cells extend along small veins. The tumour is a polymorphic-celled sarcoma and the character of the cells and their arrangement suggest a sarcoma of endothelial origin."

The specimen was obtained from a multipara, aged 57, in whom the menopause had occurred six years previously. When first seen by me she had complained of profuse vaginal hæmorrhages for five weeks, together with pelvic pain and frequency and difficulty in micturition. The condition was judged to be inoperable. She died six weeks later with symptoms of acute retention of urine and uræmia.

Dr. Kidman Bird, who asked me to see the patient with him, took so great an interest in the case that he obtained permission to remove the pelvic contents after death. I would like to record my sense of obligation to Dr. Kidman Bird for affording me the opportunity of obtaining so interesting and so valuable a specimen.

CASE II.

The specimen consists of the left half of the uterus laid open by sagittal section. The cavity of the uterus contained a growth very similar in size and appearance to that described in the specimen just shown, but no secondary deposit was found.

The microscopic sections have been preserved and show the growth to be a sarcoma.

Section (a) was taken from the point at which the tumour was attached to the uterine wall, and shows the structure of a spindle-celled sarcoma invading the muscle of the uterine wall. Many mitotic figures are seen, and they suggest a considerable degree of malignancy.

Section (b) was taken from the part of the tumour projecting into the uterine cavity, and shows on the surface an organizing blood-clot, deep to which is granulation tissue, and beneath this sarcoma cells, which in places have become necrotic.

The chief interest, however, lies in the clinical history. The specimen was obtained by panhysterectomy from a multiparous married woman, aged 67, in whom the menopause had occurred at the age of 50. She had not complained of any symptoms except of profuse vaginal hæmorrhage for three weeks only.

At the operation precautions were taken to prevent any portions of growth from passing out of the cervical canal. It was not possible to sew up the cervix, as the vagina was too narrow to permit of the necessary manipulations. But the uterus was not handled directly, traction being made on the broad ligaments only; a cuff of vagina was made over the cervix as in Wertheim's operation; the vagina was divided between two clamps, and before removal of the lower clamp the vagina was swabbed out. The patient made an uninterrupted recovery.

In spite, however, of the precautions taken at the time of the operation, bleeding recurred within two and a half months, and a red, spongy, friable growth was found at the roof of the vagina. She died within three and a half months of the operation from hæmorrhage and cachexia.

The vagina was then almost full of growth, which could be felt extending into the pelvis, but there were no secondary masses in the vaginal walls as were present in the first specimen.

I bring forward these two cases to-night because they possess certain striking features. The chief interest attaching to Case I lies in the position and character of the secondary deposits. Case II illustrates the extreme malignancy of these tumours.

It is remarkable that in both cases the duration of the initial symptom, i.e., vaginal bleeding, was short; being in the first five weeks and in the second three weeks only, and yet in both cases the growth had by that time already assumed a large size.

Chorion-epithelioma of the Uterus showing a very Extensive Growth in the Uterine Wall.

Shown by S. GORDON LUKER, M.D.

THE specimen was removed post mortem from a nulliparous woman, aged 30, who died in the London Hospital, September 28, 1922, of hæmorrhage from chorion-epithelioma of the uterus.

The history is as follows: From January to June, 1921, she had irregular bleeding, sometimes daily, and in June was sent into Walthamstow General Hospital for curettage for miscarriage, but no further details can be obtained. After this she was irregular till February, 1922, when she was unwell for one

month, and was irregular again until July, 1922. She then had amenorrhœa until September 27, when uterine hæmorrhage occurred, which resulted in her death. Abdominal pain was present during the last three or four months; it was almost constant, but not very severe. There was a history of cough at times, with streaks of blood in the sputum for a few months.

She was sent to see me on September 18. She looked pale and ill, and complained of abdominal pain. The right breast was just active. On abdominal examination there was a median elastic swelling rising out of the pelvis to within two finger-breadths of the umbilicus, rather tender on the left side. A loud uterine souffle was heard, but there were no foetal heart, foetal movements, nor ballottement. On vaginal examination the cervix was found to be greatly enlarged and expanded and slightly softened, as if some tumour was being expelled from the uterine cavity, but the external os could not be felt nor seen. The abdominal tumour was felt to be an enlarged uterus.

She was admitted to the London Hospital a few days later for further investigation. She then showed symptoms and signs suggesting pneumonia, which prevented any surgical measures.

On September 27 she had a very severe hæmorrhage of several pints from the vagina, and was treated by a hot douche and vaginal plugging and other treatment for shock. Next morning she was a little better, and I made an exploratory laparotomy. I found a very extensive hæmorrhagic growth of the uterus and pelvic organs bound down by adhesions. The condition was inoperable, and the abdomen was closed.

Post-mortem examination showed an extensive primary chorion-epithelioma of the uterus, which had perforated the right lateral fornix of the vagina, from which place fatal hæmorrhage had occurred. The body of the uterus was perforated in three places by the growth. The right ovary and broad ligament were invaded by hæmorrhagic growth. The uterine cavity was free from growth, showing a smooth endometrial lining—a very unusual condition. Secondary nodules were found in both lungs, the left suprarenal body and the ileum.

A complete summary of the necropsy by Professor H. M. Turnbull, of the Pathological Institute, the London Hospital, is appended. Sections from the uterus and ovary and secondary growths are typical.

SUMMARY OF NECROPSY, 1922.

P. N., aged 30. Died September 28; necropsy, September 29, 1922. Traumatic anæmia. Hæmoperitoneum ($1\frac{1}{2}$ pints). Hæmorrhage from chorion-carcinoma of uterus. Recent operation: Exploratory laparotomy and removal of portion of uterus for microscopic examination.

Lobulated, brown, red and purple, partly necrosed and cavitated, hæmorrhagic mass (15 cm. by 8 cm. by 8 cm.) of chorion-carcinoma, replacing the myometrium of the right side of the body and cervix of the uterus, and greatly elongating the uterus (14.5 cm. by 10 cm. by 6.5 cm.) Small hæmorrhage beneath, and in the substance of, the endometrium of the fundus; remainder of endometrium smooth, œdematous and white. Smooth mucosa in cervical canal (7.5 cm. long). Viscid mucus plugging cervical canal. Large perforation by carcinoma through the right vaginal fornix. Direct infiltration by uterine carcinoma of surface of adherent right ovary, forming hæmorrhagic mass (2.5 cm. diameter). Fibrotic corpus luteum in left ovary; left ovary a mass of thin-walled follicular cysts (from 2 cm. to 4 cm. diameter) with hypertrophied

thecæ internæ. Secondary hæmorrhagic nodule (3.5 cm. diameter) in right broad ligament. Direct extension of carcinoma from uterus into urinary bladder, to form submucous nodules (up to 1 cm. diameter) in trigone. Very numerous hæmorrhagic secondaries (averaging 1 cm. diameter, occasionally 3 cm. diameter) throughout both lungs, most numerous in lower lobes. Extension, surrounded by fibrous pleural adhesions, of one nodule through pleura of diaphragmatic surface of left lung. Blood-clot in right main bronchus. Diffuse hæmorrhagic secondary growth (4 cm. diameter) destroying greater part of left suprarenal body. Hæmorrhagic secondary (0.25 cm. diameter) in submucosa of upper ileum. No secondaries in lymphatic glands.

Anæmia of kidneys and liver. Œdema and anthracosis of spleen. Mucous catarrh of stomach. Milk spots on heart. Slight atheroma. Large fibrous breasts, with considerable glandular tissue. Great anthracosis of bronchial glands. Scar (3 cm. diameter) in right groin. Slight, wasted woman.

The points of interest in this case are as follows: (1) The chief symptom was abdominal pain with amenorrhœa for the last three months; (2) the uterus was very much enlarged, forming an abdominal tumour rising up to within two finger-breadths of the umbilicus; (3) death occurred from hæmorrhage from perforation of the right vaginal fornix by the growth; (4) the size and situation of the primary growth; (5) the extensive distribution of the secondary growths.

The Treatment of Severe and Persistent Uterine Hæmorrhage by Radium, with a Report upon Forty-five Cases.

By SIDNEY FORSDIKE, M.D., F.R.C.S.

SEVERE and persistent uterine hæmorrhage, at any age, is a source of infinite anxiety to both the patient and doctor, and when the doctor has exhausted the long and too often futile list of drugs, together with prolonged periods of rest in bed, he is constrained to recommend curettage. Many of the less severe cases are relieved by this measure, but in the type of case under consideration the relief, if at all, is only measured by weeks and the patient is too soon in the *status quo ante*; and it becomes clear that something more radical must be undertaken.

At or near the menopause the above palliative measures may suffice to pilot a patient through a stormy climacteric into the backwaters of post-menstrual life, only too often, however, at the cost of transforming the patient into a semi-invalid with the late prospect of complete neurasthenia. While this method is possible for the well-to-do patient who can afford to remain in bed, it is not feasible for the wage-earner or the mother of a family, and other means must be adopted to deal with the condition promptly and finally.

The methods advocated for this condition are: (1) Hysterectomy; (2) X-rays; (3) radium; and the analysis of a report upon forty-five cases treated by radium tends to show that it is the treatment of choice when available.

SELECTION OF CASES.

The type of case under consideration is sometimes described as the "bleeding uterus"—a most descriptive term; more commonly it is labelled "chronic metritis," "fibrosis uteri," or "chronic subinvolution." But however debateable the pathology, the one distinctive clinical feature is the excessive

70 Forsdike: *Treatment of Uterine Hæmorrhage by Radium*

and uncontrollable hæmorrhage with absence of any pelvic lesion to account for it. This series of cases does not include the ordinary causes for hæmorrhage, but only cases which had been dealt with in the usual way and proved refractory to treatment. The criteria by which cases were selected were: (a) The persistence of severe hæmorrhage after operative treatment; (b) an advanced anæmia, with shortness of breath, headaches, œdema of legs, &c., which had been treated palliatively for some time; (c) cases in which the patient had to spend some part of each month in bed.

Excluding the three cases in which an effort was made to modify the period rather than to stop it, there were forty-two cases, the youngest patient being 29 years and the oldest 55 years of age.

Three only were unmarried, and of the thirty-nine married four only were nulliparæ.

NUMBER AND TYPES OF OPERATIONS.

Of the forty-five cases, twenty-six had undergone some form of operation or combination of operations, including dilatation and curettage, removal of polypi, amputation of cervix, oöphorectomy or salpingo-oöphorectomy, and some of the cases had been curetted more than once. The remaining cases had undergone medical treatment for variable periods, or their state was so anæmic as to forbid any further effort at palliation.

CLINICAL TYPES.

Four cases were associated with fibroids, two of the interstitial variety, and two small subperitoneal fibroids. The results were equally satisfactory, and no change could subsequently be detected in the subperitoneal tumours. While all the uteri were enlarged, they were broadly divisible into two groups which might be best described as (a) systolic uterus; (b) diastolic uterus. The systolic uterus was hard, firm and regular, commonly straight, and admitted a sound for 3 in. to $3\frac{1}{2}$ in.; it yielded little or no tissue to the curette and gave the characteristic rasp of fibrosis uteri.

The diastolic uterus was bulky, retroverted, admitted a sound from $3\frac{1}{2}$ in. to 5 in., and generally yielded a thickened and œdematous tissue. An interesting point about this type of uterus was the fact that some months after irradiation it was indistinguishable from the systolic type.

TECHNIQUE.

In all cases dilatation of the cervix and an exploratory curettage were performed. In cases in which the cervix and vagina were septic a preliminary cleansing treatment was carried out. In all cases the radium was placed in the uterine cavity, and only the gamma ray was utilized. The vagina was packed with gauze moistened with liquid paraffin with the two-fold object of supporting the radium and of keeping the bladder and rectum away from the source of energy. A further precaution was adopted to maintain the bladder in a flaccid condition by the introduction of a self-retaining catheter into the viscus. In some cases it was necessary to stitch the vulva in order to support the vaginal plug.

DOSAGE.

In three cases of patients between the ages of 20 and 26, 50 mg. of radium were used for five hours. In the other cases 100 mg. were used for

twenty-four hours, the object in the former cases being to influence the menstrual period, in the latter cases to end it altogether.

I am inclined to think that 100 mg. is an unnecessarily large quantity for the purpose, for in some later cases 75 mg. appear to have the desired effect.

A certain number of patients complained of pain while the radium was *in utero*, but no more than when a stem is left in the cervix, and it disappeared promptly with the removal of the radium. In a few cases post-anæsthetic vomiting was prolonged and this appeared to be relieved by the withdrawal of the radium. Some of the patients noted a frequency of micturition following irradiation, a condition which was quickly relieved by barley-water, citrate of potash, &c. Beyond the above, no untoward symptoms were noted. In all cases nocturnal douches of salt and water are suggested for a period of six weeks following the radiation.

CLASSIFICATION OF CASES.

A.—Aged 20 to 26, three cases.

B.—Aged 29 to 38, ten cases.

C.—Aged 39 to 50, twenty-two cases.

D.—Aged 51 to 55, ten cases.

Group A will be considered in detail; a selection of cases of other groups is appended.

Case I, aged 20, married, no children. For one and a half years had been losing fourteen to twenty-one days at a time and was only clear from seven to ten days. September, 1921: Dilatation and curettage, which did not relieve her at all, and she attended out-patient department in October, November and December, and it was evident that something more would have to be done. January, 1922: 50 mg. radium introduced into uterine cavity and left there for five hours. Late in January she had a period of four days. February 28: Another period lasting four days. March and April: A more severe period. May: Admitted to another hospital where an ovary was removed. Since then she has been quite well.

Case II, aged 28, Jewess and single. She had been losing daily for seven months and was very anæmic. Had already been in hospital twice for the same condition. (1) The first time for six weeks for observation and palliative treatment. (2) Second time for dilatation and curettage; following this she was better for three months, but then relapsed into the daily loss. She was now thoroughly demoralized, and despite her religion was anxious that hysterectomy should be done. In February, 1921, I dilated the cervix and introduced 50 mg. of radium sulphate into the uterus, leaving it there for five hours. She bled steadily for eleven days, during which time the vagina had to be plugged. For the next six months she saw regularly each month for from four to six days; during that time she was in sole charge of a hemiplegic father, and since then she has had no period at all. This, I think, must be attributed to the close confinement and hard work involved rather than to the influence of radium.

Case III, aged 26, married; no children. Since marriage she had had severe periods with pain for four years, which necessitated a couple of days in bed per month. She was anæmic and had been treated with drugs for a considerable time. January, 1921: 50 mg. radium sulphate placed *in utero* for five hours. Has been regular every month since, three-day type and without symptoms.

Group B.—Aged 29 to 38. There were ten cases, eight of whom had had some form of operation, and two had been treated palliatively and had become steadily worse. Aged 29, one; aged 34, two; aged 35, one; aged 36, one; aged 37, two; aged 38, three.

72 Forsdike: *Treatment of Uterine Hæmorrhage by Radium*

Group C.—Aged 39 to 50, twenty-two cases, eleven of whom had had some form of operation, and eleven had been treated palliatively, but with advancing anæmia and persistent hæmorrhage. Aged 39, six; aged 40, three; aged 41, two; aged 43, one; aged 44, one; aged 46, four; aged 47, one; aged 49, three; aged 50, one.

Group D.—Aged 51 to 55, ten cases, five of whom had had some form of operation and five had been treated palliatively. Aged 51, three; aged 52, one; aged 53, three; aged 55, three.

SYMPTOMS FOLLOWING THE RADIUM.

(1) No further loss in ten; (2) one period in seventeen; (3) two periods in eleven; (4) three periods in four.

The periods usually consisted of long drawn out "shows" varying from three days to three to four weeks, the actual quantity of blood lost being small. In cases where the exposure had been given immediately preceding a period it usually resulted in a severe loss.

In five cases radium had to be employed a second time; in three of them I have little doubt that the first exposure would have sufficed, but the patients were so thoroughly frightened by two prolonged shows following the first exposure that it was considered advisable to comply with their demands. In only two of these five was an anæsthetic necessary.

VASOMOTOR SYMPTOMS.

Flushing as an index of vasomotor disturbance is liable to a very considerable margin of error, and consequently is open to legitimate criticism. Many women will omit mention of this symptom in a carefully taken history, and yet, when it is suggested to them, they will admit experiencing the symptom occasionally; where flushing is a real signal symptom it is mentioned spontaneously. The graver vasomotor changes as signified by a profuse perspiration or a generalized fall in blood-pressure, are never omitted in the history, for they produce considerable distress. Consequently I have adopted the plan of listening to their history patiently, and if they do not mention vasomotor symptoms I have carefully refrained from asking them, and in the after-result it was a fair conclusion that such symptoms did not exist.

Group A.—Aged 20 to 26. There were naturally no symptoms.

Group B.—Aged 29 to 38, ten cases. Eight had no symptoms; two complained of flushes. This was the group in which one would have expected to find vasomotor symptoms on the assumption that the ovary was being affected.

Group C.—Aged 39 to 50, twenty-two cases: nine had no symptoms; four, flushing; three, flushing and sweating; six complained of flushing before treatment, which remained the same afterwards.

Group D.—Aged 51 to 55, ten cases: six suffered from flushes before treatment and remained much the same; four made no mention before or after.

WHAT IS THE CAUSE OF RADIUM MENOPAUSE?

Is it due to action upon the uterus, or the ovary, or both? The available evidence points to action upon the uterus solely in the majority of cases, and is based upon (a) clinical, (b) experimental grounds.

(a) *Clinical Evidence.*—In the younger women aged from 29 to 38 only two out of the ten mentioned flushes, where this symptom should have been pronounced, on the assumption that the ovary was influenced. Again, in the group 39 to 50 the symptom was not unduly increased, and only in three cases was sweating added to flushing. Histological evidence is difficult to obtain, for removal of the uterus following treatment by radium for this condition must be very rare. However, by the courtesy of Dr. Miles Phillips, of Sheffield, I have examined sections from two uteri after exposure to radium, and a section of the ovary from one of the cases. In the uterine sections there was a diminution in the number and size of the glands of the endometrium, and fewer capillaries than normal, but in the ovary there was no change suggestive of irradiation.

(b) *Experimental Evidence.*—The normally placed ovary is 8 cm. away from the uterine cavity, and beyond the effective range of radium, but a prolapsed ovary would be very much nearer and to some extent would account for variable results. The uterus of a cat being bicornute, the conditions under which radium is used clinically can be reproduced by the implantation of the radium in one horn whilst the contralateral ovary is fixed by a stitch at 8 cm. distance from it. The experiment was performed in this way. A pregnant horn was opened and the foetus removed; the radium was then implanted and the horn closed by suture. The ovary of the same side was lightly attached to the wall of the uterus, and the opposite ovary was fixed at a distance of 8 cm. by suture and rubber tube. The radium was removed in twenty-four hours' time. This experiment was repeated, the animals being killed at intervals of twelve, twenty-one, and sixty days. In none of the experiments did the contralateral ovary show any macroscopic or microscopic change. In all the experiments the ovary of the irradiated side shows a profound change, consisting of complete destruction of both large and small follicles. The corpora lutea are broken up, and in the later survivals there are only a few scattered lutein cells recognizable. Vessels are for the most part obliterated, and few remain normal. The uterus on the affected side shows atrophy of the endometrium, a diminution in the number and size of the glands, and a diminution in the number of capillaries. There is no characteristic change in the stroma cells, although they appear to be fewer; but in the later survivals, there are no glands, few capillaries, and a great increase in connective tissue with few stroma cells. [Photo-micrographs were thrown on the screen, see figs. 1-6.]

I then endeavoured to show that the remaining normal ovary was functioning by breeding from a cat which had been exposed in this way, but hitherto that ambition has not been gratified, mainly, I think, because the condition of the animal and its environment tend to depress the sexual instinct.

CONTRA-INDICATIONS.

The only contra-indication is a previous pelvic inflammation, and this is a very real danger, for the pelvis may become filled with an inflammatory tumour rising into the abdomen, the nucleus of which is an abscess deeply seated in the pelvis and nearly impossible to deal with. I have experienced a case of this kind in treating a carcinoma of the cervix, where one had to risk this evil in order to combat the growth.

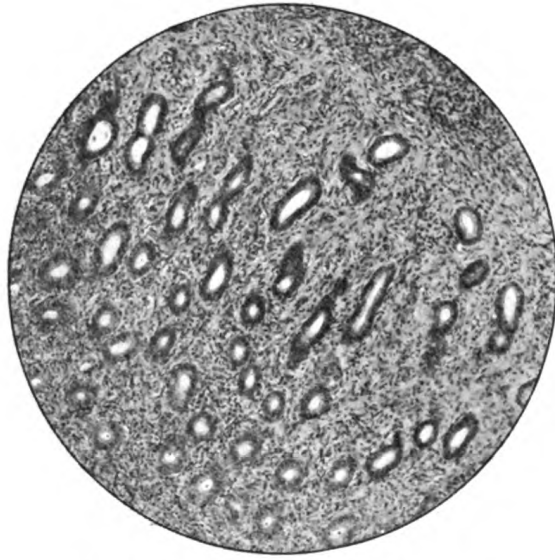


FIG. 1.—Normal endometrium, low power.

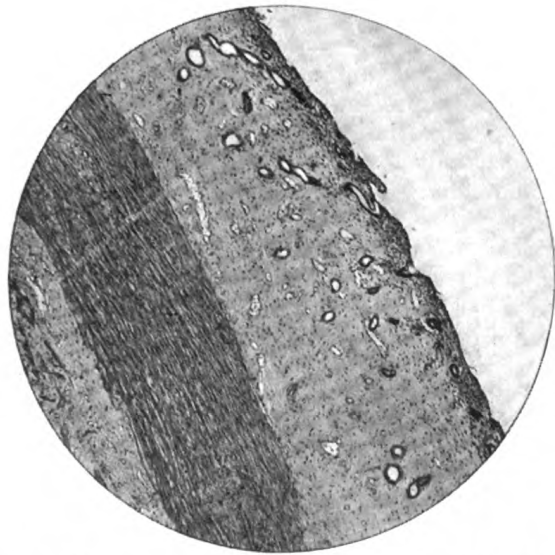


FIG. 2.—Endometrium exposed for twenty-four hours, survival for twelve days. The endometrium is reduced in depth, the glands are reduced in number and size, and capillaries are few.

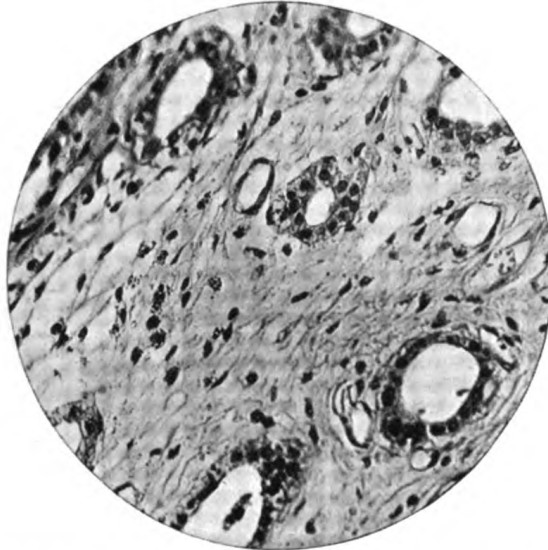


FIG. 3.—The same section under a high power shows some diminution in stroma cells with an increase in connective tissue.

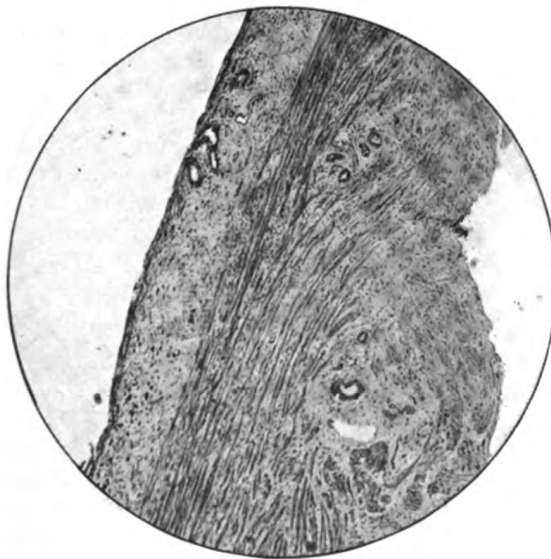


FIG. 4.—The same exposure, survival for twenty-one days. Endometrium shows pronounced atrophy, almost complete absence of glands and blood-vessels, and a poorly staining stroma.

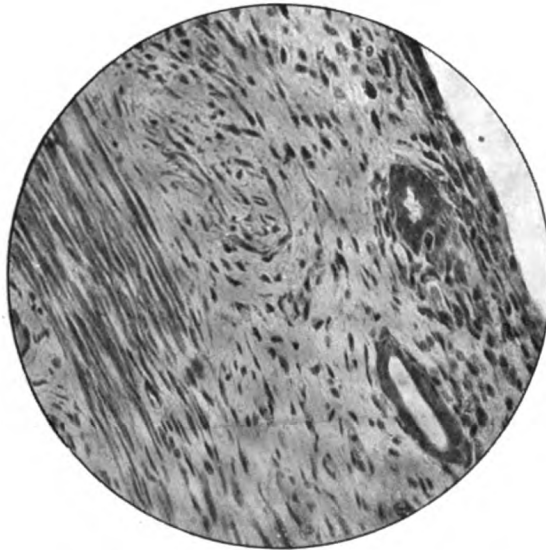


FIG. 5.—The same section under a high power.

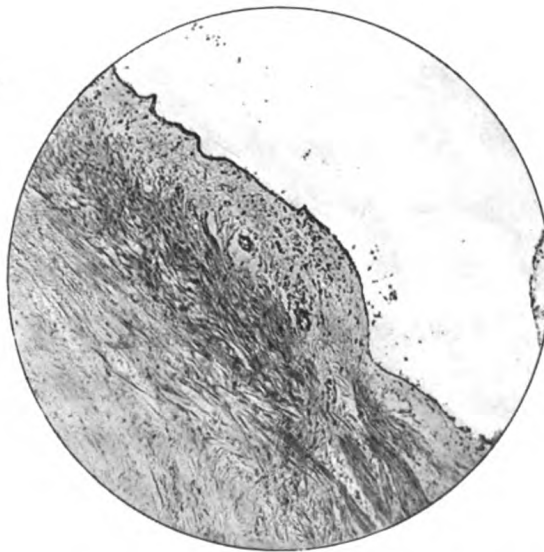


FIG. 6.—Endometrium under the same exposure, survival sixty days. The atrophy is still more marked, with great increase of connective tissue, and paucity of stroma cells.

RELATIVE ADVANTAGES OF RADIUM, X-RAY AND OPERATION.

(a) *Radium* is suitable for all, with the exception of cases complicated by inflammatory disease. There is no mortality and no morbidity, and only the minimum amount of time is necessary for the treatment—from three to seven days. There is no prolonged convalescence, stay in hospital or nursing-home. There were no failures in this series. The one drawback is the comparative scarcity of radium.

(b) *X-Rays*.—The use of X-rays for the treatment of severe uterine hæmorrhage has been fairly successful, but it is accompanied by the following very definite drawbacks: (1) The X-rays are utilized to destroy the ovaries, and their use raises the objection that normal organs are being destroyed in order to influence an abnormal organ, and this is accompanied by a considerable proportion of cases exhibiting severe vasomotor symptoms (38 per cent.: Eden and Provis). (2) The treatment is prolonged, lasting three to four hours on two succeeding days, repeated at intervals of twenty-one days on four to six occasions. (3) There is a danger that the treatment may be used without a diagnostic curettage. (4) Ulceration of the bowel, superficial burns, &c., have been noted. (5) Finally, a certain number of failures have been noted.

(c) *Hysterectomy*, despite its low mortality, is a severe operation and frequently associated with post-operative results which are difficult to assess, e.g., the profound depression and loss of morale not infrequently seen. There is a more or less prolonged period of invalidism following any abdominal operation, and a certain percentage of morbidity as shown by subsequent pain, herniæ, adhesions and constipation. Nevertheless, the operation is satisfactory inasmuch as the bleeding is cured beyond a question, and the only criticism is that the subtotal operation leaves a useless cervix, too often a diseased cervix, and less frequently a cervix which may become the seat of a malignant growth, of which I have met one example this year. The drawbacks of this particular treatment to the wage-earner are the length of stay necessary in hospital, the length of the subsequent convalescence, and the problematical complete restoration of health.

CONCLUSIONS.

(1) In my opinion radium treatment should be the method of choice in all uncomplicated cases of severe and persistent hæmorrhage due to chronic metritis, inflammatory disease of the tubes and ovaries constituting the sole contra-indication.

(2) The radium menopause is not usually accompanied by any symptoms attributable to action upon the ovaries, and it produces the least disturbance of the patient's economic life.

EPITOME OF CASES.

Group A.—Three cases, aged 20, 23, 26.

Group B.—Ten cases, aged 29 to 38. Aged 29, one; aged 34, two; aged 35, one; aged 36, one; aged 37, two; aged 38, three.

Group C.—Twenty-two cases, aged 38 to 50. Aged 39, six; aged 40, three; aged 41, two; aged 43, one; aged 44, one; aged 46, four; aged 47, one; aged 49, three; aged 50, one.

Group D.—Ten cases, aged 51 to 55. Aged 51, three; aged 52, one; aged 53, three; aged 55, three.

Multiparæ, thirty-five; nulliparæ, six; single, four.

78 Forsdike: *Treatment of Uterine Hæmorrhage by Radium*

Periods subsequent to exposure: *Nil*, ten; one, seventeen; two, eleven; three, four.

Vasomotor symptoms: twenty-one had no symptoms; nine complained of flushes after treatment; twelve had flushes before and after treatment.

SELECTION OF CASES.

Case XXXVII, aged 29, multipara 3. Hæmorrhage for seven to eight days every fourteen days, severe loss. Dilatation and curetting, but the hæmorrhage was worse, not being absent for even one day. Had returned to hospital for a hysterectomy, for which she had been prepared. Radium was suggested and given. The uterus admitted a sound for 8 in., and the curette gave the hard rasp of fibrosis uteri. Two prolonged periods followed, the loss, however, being comparatively slight. Then a second exposure of radium, followed by a few days' loss and nothing has been seen since.

Case XXVI, aged 37, multipara 3.—Had been losing excessively for many months, and showed pronounced anæmia. Admitted to hospital; dilatation and curettage January. Got steadily worse after discharge from hospital, and was uninfluenced by drugs, and then a prolonged loss of eight weeks. June: Radium sulphate 100 mg. for twenty-four hours. After leaving hospital she had a prolonged daily loss for seven to eight weeks; then clear for two months, then a further daily loss of a month, made worse by ergot, &c. Second exposure without anæsthesia, 100 mg. radium sulphate. No further loss.

Case XXV, aged 50, multipara 4.—Daily loss with floodings; large part of her time spent in bed. Secondary anæmia pronounced. Uterus hard, regular and firm, a scanty amount of tissue being removed: 100 mg. radium sulphate for twenty-four hours. Two prolonged shows of eight and seventeen days followed, the loss being small. The patient was nervous and insisted upon further treatment. Second exposure given; no further loss occurred.

Case XXXII, aged 34, multipara 1.—Severe flooding for one year nine months, and showed pronounced anæmic symptoms. Admitted to hospital, where dilatation and curettage were done. The relief was very temporary, and two months later she was losing as freely as ever. A period of medicinal treatment was fruitless, and she was getting steadily worse. Six months after the former operation, radium was placed *in utero*. Three periods followed, the patient again getting gradually worse, and so radium was given a second time, and since that time there has been no further loss.

Case XII, aged 51, multipara 6.—Severe floodings and constant loss for many months, which drugs did not influence at all; and she spent most of her time in bed. She was not a good subject for a severe operation, and therefore radium was given a trial. After the first exposure there were two prolonged shows, but the loss was comparatively slight. However, the patient was demoralized, and insisted upon further treatment, and accordingly a second exposure was given. A recent letter from her doctor informs me that she had had no further loss, and she feels well.

Case XIX, aged 49, multipara 2.—Catamenia fourteen to seventeen days, every twenty-one to twenty-eight days. Anæmia. Interstitial fibroid. Had been treated by palliatives for some time, and was no better. Radium followed by one show which lasted twenty-one days. Has seen nothing since.

Case XXXIX, aged 39, single.—Severe and prolonged periods treated by palliatives; no relief. Small interstitial fibroid present; 100 mg. radium sulphate for twenty-four hours. Two shows subsequently of two and ten days. No further loss.

Case XLII, aged 46, multipara 1.—Catamenia eight to ten days every twenty-one to twenty-five days, with severe loss. *Per vaginam* two small sub-peritoneal fibroids, the size of marbles, could be felt, one anteriorly and the other posteriorly. Exposure to radium. One show of three days subsequently, then no further loss. The fibroid in the anterior wall, by reason of its position in relation to the bladder, would have to be removed if it showed any increase in growth. So far, however, it has remained of the same size.

DISCUSSION.

Dr. LAPHORN SMITH said that Mr. Forsdike had shown great energy in carrying out this comparatively new treatment. While he (Dr. Laphorn Smith) admitted that radium was a powerful irritant when applied to the inside of the uterus, he was not sure that, like X-rays, it did not act rather by partially sterilizing the ovaries. None of the women and none of the cats to whom it had been applied had subsequently had any offspring. As far as the women over 45 were concerned, it did not make any difference whether it sterilized the ovaries or not, as long as it cured the hæmorrhage. Some of the members of the Section might remember Professor Batty's operation. He was one of the founders of the American Gynæcological Society, and was one of the first to remove ovaries for the sole purpose of stopping hæmorrhage. He (Dr. Laphorn Smith) could testify to its efficacy in many cases. But it was not justifiable treatment in the case of a woman younger than 45, because there were other equally effective measures. He alluded to Apostoli's method and to atmocausis, and the use of ergot and calcium.

Dr. G. F. BLACKER said he had been treating hæmorrhage from the uterus at the menopause, from small fibroids, and in cases of so-called functional menorrhagia since 1916. He had altogether had seventy-seven of these cases, and his results had been uniformly good. Radium was a specific for climacteric hæmorrhage. It should only be employed in small uncomplicated fibroids: any tumours larger than the pregnant uterus at the fifth month should be operated upon. He regarded any pelvic complication, such as disease of the appendages, as a complete bar to the use of radium. He had used large doses both internally and externally. The method was without danger in carefully selected cases. He had recently been collating his cases and the results had been published in the *Lancet*, March 8, 1922. He thought Mr. Forsdike was a little too optimistic, and that he would no doubt find that if he followed up the after-course of his patients for a sufficient length of time, in some the hæmorrhage recurred, and further treatment became necessary; this was particularly likely to happen in cases complicated by the presence of fibromyomata. He (Dr. Blacker) believed that the chief action of the radium was on the ovaries, and that this accounted for the marked hæmorrhage which often followed immediately, for the symptoms of the menopause, which were not uncommon, and for the effect following the application of radium over the ovarian regions through the abdomen.

Mr. L. PROVIS said that he thought that X-rays were just as successful as radium, if not more so in cases of menopausal hæmorrhage. There was no necessity, with X-rays, for any anæsthetic. The action of radium was probably chiefly on the ovary, but it had also a local action on the uterus.

Dr. H. RUSSELL ANDREWS said that he could not believe that the action of a tube of radium in the uterus had produced vesico-vaginal and recto-vaginal fistulæ. He suggested as a possible explanation of the case mentioned by Mr. Provis that the vagina had been inefficiently packed, and that the radium had been expelled into the vagina soon after its insertion into the uterus.

Mr. SIDNEY FORSDIKE (in reply) said that the amount of interest shown in his paper was very gratifying. Dr. Laphorn Smith had mentioned atmocausis and electricity in the treatment of this class of case: he (Mr. Forsdike) was under the impression that these methods had long since been abandoned—in any case he had had no experience of them. Dr. Laphorn Smith had also dealt with gauze plugging, calcium salts and ergot; these were of course all temporary measures, and did not touch the problem of radical treatment. Incidentally in the particular class of case under consideration he (Mr. Forsdike) had always found that ergot aggravated the hæmorrhage. He entirely disagreed with Dr. Blacker on the question of treating fibroids by radium; he did not think that radium had any part to play in the treatment of these tumours save as a preliminary to operation, and in cases such as those quoted above, where the hæmorrhage was the leading symptom, and the small fibroids merely an expression of the general

80 Forsdike: *Treatment of Uterine Hæmorrhage by Radium*

condition of the uterus. For a case of fibroids very much smaller than a five months' uterus, where the fibroids demanded treatment at all, his opinion was that myomectomy or hysterectomy was the proper treatment. He had already pointed out that pelvic inflammation was an absolute contra-indication, but the fact that some other surgeon had removed a tube and ovary from a patient for excessive hæmorrhage did not deter him from exposing them to radium so long as he could not satisfy himself that pelvic inflammation existed. Again, he differed from Dr. Blacker in the question of the scarring of the endometrium. In none of his experimental cases was there any evidence of scar formation. In reply to Mr. Provis, with regard to the length of time required for the treatment of this condition by X-rays, and the occasional fatality following it, he (Mr. Forsdike) had only quoted information from a paper by Dr. Eden and Mr. Provis, read before the Section just two years ago.¹ With regard to the formation of fistulæ, he could not conceive of this happening where the radium was placed *in utero* under proper conditions.

¹ *Proceedings*, 1921, xiv (Sect. Obst. and Gyn.), pp. 282-303.

Section of Obstetrics and Gynæcology.

President—Dr. T. W. EDEN.

A Calcified Tumour of the Recto-vaginal Septum.

By L. CARNAC RIVETT, F.R.C.S.

A CALCIFIED mass 2 in. (5 cm.) by $1\frac{1}{2}$ in. (4 cm.) by $1\frac{1}{4}$ in. (3 cm.) removed from the recto-vaginal septum of a woman, aged 54.

The patient complained of pain low down in the back, some straining and tenesmus. On vaginal examination the mass was felt in the lower half of the posterior vaginal wall. *Per rectum*, the mass was felt lower and more superficially. Under anæsthesia the sphincter ani was stretched and, with one finger in the vagina, the tumour was pressed downwards until it appeared at the anus. The rectal mucous membrane was incised and the tumour easily shelled out. The patient made an uninterrupted recovery.

The interest of this specimen lies in the question of its origin. Is it an adenoma of the recto-vaginal septum which has become calcified? Is it a subperitoneal fibroid which has become detached and migrated through the floor of the pouch of Douglas? Or is it a calcareous deposit in the rectal wall? This last seems to me to be a possible explanation as the tumour appeared to be immediately under the rectal mucosa.

Dr. LAPHORN SMITH said he had seen two other conditions which he would suggest as possible explanations of the presence of such a hard stone as the one shown and removed from the posterior vaginal wall. One was a phlebolith which had formed in a vein and was about half the size of the specimen shown. The other was the remains of an abscess which had never been recognized until many years later when the liquid had been absorbed and the solid part, probably phosphates, had been calcified by accretion of lime salts, a method of Nature for walling off foreign matter. It was quite likely to be one of the other conditions mentioned, but those he (Dr. Laphorn Smith) had stated might be added. It was certainly very rare.

A Ruptured Hæmatoma of the Ovary, with Extensive Intraperitoneal Hæmorrhage.

By L. CARNAC RIVETT, F.R.C.S.

THIS other specimen is a lutein cyst which occurred in a single woman, aged 25, who was seized with sudden abdominal pain and collapse. On examination the abdomen was tender over the right lower quadrant, and appendicitis was diagnosed. Laparotomy was performed and about 25 oz. of blood and clots were found in the peritoneal cavity. The cyst was shelled out,

and a couple of hæmostatic stitches put in the ovary. The patient made an uninterrupted recovery.

Cases of rupture of a hæmatoma of the ovary giving rise to such extensive hæmorrhage are sufficiently rare to be worth recording. There was nothing to indicate ectopic gestation, and a section is exhibited.

The Clinical Aspects of Adenomyomata of the Female Pelvic Organs.

By ARCHIBALD DONALD, M.D., Ch.M.

(*Professor of Clinical Obstetrics and Gynæcology, University of Manchester.*)

Terminology.—An adenomyoma is, as the name implies, a tumour composed of glandular and muscular elements. The glands are lined by columnar cells and are surrounded by, or rest on, unstriped muscle cells and a stroma of small cells. The microscopical picture closely resembles that of the endometrium. For this reason the name of "endometrioma" has been suggested, but, as that name implies the acceptance of a theory as to the mode of origin of these growths, it seems to me better not to use it. At the same time, there is no doubt that the elements in adenomyomata, in some cases at least, behave like those in the endometrium. In some cases the "cytogenous tissue" takes on a decidual reaction during pregnancy—and in many cases the spaces are filled with altered blood, the result of hæmorrhage during the menstrual periods. It seems probable that this causes the acute dysmenorrhœa which is so common a symptom in adenomyomata. Some writers believe that the mass of glandular and muscular tissue found between uterus and rectum—adenoma of the rectovaginal space—is the result of inflammation and not a true neoplasm, and have invented such strange terms as "adenomyositis." But even if this view is correct for swellings in that particular place, it does not apply universally. It seems best for the present to keep to the original term of adenomyoma. Further research may enable us to find a more suitable name and one which will embrace the different varieties of the disease.

Situations.—Adenomyomatous growths are not confined to the female pelvic organs. They are found in the intestinal canal and in the umbilicus. In the pelvis they are found in the uterus, in the round ligament, Fallopian tube, ligament of the ovary, ovary, and rectovaginal space. They may be in only one of these situations, or in more than one, and may even be found in all six simultaneously. It is not possible to state definitely the relative frequency in which they are found in each of the situations noted. At one time it was thought that they were the most common in the uterus and occurred rarely in the ovary. Now it seems fairly probable that the ovary is a frequent, if not the most common, site of these growths; that the pouch of Douglas is also somewhat frequently involved; and that the other structures—including even the uterus (if that is not included in the rectovaginal space)—are less frequently affected.

Infiltrating Ovarian Cysts with Tarry Contents.—In a previous communication (*Journal of Obstetrics and Gynæcology of the British Empire*, Autumn, 1922) I have remarked on the frequent association of these cysts and adenomyomatous growths in the rectovaginal space. It is a type of cyst long known to all gynæcologists, but its true nature has only recently been

discovered. These cysts are situated on the back of the broad ligament, are often bilateral, never attain to a very great size—varying generally from that of a walnut to that of a Jaffa orange. They are nearly always deeply embedded in the broad ligament and adherent to the surrounding structures in the pelvis. They are peculiar in that they possess powers of infiltration or dissection and yet are not truly malignant. They contain somewhat thick, viscid contents of a tarry or chocolate-brown colour. If these tumours are cut in serial sections and carefully examined, adenomyomatous elements are generally found; sometimes the muscular element is missing, but in others the glandular, cytogenous and muscular constituents can plainly be seen. In a large number of cases they are associated with adenomyomata in the rectovaginal space. They are now recognized as adenomyoma of the ovary, and it seems possible that, in some cases at least, the adenomyomata of the pouch of Douglas are derived from them, just as the warty growths in the peritoneum are caused by the rupture of a papilliferous ovarian cyst.

Cases.—As comparatively few cases of operation for adenomyoma have been so far published in this country, a report of my recent experience may be of interest. The total number of the cases is sixteen. The operations were all performed last year, that is, between January and December, 1922. The number is not sufficiently large to allow of any dogmatic conclusions being drawn with reference to onset, symptoms, &c., but the record of the cases will facilitate comparison with other lists.

Age.—The youngest patient was 23 and the oldest 47. Half of the cases were under 40 and the other half between 40 and 47. None of the patients had reached the menopause.

Social Conditions and Obstetric History.—Two of the patients were single. Of the fourteen married patients, five, or nearly one-third, had never been pregnant. Of the remaining nine, five patients had one or more miscarriages. These figures (for what they are worth) seem to indicate a relationship between adenomyoma and sterility, and abortion.

Symptoms.—The symptoms of which complaint was made were dysmenorrhœa, pain or pressure in the rectum, and dyspareunia. These symptoms occurred in various combinations. *Dysmenorrhœa* was much the commonest symptom; it occurred in thirteen of the cases, if we include one case in which the pain in the rectum was greatly aggravated at the time of the period. Sometimes it occurred before the period began and was generally most acute during the first day or two of the period. In the remaining three cases there is no note of it, but in one of these, pain in both iliac regions and pressure on the rectum was more or less constant. *Pain in the rectum* was noted in three cases and *dyspareunia* in four cases. As regards this last symptom, it is probable that no direct questions were asked; otherwise I think the numbers would have been larger. There was complaint of *profuse hæmorrhage* at the periods in seven cases, or nearly half, but there is reason to believe that this is not a symptom of adenomyoma, unless the growth is actually in the uterus, but is generally due to some other co-existing condition of the uterus.

Physical Signs.—These consisted mainly of the following conditions:

- (1) *Hard or irregular swelling or nodules* felt through the posterior fornix. This condition was present in twelve, or three-quarters of the sixteen cases.
- (2) In nine cases there was *indefinite resistance at one or both sides of the uterus*; and in three of these, definite enlargement of the ovary was made out.
- (3) In four cases the *uterus was retroposed and its mobility was much limited*.

TABLE OF CASES OF ADENOMYOMA OF THE FEMALE PELVIC ORGANS IN WHICH OPERATION WAS PERFORMED.

Case	Age, social and obstetrical history	Symptoms	Physical signs	Diagnosis	Conditions found at operation	Result and Pathological Report
I	Mrs. B., 41; no pregnancy; Dr. Robinson, Warrington	Dysmenorrhœa (first two days), menorrhagia, leucorrhœa	Large cervix; uterus retroposed with limited mobility; indefinite resistance on both sides; tender, nodular mass behind uterus	Chronic metritis with old inflammatory trouble in appendages or adenomyoma	January 8, nursing home: Tarry cyst in each ovary, very adherent; recto-vaginal space mass adherent to uterus in front and rectum behind; panhysterectomy	Recovery; smooth convalescence; adenomyoma
II	Mrs. A., 37; three children, one abortion seven months previously; Dr. John Steward, Manchester	Sharp pain in rectum when bowels are moved; worse at periods; dyspareunia; cured in St. Mary's Hospital (Manchester) two years ago	Hard, round, tender swelling size of filbert in pouch of Douglas; felt also through rectal wall	Adenomyoma	January 19, St. Mary's Hospital: Uterus and ovaries normal, but uterus adherent to rectum below level of internal os; nodular growth (size of filbert) dissected out with scissors	Recovery; adenomyoma in recto-vaginal space
III	Miss A., 26; Dr. Cryer, Manchester	Dysmenorrhœa (left iliac) two days before period, for four years leucorrhœa	In pouch of Douglas irregular hard mass which seems adherent to uterus and rectum; very tender, round swelling on left and on right indefinite resistance	Adenomyoma with thickened uterine appendages	February 3, St. Mary's Hospital: Tarry cyst in each ovary; pouch of Douglas obliterated up to level of internal os; uterus removed by panhysterectomy and hard, diffused mass dissected from rectum	Recovery; recto-vaginal fistula for five or six weeks, then complete recovery; adenomyoma in recto-vaginal space; hyperplasia of endometrium
IV	Mrs. W., 42; no children, one miscarriage eight years ago; Dr. Taylor, Moston, Manchester	Dysmenorrhœa (whole of menstrual period), worse first two days (mainly right iliac)	Indefinite mass behind and on right of uterus; ovaries felt enlarged and adherent	Ovarian cysts and adenomyoma	March 16, St. Mary's Hospital: Tarry cysts of both ovaries very adherent; rectum adherent to uterus; when this was separated, hard nodule size of pea was removed from rectum; both ovaries removed	Recovery; adenomyoma in recto-vaginal space
V	Mrs. F., 43; two children, two miscarriages, last eight and half years ago; Dr. Seager, Warrington	Dysmenorrhœa (first two days), profuse hæmorrhage	Uterus enlarged, retroposed; hard mass in recto-vaginal space; indefinite resistance on both sides	Adenomyoma; cystic ovaries	March 27, St. Mary's Hospital: Tarry cysts of both ovaries with extensive burrowing; uterus very adherent to rectum and hard mass below; panhysterectomy	Death; general condition, very poor and shock was pronounced; an attack of acute bronchitis on third day; pulse always feeble; died on fifth day; adenomyoma in recto-vaginal space

VI	Mrs. V., 43; one child, two miscarriages, last three years ago	Continuous hemor- rhage for six weeks; curetted seven months be- fore for (?) mis- carriage	Cervix much hypertrophied with large masses of glan- dular tissue; uterus has limited mobility	Chronic metritis	May 1, at nursing home in Hudders- field with Dr. Pye-Smith: Rectum adherent half-way up uterus; adhe- sions separated; panhysterectomy; nodule removed from anterior wall of rectum	Recovery; adenomyoma in recto-vaginal space
VII	Mrs. C., 47; no children, three mis- carriages; Dr. Marshall, Bolton	Dysmenorrhœa; abdominal opera- tion two years ago; one ovary re- moved at Bolton two years ago	Mass in pouch of Douglas; nodular, tender	Adenomyoma	June 20, St. Mary's Hospital: Pan- hysterectomy; recto-vaginal ade- noma dissected out	Recovery; adenomyoma of uterus and rectal wall
VIII	Mrs. C., 41; three confine- ments; Dr. Jago, Glaze- brook	Periods rather less of late; no men- tion of dysmenor- rhœa; dyspar- œunia	Large catarrhal cervix; nod- ular thickening in pouch of Douglas; very tender; in- definite thickening on both sides	Adenomyoma; ? ovarian cysts	June 22, St. Mary's Hospital: Tarry cysts in both ovaries, very adherent; rectum adherent to uterus; pan- hysterectomy; nodule dissected out of anterior rectal wall	Recovery; adenomyoma in ovaries and uterus
IX	Mrs. G., 39; no pregnancy; Dr. Grainger, Oldham	Dysmenorrhœa (three or four days before period); leu- dyspareunia; leu- corrhœa	Hard, tender mass in pouch of Douglas; bimanual diffi- cult; indefinite resistance	Adenomyoma	June 26, St. Mary's Hospital: Uterus enlarged and globular, thickening in tubes and round ligaments; tarry cysts in both ovaries; mass in ob- literated recto-vaginal space; pan- hysterectomy and dissection of mass from rectum	Recovery; adenomyoma in recto-vaginal space
X	Mrs. S., 47; two children	Profuse menstrua- tion; formerly great pain second and third day	Large cervix with much glan- dular hypertrophy; indefi- nite resistance in posterior fornix and hard nodule in recto-vaginal space; thickening on left	? Adenomyoma	August 1, nursing home in Hudders- field with Dr. Irving: Rectum very adherent; tarry cyst in left ovary; right ovary cystic but fluid clear; operation terminated by subtotal hysterectomy; removal of append- ages and dissection of nodule; patient was extremely stout	Recovery; adenomyoma of uterus and ovary
XI	Mrs. L., 37; one child	Dysmenorrhœa second day (hypo- gastric and iliac); leucorrhœa	Cervix thickened; swelling in pouch of Douglas	Ovarian tumour	October 15, nursing home: Tarry cyst of left ovary, very adherent, size of hen's egg; ruptured during re- moval; no other evidence of adeno- myoma	Recovery; pathological report states: "No microscopic confir- mation of macroscopic findings"
XII	Mrs. W., 34; no pregnancy; Dr. Riddell, Sandbach	Dysmenorrhœa four days (right iliac); menorrhagia	Uterus retroverted, adherent with mass on right and rounded swelling behind and on left side, size of hazel nut	Adenomyoma	October 18, nursing home: Both ovaries very adherent; contained tarry cysts; panhysterectomy; tumour dissected from rectal wall (rectum opened and tear repaired)	Recovery; adenomyoma in recto-vaginal space and ovary

TABLE OF CASES.—Continued.

Case	Age, social and obstetrical history	Symptoms	Physical signs	Diagnosis	Conditions found at operation	Result and Pathological Report
XIII	Mrs. R., 37; three children; Dr. Ainscow, Hindley	Backache and pain in rectum especially during menstruation; menorrhagia	Uterus irregularly enlarged, retroverted and fixed; much tenderness behind on examination	Chronic metritis with pelvic peritonitis, or adenomyoma	October 26, St. Mary's Hospital: Uterus globular and enlarged; thickening of round ligaments; small cysts in posterior uterine wall; rectum adherent; left ovary contains small cyst with tarry contents; panhysterectomy and dissection of recto-vaginal adenomyoma	Recovery; adenomyoma left ovary, recto-vaginal space, posterior wall of uterus and fundus
XIV	Mrs. M., 34; no pregnancy; Dr. Lakin, Manchester	Periods scanty; dysmenorrhœa (hypogastric); dyspareunia	On examination under ether, small, hard nodule in recto-vaginal space	Adenomyoma	October 19, St. Mary's Hospital: Rectum adherent to uterus; dissection of nodules from anterior rectal wall; left ovary cystic (clear contents) removed	Recovery; adenomyoma ovary and recto-vaginal space
XV	Mrs. B., 41; no pregnancy; Dr. Hartley, Heywood	Left iliac and hypogastric pains more or less constant, at times severe; was cured two years ago in St. Mary's Hospital for heavy retroversion; still has pain and pressure on rectum	Retroversion not marked, uterus retroposed, and small nodules in pouch of Douglas; rounded swelling in the anterior wall of uterus	Adenomyoma in recto-vaginal space	December 7, 1922, St. Mary's Hospital: Small fibroid removed from anterior wall of uterus by myomectomy; rectum adherent to uterus; separated and small nodule dissected out; both appendages apparently healthy, not removed	Recovery; adenomyoma in recto-vaginal space
XVI	Miss M., 23; Dr. Affleck, Farnworth, near Bolton	Acute dysmenorrhœa (three days before onset and first day); menorrhagia; leucorrhœa; hypogastric pain more or less constant	Great tenderness in posterior fornix with indefinite thickening; under an anæsthetic distinct mass was felt in pouch of Douglas with indefinite thickening on each side	Adenomyoma in recto-vaginal space and thickening of both ovaries	December 14, 1922, St. Mary's Hospital: Both ovaries converted into tarry cysts, very adherent; removed along with uterus (panhysterectomy); rectum adherent to uterus; thickened mass dissected from anterior wall; rectum opened in process; covered over	Recovery; although the specimen seems typical to the naked eye; pathological examination has not revealed any microscopical evidence of adenomyoma in the ovary or in the tissue dissected from the rectum.

Diagnosis.—The disease was diagnosed in almost every case, if not with certainty, at least as a probability, owing to the conditions which have here been noted. Sometimes only one of the signs was discovered; sometimes two in combination. In several cases the history suggested the possibility of adenomyoma: very acute dysmenorrhœa beginning before the onset of the period or during the first day, and especially so if the pain has come on some years after the periods have been established, is always suspicious. Pressure on the rectum is another important symptom, as is also acute dyspareunia. Indefinite resistance at the sides of the uterus is suggestive of adherent, tarry, ovarian cysts. The hard mass at the back of the uterus suggests the rectovaginal adenomyoma. The easiest case for diagnosis is that in which a distinct, hard tender nodule is felt through the posterior fornix. This nodule is generally not much bigger than a pea, and may be smaller—sometimes there is a collection of nodules. The retroposition and limited mobility of the uterus suggest adhesion of the uterus to the rectum.

Conditions found at Operation.—Tarry cysts of one or both ovaries were found in eleven cases (in three cases unilateral, in eight cases bilateral). In one other case, a cystic ovary containing clear fluid was removed, but the pathologist succeeded in finding adenomyomatous tissue. The growths in the pouch of Douglas varied considerably, from a hard mass that stretched from side to side and filled the pouch of Douglas, to a small nodule on the anterior wall of the rectum or posterior wall of the uterus. Where a mass existed, the rectum and posterior aspect of cervix and uterus were involved. In the other cases where only nodules could be found, the rectum was adherent to the lower part of the uterus as high as the internal os and had to be dissected off.

Operations.—The operations in the sixteen cases were as follows: (1) Panhysterectomy or subtotal hysterectomy with dissection of adenomyomatous growth from the pouch of Douglas or rectal wall, twelve cases. (2) Removal of one ovary and dissection of nodules from uterus and rectum, one case. (3) Removal of fibroid by myomectomy, separation of rectum and dissection of small adenomyoma from the rectal wall, one case. (4) Removal of tarry ovarian cyst, one case. (5) Dissection of rectovaginal growth only, one case.

As will be seen from this table the operations varied very considerably in severity. In twelve of them, however, panhysterectomy or subtotal hysterectomy had to be performed in order to make it possible to dissect out the adenomyomatous growths in the pouch of Douglas. All these cases were difficult, but the difficulty varied according to the size of the growths in the rectovaginal space. There was one death, which followed a very difficult operation in a patient whose pelvis was widely infiltrated with the growth. In three other cases, the rectum was opened and had to be covered by suture. In one of these a rectovaginal fistula persisted for six weeks. In one case all that was required was to separate the adherent rectum from the uterus and dissect out an adenomyoma from the rectal wall. Dissection in this region has to be very carefully performed, as any serious weakening of the rectal wall may lead to subsequent disaster. The remaining three cases were unusual—in one there was the removal of a tarry cyst of one ovary, with no other complication: in another, the removal of a fibroid tumour by myomectomy in addition to the removal of a small adenomyoma of the rectovaginal space; and in the remaining case the removal of a clear cyst of one ovary (which the microscope proved to contain adenomyoma) with dissection of some nodules from the rectum.

Pathological Reports.—I have pathological reports on every case; the investigations have been made by Dr. Addis, to whom I am much indebted, in the clinical laboratory of St. Mary's Hospital, Manchester. Microscopic evidence of adenomyoma was found in every case except two (Cases XI and XVI, pp. 85, 86). In Case XI a tarry cyst of the left ovary was removed, but there was no sign of growth elsewhere. The cyst was very adherent, and had to be dug out of the back of the broad ligament, and corresponded in every respect to the tarry cysts in the other cases; but the microscope failed to show the usual glandular structures. Case XVI was more striking. It was a typical case of widespread adenomyomata in the ovaries and pouch of Douglas, but so far the microscope has not confirmed the diagnosis made by the naked eye. The specimen is still being investigated. In the remaining fourteen cases the presence of adenomyoma was confirmed by the microscope; in all of these it was found in the growths removed from the rectovaginal space or from the posterior wall of the uterus or anterior wall of the rectum. In the eleven cases of tarry ovarian cyst, microscopic confirmation of adenomyoma was obtained only in two cases; but in one other case, in which an ovarian cyst with clear contents was removed, typical adenomyomata were found by the microscope in the cyst wall. There is no doubt that the microscopic investigation of these tarry cysts is very difficult. The effusion of blood, which is the characteristic property of these growths, destroys the glandular element, and it may require a very long and patient investigation of a cyst before definite microscopic evidence is obtained. The larger and older the cyst, the more complete is the destruction. But the naked-eye appearances and general characteristics are quite sufficient for diagnosis.

General Remarks.—It is generally assumed that adenomyomata belong to the very rare kinds of tumour, but the experience I have here recorded seems to show that they are much more common than is generally believed. Looking back on my operative work of many years, I am fully convinced that I have had many such cases in the past that I did not recognize even at the operation. They were generally taken to be cases of pelvic inflammation, or ovarian cysts that had become infected. The ordinary blood cyst of the ovary is not adherent, and it is filled with fluid or clotted blood, which is of the normal colour, and has not the viscid consistence of the contents of a tarry cyst. Sometimes the contents of one loculus of a large multilocular ovarian cyst may have a dark colour, due to the effusion of blood, but these cysts have not the penetrating power of the adenomyomatous cyst. The adhesion of the rectum to the lower half of the uterus is a condition that one has often met with, but we have failed to appreciate its significance. There is little difficulty in recognizing the condition when the abdomen is opened. The existence of adherent cystic ovaries with viscid tarry-coloured contents is present in about 75 per cent. of the cases. The adhesion of the rectum to the lower part of the uterus and the obliteration of the pouch of Douglas is practically always present in rectovaginal adenomyoma, and when the adhesions are separated, if there is not a distinct mass, the hard, round, little bodies will be felt.

The recognition of the conditions before operation is not very difficult if they are borne in mind at the time. The indefinite resistance on each side of the uterus, indicating tarry ovarian cysts; the retroposition and limited mobility of the uterus, due to adhesion of the rectum; or the hard irregular mass or firm, round nodules like gunshot are all suggestive, if not conclusive, of the diagnosis. The severe dysmenorrhœa coming on before

the period and lasting for the first day or longer, especially if this has been increasing during adult life, should lead us to suspect the condition.

Possibly a critic may ask, "Why operate on these cases? Why not leave them, and see if they develop?" In the first place, most of them would be operated upon even if they were not recognized as cases of adenomyomata. The typical uterine adenomyoma is generally mistaken for a fibroid, and operation advised. When the growth exists in one or both ovaries, causing enlargement which can be felt, ovariectomy is recommended. In the diffuse forms of growth which simulate pelvic inflammation, an exploratory operation is generally carried out. One or other of these two latter conditions existed in the majority of my cases. There remains the case in which there are one or more small masses to be found in the pouch of Douglas. The symptoms in these cases are always much out of proportion to the size of the growth. The dysmenorrhœa, dyspareunia, and rectal symptoms are sufficient to induce the patient to seek relief. In two of the cases in the table (Cases II and XIV, pp. 84, 86), curetting had been performed in the hope of obtaining some relief; but both patients had returned with the request that something more should be done.

The further question arises as to the nature of the operation which should be undertaken in the various types of case. The conclusions at which I have arrived are the following:—

(1) When both ovaries contain tarry cysts and there is a mass or some nodules in the rectovaginal space, a radical operation is indicated. When both ovaries are removed there is no object in leaving the uterus, and a panhysterectomy gives the easiest access to the nodular growths between cervix and rectum.

(2) When one ovary only is diseased but there is a large mass involving rectum and uterus, the radical operation is still advisable.

(3) If one ovary only is affected and there are merely some small nodules in the pouch of Douglas, a conservative operation may be sufficient—removal of the affected ovary and dissection of the nodules after separation of the rectum. But if this course is followed, it is necessary to examine the second ovary very carefully, as the tarry cysts are sometimes small; and it is also advisable that the patient should report from time to time.

(4) The conservative method applies still more to those cases in which the only lesion found is the presence of one or two small nodules in the rectovaginal space.

Since the preceding part of this paper was written, I have been able to procure the article in full on "Perforating Hæmorrhagic (chocolate) Cysts of the Ovary," which was communicated by Dr. John A. Sampson to the American Gynecological Society in 1921. I have already in my previous paper quoted an abstract of this communication, which appeared in the *British Medical Journal* of February, 1922, but that contained no reference to the methods of operation nor to the clinical symptoms or treatment. It is of interest to compare Dr. Sampson's conclusions with my own. The age-incidence in his cases was from 26 to 47—only two cases were under 30, and none occurred after the menopause. As regards pregnancy, of the fifteen married patients in his list, nine had never been pregnant. He concludes from this, "It would seem that this condition is likely to occur in women who are sterile." There is no mention, however, of miscarriages in the remaining six patients. Dysmenorrhœa was noted in only eleven of his twenty-three cases. This

finding is in marked contrast to my own observations. Menorrhagia he found in six cases, but in three of these myomata were found. Pain was present in only fifteen of the twenty-three, and its location and severity varied. It will thus be seen that there was not the definite indication of trouble that I found in my cases. This may be due to the fact that the ovarian cysts in Dr. Sampson's cases were usually small. The majority were under 4 cm. in diameter, and the largest measured 9 cm.

I regard Dr. Sampson's paper as the most important contribution that we have had to this difficult subject. He is still carrying on investigations, and has reserved judgment on some of the problems involved; but he is fully convinced that when we find adenoma in the ovary and also in the recto-vaginal space the growth in the ovary is primary.

DISCUSSION.

Dr. HERBERT SPENCER said he was much interested in Professor Donald's cases of blood-cysts of the ovary and their relationship to "adenomyoma" of the utero-rectal space and was glad to find that Dr. Donald did not employ the erroneous term "recto-vaginal septum," in which these growths did not usually occur. He also agreed with the author in objecting to the term "endometrioma" as applied to the growth in the ovary. The presence of glands in the ovary was not rare; but it would require better sections than those exhibited to persuade him that the stroma was identical with the cytogenous tissue of the uterine endometrium. The presence of a stroma resembling that of the uterine endometrium was observed in some cases and this condition had been known for many years—having been described by Pick, in 1905, as adenoma endometrioides. In many of the cases, however, the blood cysts showed no such stroma and some of them were undoubtedly corpus luteum hæmatomata of which an example had been shown that evening and he had removed one thirty-two years ago. He had removed an adenomyoma of the recto-uterine space from a virgin suffering from severe dysmenorrhæa, by total hysterectomy, opening the anterior fornix and amputating the body, to clear the field, before removing the cervix and the growth attached to the rectum. He did not think subtotal hysterectomy was a suitable operation for these cases. In this instance there were no macroscopic cysts in the ovaries, which were not removed, and the patient had remained well nine years later. In another case the uterus and ovaries were removed by vaginal hysterectomy for cystic ovaries and adenomyoma of the whole of the posterior surface of the uterus. In this case it seemed that the ruptured cysts had given rise to the adenomyoma. In order to exclude the uterine endometrium as a source he had made a coronal section through the middle of the posterior wall: this did not contain any glands and showed that the growth came from the peritoneal surface.

While blood-cysts of the ovary usually needed removal he thought the importance of adenomyoma of the recto-uterine space was being exaggerated. The condition was much commoner than it was formerly thought to be: no doubt the "posterior parametritis" of old writers was often of this nature. One diagnostic point (the presence of nipple-like projections in the posterior fornix) had not been alluded to by the author. He (Dr. Spencer) had watched one of these cases for years without any change occurring in the local condition, which gave rise to no serious symptoms. In view of the difficult and dangerous operation for the removal of adenomyoma of the recto-uterine space he thought few of these cases—except those attended by severe dysmenorrhæa or other serious local condition—should be subjected to extirpation, which, when attempted, was often incomplete and involved considerable risk of injury to the rectum.

Dr. RUSSELL ANDREWS remarked on the extreme interest of Dr. Donald's communication and gave a short account of a case in which radium had been used with a good effect in the treatment of adenomyoma affecting the uterosacral ligaments and the bases of the broad ligaments. Three years earlier a very adherent left-sided ovarian

blood-cyst had been removed in South America and an attempt had been made to remove a hard nodular growth in the rectovaginal space. Three nodules were removed but some indurated tissue was left. When he (Dr. Andrews) saw the patient he considered that it was useless to attempt to remove the nodular growth, and Dr. Fairbairn agreed with him. The patient was suffering very severe pain. After treatment by radium the nodules became slightly diminished in size, the pain disappeared and the general condition improved greatly. Six months later there was no pain or discomfort but there was still a great deal of induration surrounding the rectum.

Dr. HERBERT WILLIAMSON said that the Society was indebted to Dr. Donald for focussing attention upon the clinical aspect of these important cases. The speaker desired to direct attention to two statements of opinion commonly held which he believed to be erroneous: (1) With regard to the question of leaving a small portion of the growth adherent to the uterine wall; (2) with regard to the value of irradiation in treatment. For example, in Dr. Lockyer's classical monograph the following passage occurred (p. 371).

"The portion adherent to the bowel wall should where possible be removed by blunt dissection. Where this is not feasible owing to deep infiltration of the bowel-wall it seems unnecessary to sacrifice the bowel by excision since small areas of 'growth' have been known to disappear. It seems that infiltrated bowel-wall may safely be left if the bulk of the adenomyomatous material be removed."

And again on p. 444:—

"All forms of radio-therapy for adenomyoma are disappointing; only in one case have I heard of radium doing good. It is my belief that such treatment is liable to excite the inflammatory process which is the pathological basis of the disease."

He would quote two cases bearing upon these points:—

(1) That of a lady, aged 37, first seen by him in October, 1920. She had been seen by Dr. Herbert Spencer, in 1914, who told her she had "a small growth low down on the side of the womb." Shortly after this she returned to India and in 1917 consulted Major Bott on account of pelvic pain. He found a nodule the size of a large walnut to the posterior and right side of the cervix extending into the base of the right broad ligament and adherent to the anterior aspect of the rectum. Two years later, in 1919, she consulted him again because the pain had become more severe. Finding the tumour had grown considerably he performed a complete hysterectomy. Major Bott wrote:—

"The fibroid was so closely adherent to the anterior wall of the rectum that I had to leave a small portion of it behind. As she has experienced some return of her old pain, and as she is leaving for England, I have told her to come and see you."

He (Dr. Williamson) saw her in October, 1920. She complained of severe rectal tenesmus and of increasing difficulty in evacuating the bowel. On examination he found a hard, tender mass, the size of a pigeon's egg, in the situation of the rectovaginal space, fixed to both vaginal and rectal walls. The growth had involved both utero-sacral folds. Excision of a small portion showed the presence of dilated glandular spaces in a cytogenous stroma. Dr. Cuthbert Lockyer saw the patient at this time and advised colotomy and then an attempt at complete excision. He (Dr. Williamson) decided, however, in the first place, to try the effect of radium, and he obtained from the Radium Institute emanation equivalent to 100 mg. of radium metal contained in a silver tube of 1 mm. thickness. On December 3, 1920, the tube was buried in the growth and left *in situ* for thirty-six hours. On January 18, 1921, the growth was found to have shrunk to about half its original size and a second tube of emanation equivalent to 95 mg. of radium metal was inserted for twenty-four hours. Six weeks later on examination he could feel no growth at all, but only a little thickening in the situation it had occupied. She had lost the pain and there was no difficulty in the action of the bowels. Since then she had been quite well.

¹ "Fibroid and Allied Tumours (Myoma and Adenomyoma)," 1918.

The second case was that of an unmarried woman, aged 32. Her periods were normal until March, 1922. On the last day of this period she felt severe aching, dragging pain in the right leg which lasted a week and gradually became easier. The pain recurred with each successive period from March to September and became more severe, always setting in on the last day of the period and persisting for seven or eight days. She was obliged to give up work for that time and had difficulty in walking. When examined on September 18, an irregular swelling was felt in the posterior fornix, situated partly in the vaginal wall and partly in the cervix itself. It was a hard nodular swelling about the circumference of a half-crown and it bled freely on touch. The vaginal mucous membrane was adherent to it. Bimanual examination showed that the growth was firmly attached to the uterus and hard fixed indurations were felt in both broad ligaments and the uterosacral folds. The nodule projected into the bowel and the rectal mucosa was fixed to it but not ulcerated. Sections of a portion removed showed a stroma consisting mainly of fibrous tissue containing glands like those of uterine mucosa and cystic spaces lined with epithelium. It was decided to try the effect of hard X-rays and accordingly on September 10 a dose of 8H was applied over the hypogastrium, and on September 11, a similar dose over the sacral region. After this the periods ceased and the pain was lost. On January 5, 6, 8, and 9, 1923, doses of 11H were applied over seven areas of the lower abdomen and back. When examined on April 27, no trace of growth could be felt, but a puckered scar on the vaginal wall marked the spot where it was previously situated. The uterus was freely mobile. *Per rectum* no nodule was felt but a thickening over which the rectal mucosa moved freely. There was very little induration in the pelvic cellular tissue, no more than could be accounted for by the presence of scar tissue. Apart from troublesome flushings due to the premature menopause the patient was quite well in all respects.

These two cases suggested that possibly in many cases radical operations were not necessary and that treatment by irradiation ought to be given a more extensive trial.

Mr. W. W. KING said that adenomyomata were relatively common in Sheffield. He had had eighty-four cases under his care during the past ten years. Glandular tissue of endometrial type was found in the ovaries in nineteen of the seventy-two extra-uterine cases, but they had not been looked for before the publication of Sampson's first paper. Sterility was very common, but complaint of severe dysmenorrhœa had only been made by about one quarter of the patients. The commonest complaint made was that of abdominal pain, and he had been struck with the frequency with which it was associated with nausea and vomiting. He asked if this association had been noted by others. There was little doubt that radium could cure adenomyoma, but the practical difficulty lay in the differential diagnosis between ovarian growths of this nature and chronic inflammation of the adnexa.

Graves' Disease and Thyroid Instability in the Cow, and its Relation to Ovarian Disease.

By L. P. PUGH, B.Sc.Lond., F.R.C.V.S.

INTRODUCTION.

OBSERVATIONS extending over some years have at length led the writer to the assumption that the so-called "nymphomania" of cows is merely one phase of a process which is in its entirety analogous to the hyperthyroidism-myxœdema syndrome in the human species. But to establish this thesis is a matter of no small difficulty. The symptom most readily

discovered in cows, i.e., the sexual excitement, may be carefully concealed in human beings. On the other hand the vascular changes which can be readily studied in man are not easy and often impossible to elucidate in animals. These very facts make the task more urgent, since here is an obvious example (if the thesis be proved) of the great mutual assistance which veterinary and human pathology may afford each other. Study of this subject has naturally opened up more lines of inquiry than the mere identification of the two diseases, but in this paper it is proposed to advance and support, however inadequately, the following propositions, namely:—

(1) That "nymphomania" in cows is a part of a constitutional disorder known in human pathology as Graves' disease.

(2) That disorder of the thyroid function is an essential feature of the "nymphomania" syndrome.

(3) That this disorder is profoundly influenced by ovarian disease.

It is impossible in cows, and, as the writer imagines, difficult in human beings, to separate with certainty the symptoms of hyperthyroidism from those of myxœdema. It is true that at one time physicians saw Graves' disease as one well-defined syndrome and myxœdema as another. Later, these were found to be much more closely connected than had hitherto been considered probable. Later still the conceptions of hyper- and hypothyroidism have extended into an ever-widening sphere until the original clear-cut syndromes have begun to be obscured by the results of clinical and experimental observation.

Thus it comes about that nymphomania in cows cannot be presented as a syndrome due to the disorder of a single endocrine gland but rather as a disturbance of that endocrine balance which we call good health, a balance to which each organ contributes its proper share of some specific secretion. There are indeed many such disturbances, nor can we say with certainty which gland is primarily at fault in each of them. But from many which are known we pick out Graves' disease in man and nymphomania in cattle because they are seen to have so many features in common that their comparison seems likely to be the most fruitful line of research in approaching the many problems which await solution in endocrinology and in practical therapeutics.

When we speak of Graves' disease that name must be considered to apply to the whole symptom-complex of which a portion was first described by Parry in 1825, then by Graves in 1835, later by Basedow in 1840, while a further portion was described by Sir William Gull as a cretinoid condition and by Ord as myxœdema. In dealing with this whole syndrome it is important to keep in mind the fact that hyperthyroidism does not progress gradually and in an orderly manner to the other extreme of myxœdema. An infinity of phases divide the two and in these sometimes hyperthyroidism, sometimes hypothyroidism appears to predominate. Moreover there are effects of hyperthyroidism which do not immediately pass away when hypothyroidism gets the upper hand and so also there are signs of myxœdema which may persist through a subsequent phase of hyperthyroidism. In an early case the picture may be comparatively simple, but in one of long standing the clinical appearances may be complicated not only by the varying effects of previous phases but by the predominance of symptoms referable to other endocrine organs the implication of which is secondary.

THE CLINICAL PICTURE OF GRAVES' DISEASE.

The signs and symptoms of Graves' disease are conveniently summarized by the late Dr. T. D. Savill [1] whose "System of Clinical Medicine" is

quoted not because his authority is greater than that of other writers but because he was more concerned to crystallize the essentials from the very extensive literature on this subject. Dr. Savill summarized the following five chief groups of symptoms:—

(a) *Cardiovascular*.—Including palpitation, increased frequency and tumultuous heart action, paroxysmal dyspnoea, cardiac murmurs, sometimes dropsy and albuminuria.

(b) *Nervous*.—Including nervousness, irritability, insomnia, depression alternating with excitement, mania, vertigo, hallucinations, tremor, sudden perspirations, loss of hair and diarrhoea.

(c) *Thyroid Enlargement*.—Variable but always present at some stage. Mechanical effects of thyroid enlargement, such as change of voice.

(d) *Exophthalmos*.—Present in varying degree though sometimes not until late in the disease.

(e) *Disturbance of General Health*.—Progressive weakness.

Savill does not mention wasting, but that this is a characteristic feature is acknowledged by most writers. He gives the following additional facts:—

(f) Upwards of 95 per cent. of cases are females, a large number young adults between the ages of 15 and 30.

(g) Heredity has not been traced but hereditary neuroses are commonly present.¹

(h) Fright, anxiety, love affairs, and mental overwork are potent factors in determining the disease.

The same writer details the following as the chief characteristics of myxœdema:—

(a) Weakness of very gradual onset with characteristic slowness of action, &c.

(b) Puffy, immobile, vacant face with flushed cheeks. Scanty hair. Brittle nails.

(c) Thickened skin.

(d) Intolerance of cold. Dry, scaly skin, slow pulse.

From the foregoing quotations we can obtain a sufficiently accurate idea of the main symptoms of the hypo-hyper-thyroid syndrome, but before proceeding to a detailed comparison with nymphomania I will indicate the chief features in a typical case of the latter disease.

THE CLINICAL PICTURE OF NYMPHOMANIA.

The disease is most commonly met with in pedigree herds, the individuals comprising which are commonly of a distinctive nervous temperament. In some cases it is hereditary. The subject is always of the female sex and the usually antecedent circumstances are pregnancy and heavy milking. Owing to the habit of removing the calf from the mother, which is an essential if regrettable factor in the production of milking records, the emotional stress occasioned by these circumstances is exceptionally severe. Those who have heard the continuous cries of the cow from whom the calf has been removed will be able to form some estimate of its severity.

Early stages of the disease most frequently seen during the latter half of

¹ The writer is informed by Dr. Gordon Ward that he has under his care at the present time two families in which mother and daughter have been affected with exophthalmic goitre.

pregnancy seldom come under the observation of veterinary surgeons and when the stage of nymphomania is reached the chief symptoms of hyperthyroidism have been established for some time. The animal is excitable, nervous and even maniacal. The sexual phase known as œstrum becomes continuous and the animal will ride others and even inanimate objects. The symptoms are further aggravated by the presence of a bull. This stage, the "nymphomania" of text-books, is of several months' duration in an average case and often terminates in the slaughter of the animal, since it is no longer of use for milking or breeding purposes. At this stage there can always be found follicular ovarian cysts and rupture of these will ameliorate the symptoms. In the next stage the cow begins to assume male characteristics. The voice changes, there is thickening of the neck and alteration in the shape of the trunk together with other signs which will be described later. The last stage is one of lethargy and slow pulse, a condition in which the results of previous stages still remain, but no longer progress. Although these stages are described in their most usual order, it is to be remembered that any individual case may be difficult to assign to a particular stage and that the progress is rather a swing backwards and forwards with a prevailing general tendency to myxœdema than an ordinary process such as we have described.

We now return to our thesis: That "nymphomania" in cows is a part of a constitutional disorder known in human pathology as Graves' disease.

To establish this it is necessary to compare the symptoms in fuller detail. It must be explained that every attempt has been made to procure specimens for histological examination. Some have been obtained, but only with very great difficulty, notably from one case of very long standing in which the thyroid was obviously fibrotic. But the specimens are so few and there is so complete a lack of normal standards in cattle that the case must for the present stand or fall on clinical grounds. In any case it is not claimed that this paper can do more than afford a basis for future inquiry.

COMPARISON OF GRAVES' DISEASE AND NYMPHOMANIA.

Age and Sex Incidence.—In both, the victims are predominantly females at the fertile period of life. This predominance is 95 to 5 in human beings [1] and as much or more in cattle. No case has yet been described in a bull but it may be remarked that bulls have a limited commercial value and find their way to the market at an early age. There may be one bull to twenty cows in the country at any particular time. Moreover the bull is not exposed to the stresses incidental to heavy milking and pregnancy but on the contrary is kept quiet and secluded. Finally, it may be noted that diagnosis would be extremely hazardous. For all these reasons we may have to wait some time for a description of the disease in the male sex in cattle.

Breed and Heredity.—Both occur typically in individuals of "thoroughbred" temperament. The writer knows of the disease in three successive generations in cattle and is informed of hereditary cases in human families. Whether it is more than inheritance of temperament may be open to doubt, but, on the other hand, it may also be doubted whether temperament is anything more than an expression of the endocrine balance in individuals.

Exciting Causes.—In the human disease exciting causes seem to operate mostly through the nervous system. At first sight we may hardly expect to find an analogy with these in cattle. But the distress of the mother after removal of her calf must be admitted to be a frequent antecedent and to be an emotional shock greater than falls to the lot of most human cases.

Cardio-vascular Symptoms.—The writer has been able to satisfy himself that the typical tumultuous action of the heart, which may even be audible at a distance, is present in cows as in human beings during the early stages of the disease. Tachycardia accompanies it. It is not possible to identify palpitation in cows, nor have dropsy or albuminuria been noted—but these are not common in any case.

I have under my observation at the present time a cow that developed nymphomania eighteen months ago. She recovered quickly at the time from the sexual mania, but is still affected with tachycardia and extreme nervous irritability.

Nervous Symptoms.—On general grounds the similarity between the two diseases is exact. In both cases we have extreme irritability and excitement with mania as an occasional *dénouement*. In Graves' disease tremor is most apparent when the hand is held out, or held in such a way that no steadying weight is thrown upon the limb. These circumstances do not occur in cattle. I have noted a fine skin tremor presumably due to the underlying muscles which may be analogous to the tremors seen in Graves' disease. Diarrhoea is seen in cattle during the early stages, a cow often having frequent evacuations if a stranger appears, or from other slight cause. This reflex is under closer control and is naturally less apparent in human beings but has nevertheless been recognized and mentioned in the literature, e.g., Savill [1], already quoted.

Thyroid Enlargement.—This is not an invariable feature in women but is usual, and is easily seen since the gland is not deep seated. In the cow it is deep seated and consequently it is difficult to appreciate changes. We have no evidence either way that it is enlarged or not enlarged. This makes it doubly unfortunate that no histological reports are available.

Exophthalmos.—This has been observed in the early acute stage of nymphomania and is common in Graves' disease. The eye of the cow is normally somewhat exophthalmic, which makes this sign less easy to detect unless one is on the look-out for it.

General Health.—In both diseases failure in this is of the same character, wasting being the most prominent feature.

Myxædema as a Sequel.—This is well known in Graves' disease and advanced cases of nymphomania present similar symptoms. The hair becomes dry and harsh, the disposition extremely lethargic and the pulse slow. I have been able to satisfy myself as to thickening of the skin but this is naturally less apparent in a normally thick-skinned and hairy animal. The animal becomes constipated. The above parallels between the two diseases amount almost to identity. In each case there is a stage of excitability (the detailed symptoms being in many cases identical) followed by one of lethargy, mental and physical. In both conditions there is an extraordinary type of heart action which is typical and which is by universal consent associated with thyroid hyperactivity. Both present the highly characteristic age, sex and temperament incidence and in both there are the possible terminations, namely, cessation of the disease or steady progress to a state of lethargy. That the parallel may be more complete it may here be noted that the administration of thyroid extract has in each case the same effect, namely, amelioration of the hypothyroidism symptoms.

My opinion is that the evidence already produced is sufficient to establish a degree of clinical identity which goes far to justify the thesis already put forward, namely, that nymphomania and the Graves'-myxædema syndrome are essentially the same. It will now be assumed that this is true for the

purpose of considering the following symptoms, which are of much greater interest and importance when regarded from this point of view, namely: (1) Sexual excitement—nymphomania as a symptom; (2) pelvic and ovarian disorder; (3) assumption of male characteristics.

Nymphomania as a Symptom.—Œstrum in the cow normally lasts for about twenty-four hours. It seems to coincide with the final period of ripening of an ovarian follicle and is certainly brought to a conclusion by the rupture of a follicle whether natural or artificial. I have several times terminated œstrum in this way. The first sign of nymphomania is commonly a lengthening of the œstral period, perhaps to three or four days. This attracts the attention of the herdsman and if a veterinary surgeon be consulted he will invariably obtain the history that the cow has been one of exceptionally nervous disposition. This history is best obtained by suggesting the contrary, which will be denied. At this time it is always possible to find an unruptured follicle and if this be ruptured *per vaginam* œstrum terminates and the animal may return to the normal state or may suffer from a recurrence of symptoms associated with delayed bursting of the follicle at the next œstral period. A repetition of artificial rupture on two or three occasions will not uncommonly restore the animal to normal health. Failing any treatment œstrum is further prolonged until it becomes practically continuous and the associated symptoms increase in severity. The excitability becomes extreme and the disease, "nymphomania," is fully established.

It does not seem that there is any human parallel to these symptoms, but it has to be remembered that no such parallel has been sought for, and that for social reasons any such symptoms would be kept in the background. The cow does not undergo the years of training which prompt women to suppress manifestations of sexual desire. The result is frank and obvious nymphomania. We cannot expect to see such symptoms in subjects of Graves' disease, nor, in fact, do we find them mentioned in the literature. Geikie Cobb [2] says, "Perhaps the saddest of the changes wrought by this disease are the changes for the worse in the psyche," but the exact intention of this remark is not certainly evident. It may be that gynæcologists can throw some light on the question. Certainly hyperthyroidism is associated with the attainment of puberty and also with pregnancy, so that we might well expect psychic change associated with changes of a physical nature. But beyond this we cannot go at present.

GYNÆCOLOGICAL DISORDERS.

Pelvic troubles are extremely frequent in cows, occurring perhaps in 40 per cent. in the form of metritis, cervicitis, &c., and are all of a definitely septic type. Such pelvic troubles may of course complicate nymphomania, but they are not sufficiently constant to suggest that they have direct ætiological significance. A constant pelvic abnormality in nymphomania is patency of the cervical canal together with a relaxation of the vaginal tissues. If the cow has calved there is added definite subinvolution of the uterus. The second and most striking abnormality is the invariable existence of ovarian cysts. These are always follicular in origin, and originate in ripened but unburst follicles. That they are closely associated with the nymphomania is evidenced by the cessation of the latter if they be ruptured or removed by ovariectomy. Their exact mode of action cannot be understood without advancing into the debateable fields of endocrinology, and to do this would unduly enlarge the scope of this paper.

Very few figures seem to be available with regard to pelvic lesions in Graves' disease. Helen Gurney [7] states that in 8 per cent. of cases there was "disturbance of the generative function"; in 15 per cent. there was amenorrhœa; 6 per cent. of the cases developed after miscarriage. These figures are neither very definite nor very satisfactory. In a case published by Ward (the reference is unfortunately not obtainable) both ovaries were cystic, and it is probable that isolated cases of the sort might be disclosed by a more thorough search of the literature than active country practice of a profession allows. But at present there is no real information available either as to the existence of nymphomania or of pelvic lesions.

There remains for discussion under this heading a pelvic lesion of quite different character, i.e., actual pelvic deformity and at times multiple fractures. There would appear to be at least two factors present, namely, slackness of ligaments and softening of bone. The photographs show the resulting clinical characteristics, the most important of which is the elevation of the root of the tail. This would seem to be due to a rotation of the pelvis on the line joining the acetabula. That the change is due to ligamentous relaxation is strongly suggested by the fact that it disappears under appropriate treatment, whereas bony changes would remain. Relaxation of ligaments is symptomatic rather of myxœdema than of hyperthyroidism. Geikie Cobb [3], in connexion with the human disease, says that he is "certainly of opinion that the relaxation of the ligamentous structures in submyxœdema is something more than an artificial symptom. He has noticed it in a number of patients affected with the disease in which the diagnosis was confirmed by treatment. There are certain other signs of ligamentous relaxation which have been noted in cows, e.g., "rheumatic" joint sounds, slipping of the patella, and partial eversion of the vagina. It is possible that similar signs might be found in human beings.

Multiple fractures of the pelvis do not seem to have any parallel in myxœdema. One is naturally inclined to suggest that they are due to excessive decalcification in the hyperthyroid stage, but there is no direct evidence of this. Little is known about these fractures beyond the fact of their occurrence.

Assumption of Male Characters.—This is a very striking characteristic of the middle stages of nymphomania. The nervous irritability of the earlier stages give place to a certain aggressiveness, and the cow will paw the ground, kick out with its forelegs, and charge other animals or its attendants. The voice changes to the deep note of the male sex. The appearance of the head changes together with the expression, and the neck grows thicker. Even those who are inexpert in veterinary matters frequently comment on the change. The female characters regress. The mammary gland atrophies, while the clitoris is often enlarged. The general appearance is well shown in the photographs exhibited.

These remarkable changes do not seem to have any complete parallel in myxœdema, but there is a partial parallel in the loss of many attributes of femininity. It is usually allowed that the anterior portion of the pituitary becomes more active as the thyroid activity fails. But as a rule this is not sufficient to give rise to symptoms of hyperpituitarism in women. Swale Vincent [5] sums up the matter as follows: "When the pituitary hypertrophies as a result of subthyroidism, there are no symptoms of superpituitarism, so that the pituitary as a whole does not become more active."

On the other hand, the increasing ovarian activity that accompanies the

subthyroidism (especially when full sexual indulgence is allowed), undoubtedly stimulates the anterior lobe of the pituitary. But beyond the assumption of male characteristics, there are no other indications suggestive of acromegaly.

There are certain reasons for supposing that the thyroid-pituitary balance in cattle is not of quite the same order as in man. It is a fact (and to this Swale Vincent [4] also calls attention) that adult herbivora can support life quite well without their thyroid glands; this might perhaps be interpreted as meaning that adult herbivora lose their thyroid at the close of the fertile period of their lives. Cases of nymphomania have not yet reached this stage, but there is no doubt that this disease quickly renders them sterile, and that exhaustion of the thyroid seems to supervene much more speedily and completely than in man. One is consequently not very surprised to find that actual signs of hyperpituitarism dominate the clinical picture in cows, but are relatively little obvious in human beings. But this again is an excursion into endocrinology which may be thought out of place in a purely clinical picture.

CONCLUSIONS.

(1) The foregoing details appear to substantiate the thesis that the so-called nymphomania of cows is a part of the Graves-myxœdema syndrome, the phase of sexual excitement probably marking the passing over from the hyper- to the hypo-active stage of the thyroid. (In this connexion it is interesting to recall Colonel McCarrison's [6] written query at a recent discussion on Graves' disease: "Has suppressed excessive or perverted sexual function anything to do with its onset?" From a purely clinical point of view, excessive coitus in an affected animal tends to force it more quickly into the hyperthyroidic state than the reverse.)

(2) It is further obvious that the whole syndrome from hyperthyroidism to myxœdema can be passed through much more quickly in cattle, and is in many ways more easy to study, since the life of a cow may be terminated at will.

(3) For the cattle breeder and the owner of pedigree herds the subject has great importance, because nymphomania leads to sterility, and thus to great commercial loss.

(4) Nevertheless the subject cannot be studied as it should be except on an experimental farm with proper laboratory accommodation, and it is to be hoped that someone to whom these facilities are available will devote the necessary attention to the subject.

REFERENCES.

- [1] SAVILL, "System of Clinical Medicine," 5th ed., pp. 219-223. [2] COBB, GEIKIE, "The Organs of Internal Secretion," 3rd ed., p. 170. [3] *Ibid.*, p. 106. [4] SWALE VINCENT, "Internal Secretion and the Ductless Glands," 2nd ed., p. 319. [5] *Ibid.*, p. 400. [6] MCCARRISON, Discussion on Exophthalmic Goitre (British Medical Association Annual Meeting), see *Brit. Med. Journ.*, November 11, 1922, p. 913. [7] GURNEY, HELEN M., *Brit. Med. Journ.*, 1915, vol. i, p. 924.

[N.B.—The photographs of cows illustrating this paper have been bound and are kept in the Library of the Royal Society of Medicine for reference.]

100 Pugh: *Graves' Disease and Thyroid Instability in the Cow*

Professor SWALE VINCENT, commenting upon the importance and originality of Mr. Pugh's work, said that the pathology of the disorder could not be regarded as beyond dispute until the thyroid had been studied histologically at different stages. Mr. Pugh, had, however, put forward a very good case for the thyroid. The diagnosis of disease of the ductless glands, especially in minor degrees of derangement, was notoriously difficult. When two or more glands were involved and interrelationships had to be borne in mind, the complications introduced placed the problems practically beyond our powers of analysis in the present state of our knowledge.

Section of Obstetrics and Gynæcology.

President—Dr. T. W. EDEN.

Sarcoma in an Ovarian Dermoid Tumour.

Shown by HERBERT R. SPENCER, M.D.

L. S., AGED 41, married but never pregnant, was admitted to University College Hospital, on December 8, 1915, complaining of discomfort from piles, constipation, and a feeling of weight in the rectum. Menstruation began at the age of 15, was regular, rather profuse, of seven days' duration. There was some frequency of micturition.

The patient was rather thin, the breasts and nipples small. The tongue was furred, the temperature 99° F., the pulse 104. All the patient's teeth were artificial. Nothing abnormal was detected in the chest. By palpation of the abdomen an indefinite tumour could be felt on deep pressure in the pelvic brim. Two piles protruded from the anus.

On vaginal examination the uterus was normal in size, but was pushed forwards by a tumour as big as a double fist. The tumour, wedged in the pelvis, felt as hard as a uterine fibroid; but at its lower end was a softer nodule, as big as half a large grape, which projected from the main outline of the tumour. It was thought that the tumour was an ovarian fibroid. There was no free fluid in the abdomen.

The tumour, which affected the left ovary, was removed entire by laparotomy, on December 11, 1915. There were no adhesions. The pedicle was tied with silk. A small sessile fibroid of the size of a pea on the anterior wall of the body was not removed. The wound was closed by through and through stitches of silkworm gut, buried silk (continuous for the peritoneum and interrupted for the fascia) and horsehair for the skin. The right ovary and tube were normal and were not removed. A large pile was removed by the cautery clamp. The operation lasted thirty-five minutes.

The patient made a good recovery. The highest temperature was 100·4° F., on the night of the operation. The wound healed by first intention and the patient left the hospital on January 5, 1916, weighing 6 st. 10 lb. On January 15, she weighed 7 st.

On February 15 the uterus appeared to be slightly enlarged; but no tumour could be felt on abdominal and vaginal examination. On March 28 she complained of colicky pain in walking; her weight was 7 st. 5 lb.

On May 16 she still weighed 7 st. 5 lb. and felt well. No tumour could be felt in the abdomen or pelvis. On July 11, she complained of pain in the left lumbar region; but no definite tumour could be felt. After this date I did not see her again; but I heard from her doctor that the abdomen increased

102 Spencer: *Sarcoma in an Ovarian Dermoid Tumour*

in size and several tumours became palpable in the abdomen, the largest being in the left lumbar region. She rapidly became weaker and died cachectic on November 16, 1916.

The tumour is a cyst of the left ovary, of flattened oval shape, measuring $5\frac{1}{4}$ in. by $3\frac{3}{4}$ in. by 3 in. and is smooth on the surface, over which several



FIG. 1.—Showing the outer surface of the tumour. Of the two rounded prominences below, the right is a dermoid, the left a sarcoma. Four smaller sarcomata are seen on the surface; two of them have been incised. (Natural size.)

vessels are seen coursing. The cyst wall is thin and translucent. The contents of the cyst were partly fluid and partly solid; the latter, looking like fat globules through the cyst wall, in the fresh state could be seen to float about when the cyst was shaken. On opening the cyst the contents

were seen to be particles of sebaceous material of various sizes floating in a turbid watery fluid. The particles were not rolled into round pill-like bodies. There was no mass of hair in the cyst, but two hairs were found growing from its inner wall.

Fig. 1 is a drawing of the outer surface of the tumour. The Fallopian tube

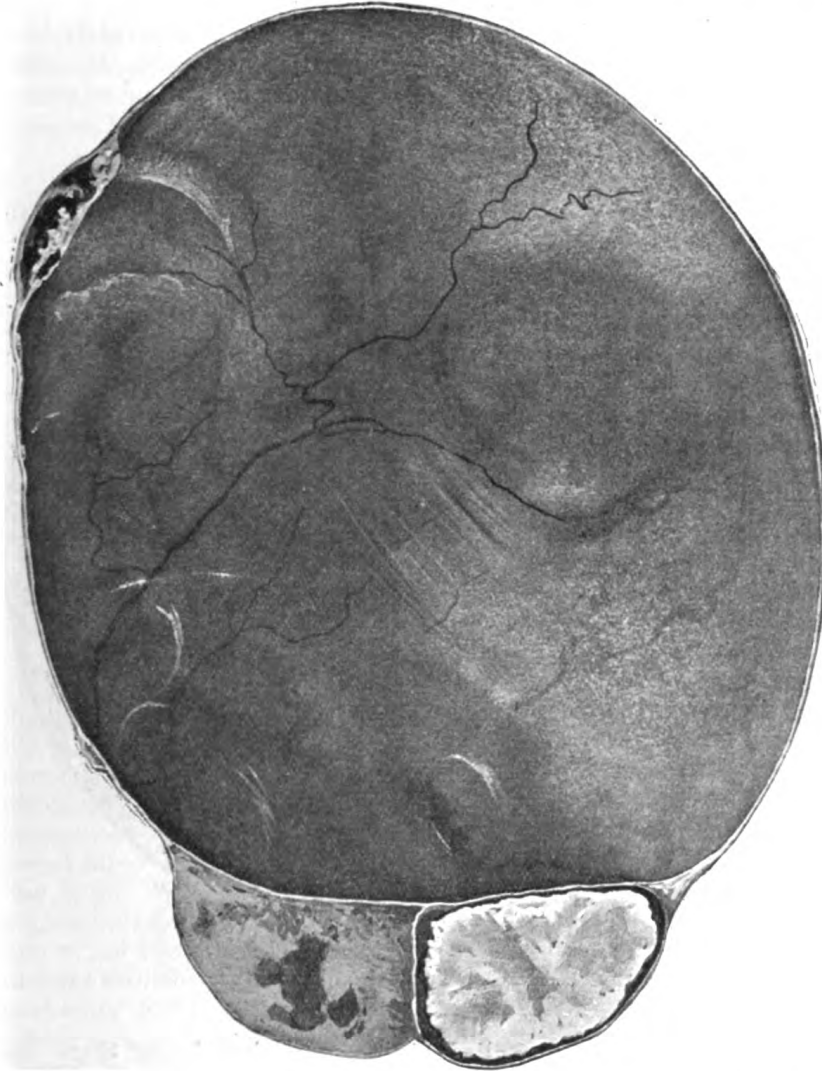


FIG. 2.—Showing the inner surface of the other half of the tumour (not the half shown in fig. 1). Of the two adjacent tumours seen on section below, the white growth on the right is the embryonic process in a dermoid cyst, the grey tumour is sarcoma. Above is another small loculus in the wall partly filled with sarcoma. (Natural size.)

is seen below with two small Kobelt's cysts. Below this are two small ovoid prominences seen in section in fig. 2. Of these that on the right is a dermoid, that on the left a sarcoma. Four other small growths are seen on the surface: the uppermost and largest of these and the lowest (just beyond

the edge of the tubal fimbriæ) have had sections removed to show their solid nature (sarcoma). The inner surface of this half of the tumour also was smooth and showed no solid growth; but there were two hairs about $\frac{3}{4}$ in. in length growing from the inner surface near the line of section. They are of course not shown in the drawing and were the only hairs present.

Fig. 2 is a drawing of the inner surface of the other half of the tumour (not the half drawn in fig. 1). It shows the thin wall with a few vessels coursing over its inner surface. Growing in the wall, apparently in definite loculi, are three tumours, the lower two in adjacent loculi. Of these two, that on the right is the embryonic process of a dermoid attached on its deeper

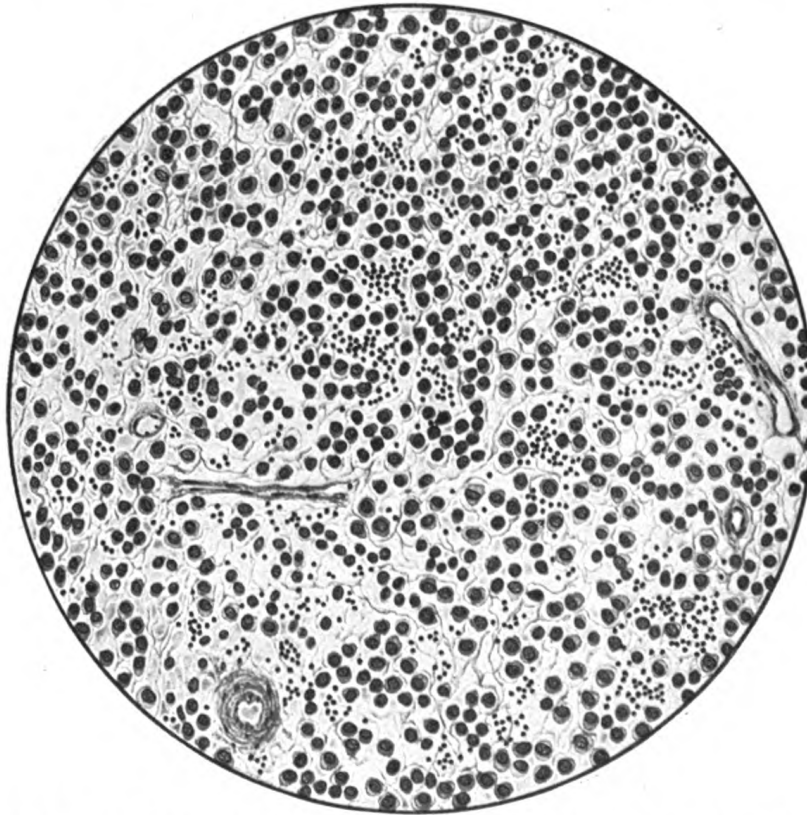


FIG 3.—Large round-cell sarcoma in ovarian dermoid tumour. (Low power.)

surface and lying in a thin-walled cavity somewhat larger than itself and surrounded, except at its base, by a narrow space which was filled by sebaceous material. The darker tumour on the left is a sarcoma, and the smaller tumour at the upper part, which only partly fills the loculus, is of the same structure.

MICROSCOPIC STRUCTURE.

(A) *Of the Main Cyst.*—The presence of sebaceous material and of two hairs growing from its inner wall shows this to be a dermoid. Sections cut from different parts show only a thin fibrous wall lined by a single layer of atrophied flat epithelium. No sebaceous glands or hairs were found in the

sections cut, and to demonstrate them microscopically would spoil the specimen for museum purposes.

(B) *Of the Cyst containing the Embryonic Process.*—The outer wall has the same structure as the main cyst: the embryonic process is covered with keratinized stratified epithelium and its substance consists of fibrous tissue and fat with a few sebaceous glands and hairs. There is no sarcoma in it.

(C) *Of the Solid Growths.*—Four of these have been cut and all show the same structure. The growth is a large round-cell sarcoma with slight small round-cell infiltration. There are a few vessels, some thin-walled and some well developed (see fig. 3). In some sections a few giant-cells are seen; these are not shown in the drawing.

Ruptured Unilateral Solid Cancer of Ovary: Ovariectomy; no Recurrence Six Years later.

By HERBERT R. SPENCER, M.D.

A. M., AGED 37, married, who had had one daughter nine years ago, was admitted to University College Hospital on January 4, 1917, complaining, for two months, of swelling of the abdomen which had suddenly increased three days before admission after an attack of sudden sharp pain. She had been anæmic for two years. Menstruation had begun at the age of 13, was regular every twenty-eight days, lasted five days and was (before her marriage) accompanied by severe pain in the abdomen. For the last two months the periods had lasted two weeks and between the periods there had been a slight bleeding, but no other discharge. There was no history of tumour in the family.

On examination the patient looked pale and ill and the skin had a slightly yellow tinge. The pulse was 136. The blood-count on January 4 showed: Red cells, 2,600,000; hæmoglobin, 50 per cent.; colour index, 0.9; total white cells, 32,000; small lymphocytes, 5 per cent.; large lymphocytes, 2 per cent.; transitional, 1 per cent.; polymorphonuclear neutrophils, 92 per cent. The breasts were flaccid and showed no sign of pregnancy. An ovoid tumour reached up to an inch above the umbilicus. It had a fleshy consistence, as if it were a tense cyst filled with blood. The cervix pointed forwards and admitted the finger for 1 in. Nothing could be felt through it. The uterus was retroverted, apparently slightly enlarged, freely movable. In front of the cervix the lower end of the tumour could be felt high up and movable. The diagnosis was ovarian cyst with hæmorrhage into it.

The tumour (left) was removed on January 6, 1917. A large quantity of bloody fluid was found in the abdomen. The tumour was blue-black in colour, of the shape of a large ovary, measured 9 in. by 6 in., narrower at the inner end, in consistence like a spleen. On the surface at the inner end was a rupture 4 in. long exposing the tissue of the tumour which was infiltrated with blood and could be easily pulped by the fingers. The tumour was removed unopened, the pedicle, which was twisted half a turn, tied and the raw surface stitched over with silk, and the wound closed with through and through stitches of silkworm gut, fine silk (continuous for the peritoneum and interrupted for the sheath of the rectus) and silkworm gut and horse-hair for the skin. The operation lasted forty-two minutes.

The wound healed by first intention and the patient left the hospital quite

well on January 31, 1917, when the uterus and the right ovary (which appeared to be quite normal at the operation) were found to be freely movable. I saw the patient every year till June 21, 1920, when she was quite well, and the uterus and right ovary appeared normal. At that time she lost a good deal at the periods, using seven or eight diapers the first day.

I examined her on May 16, 1923, six and a quarter years after the operation, when she was in excellent health and menstruated normally. The uterus and right ovary were of normal size and freely movable and the abdominal scar was sound.

Microscopic Examination.—The tumour is a medullary carcinoma consisting of masses and tracts of epithelial cells of polygonal shape set in a fibrous stroma which for the most part forms narrow bands, but in places forms considerable tracts. There is extensive congestion of the stroma and hæmorrhages occur both in the stroma and in places in the epithelial masses. In the central parts of some of the larger masses are cystic spaces filled with débris. The hæmorrhages are no doubt due to the torsion of the pedicle. There is very little small round-cell infiltration.

Remarks.—This case, except that the tumour is larger, exactly resembles a case of ruptured medullary cancer of the ovary which I published in the *Proceedings of the Royal Society of Medicine*, 1915, viii (Section of Obstetrics and Gynæcology), p. 61. In that case also only the affected ovary was removed and the patient subsequently had two children and was quite well seven years after the operation.

Stretching of the Epithelium of the Tubal Rugæ by Blood effused into them in Torsion of Pedicle of Ovarian Tumour.

By HERBERT R. SPENCER, M.D.

TORSION of the pedicle of an ovarian tumour is well known to cause infiltration of blood into the tissues of the tumour, the broad ligament and the Fallopian tube. It is also well known that the shape of the epithelium in cysts and glands in various parts of the body is modified by the pressure to which the cavity is subjected, columnar epithelium being changed to cubical or flat epithelium according to the amount of pressure. Alteration of the shape of the epithelium by pressure (from effused blood) outside the cavity of the Fallopian tube, which is new to me, is shown in the specimen exhibited. It was removed on July 2, 1920, together with a multi-locular cyst with twisted pedicle, from a lady aged 51, who made a simple recovery and was quite well two years later. The patient had had several attacks of pain in the abdomen and was very ill (temperature 100° F., pulse 100) with partial intestinal obstruction, owing to adhesions, on her arrival at the nursing home.

The tumour, a universally adherent multilocular cyst weighing 3½ lb., was removed whole. It presented the usual black-red appearance owing to infiltration of blood: the contents and lining of the cysts were also blood-stained and in some of the smaller cysts there was clotted blood.

The Fallopian tube was thickened, especially at its ampullary portion, where it was nearly ½ in. in diameter and dark red from congestion: the fimbriæ were also thickened and congested, the abdominal ostium patent. One inch from the abdominal ostium a pedunculated Kobelt's cyst (or

accessory tube) of the size of a small pea was attached by a short band-like pedicle. $\frac{3}{8}$ in. in diameter, just below the Fallopian tube. A transverse section, magnified six diameters, through the Fallopian tube and the Kobelt's cyst is shown in fig. 1, drawn with the aid of a camera lucida by Mr. Ford. Above



FIG. 1.—Transverse section through Fallopian tube and mesosalpinx of ovarian tumour with twisted pedicle.
(Magnified six diameters.)

and to the left is seen the section of the Fallopian tube, below and to the right the Kobelt's cyst; between the two is the mesosalpinx extensively infiltrated with blood.

The Kobelt's cyst (? accessory Fallopian tube) shows two rugæ, the

epithelium of which is slightly altered by effused blood as described in the Fallopian tube.

The *mesosalpinx* shows vessels distended with blood which is also infiltrated into the tissues. Four parovarial tubules are shown; but, under a high power, seventeen can be counted and they are found to be surrounded

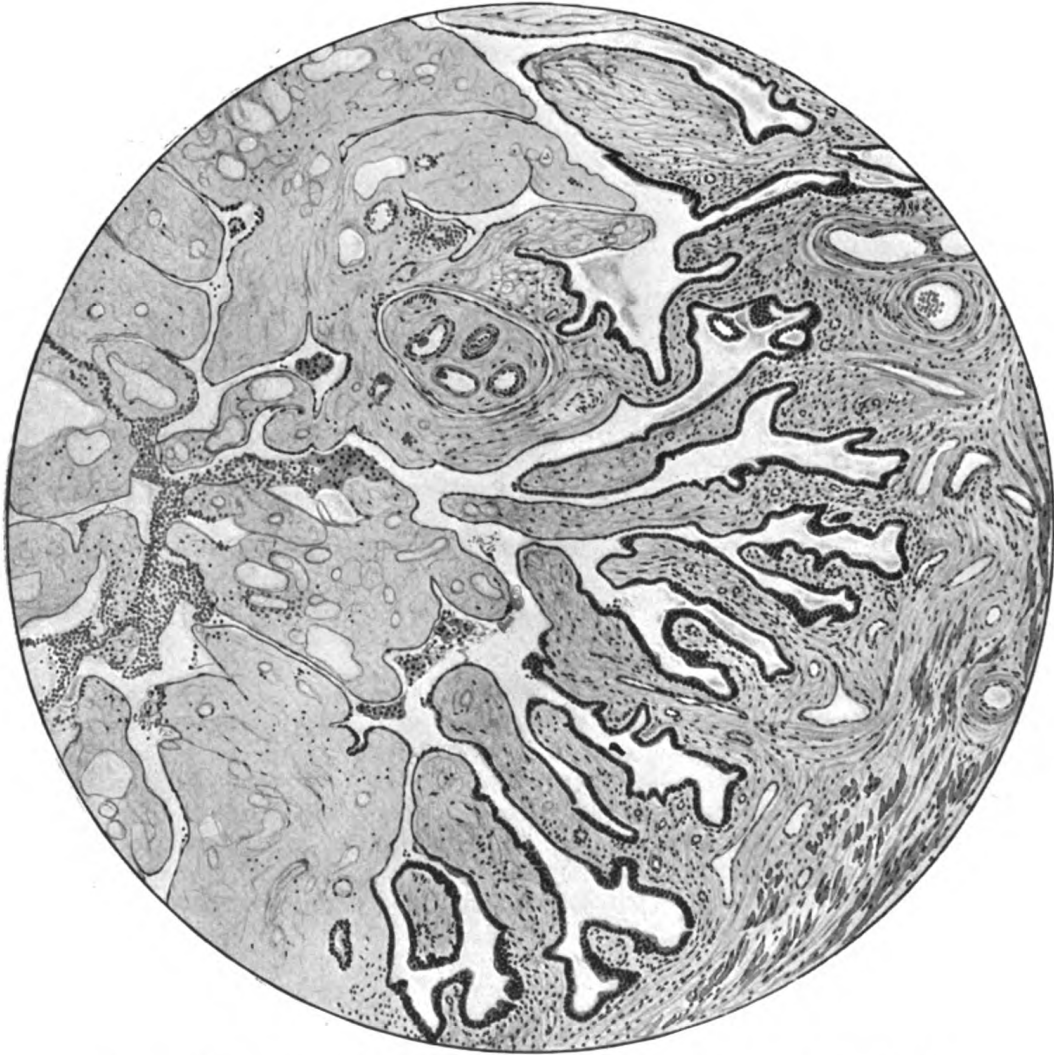


FIG. 2.—Fallopian tube of ovarian cyst with twisted pedicle, showing flattening of the epithelium of tubal rugæ by distension of the rugæ with blood.

by bundles of unstriped muscle. The epithelium of the tubules is of low columnar or cubical shape: in one case the cells fill the lumen.

The Fallopian tube is seen to be filled with several masses which are rugæ extensively infiltrated with blood. It is shown as seen under a 1-in. objective in fig. 2. Some of the rugæ are covered with columnar epithelium, others with

normal epithelium at their bases, but with cubical or flattened epithelium at their extremities, where blood is effused. In some of the rugæ the blood has stretched the epithelium to such an extent as to flatten it like a vascular endothelium.

Further research will be necessary to decide whether this is a common occurrence in cases of torsion: in another case examined by me it had not occurred. Perhaps it is the result of slight repeated torsion.

It seems to point to great elasticity of the epithelium of the Fallopian tube and to its firm adherence to the subjacent tissue.

Carcinoma of a Prolapsed Cervix in a Woman, aged 77.

By HENRY RUSSELL ANDREWS, M.D.

A LITTLE over thirteen years ago¹ I showed before the Section a specimen of carcinoma of the cervix of a prolapsed uterus in a single woman aged 88, treated by vaginal hysterectomy. I then commented on the fact that while chronic ulceration of the prolapsed cervix was very common, carcinoma of the prolapsed cervix was surprisingly rare. It was the first case I had seen. In the discussion that followed Dr. McCann had said that he had never seen carcinoma of the cervix with prolapse. Dr. (now Sir George) Blacker had said that he had seen one case, and thought that "in view of the important part that chronic irritation had been shown to play in the production of carcinoma, it was a most curious fact that carcinoma of the cervix was so uncommon in cases of prolapse." To-night I am showing the only other case I have seen.

A multiparous woman, aged 77, was sent to me in May of this year (1923), complaining of bleeding twenty-six years after the menopause. Sixteen years ago she was given a pessary on account of prolapse, and wore it for four years. For the last ten years she had worn a tight diaper to prevent the uterus from coming outside. For twelve months there had been almost daily bleeding, at first slight, lately more profuse. For five weeks she had been in bed on account of the bleeding. The vaginal portion of the cervix was outside the vulva, covered uniformly with red friable tissue, which bled readily. The supravaginal portion of the cervix was much elongated, and the atrophied body of the uterus was situated comparatively high up. I thought she had carcinoma of the cervix, but as carcinoma with prolapse is so rare I cut out a small portion for microscopical sections. The pathologist's report was: "Solid trabecular polygonal-celled carcinoma." The section is shown under a microscope.

Vaginal hysterectomy was performed—a very easy operation in this case. I had intended to repair the pelvic floor, but, at the request of the anaesthetist, I finished the operation as quickly as possible, removing a good deal of vaginal wall and packing the cavity lightly with gauze. The patient made a very good recovery.

The uterus has shrunk so much that the elongation of the supravaginal cervix is not well seen.

DISCUSSION.

Dr. CUTHBERT LOCKYER agreed with Dr. Russell Andrews' statement that carcinoma was rarely found associated with the major degrees of prolapse. He had never seen a cancer of the cervix lying outside the vulva, but had operated for primary carcinoma of the vagina in a case of procidentia, and the specimen was preserved in the Museum of the Charing Cross Hospital Medical School.

¹ *Proceedings*, 1910, iii (Sect. Obst. and Gyn.), p. 161.

Dr. T. W. EDEN (President) said that he was not satisfied that a proper statistical inquiry would show that the incidence of cancer of the cervix was less in women with a severe degree of prolapse than in those in whom the cervix occupied its normal position in the vagina. He could think of no reason why it should be less frequent in a prolapsed uterus, and clinical impressions of frequency were often fallacious.

Dr. ANDREWS (in reply) said that he was still of the opinion, with all due deference to the President, that carcinoma of the prolapsed cervix was much rarer than one would expect, seeing that carcinoma of the cervix and prolapse were both common conditions.

Mr. VICTOR BONNEY, M.S., read a paper on "Diurnal Incontinence in Women," which will be published in the *Journal of Obstetrics and Gynæcology of the British Empire*.

The Treatment of Dysmenorrhœa: An Analysis of 100 Cases.

By LEONARD PHILLIPS, M.S., M.B., B.Sc.Lond., F.R.C.S.Eng.

THIS short communication is based on the treatment of 100 cases of dysmenorrhœa without signs of organic disease. The treatment was conducted almost entirely at the Women's Hospital, Soho Square, and was chiefly non-operative. An effort was made to divide the cases into definite clinical types. The subject matter falls conveniently into three divisions:—

- (I) Interesting features of the case papers.
- (II) The treatment in general.
- (III) Discussion of four clinical types and their appropriate treatment.

(I) INTERESTING FEATURES OF THE CASE PAPERS.

(1) *Occupation*.—Practically always sedentary—typist, clerk, leatherworker, machinist, &c.

(2) *State*.—Sixty-seven were spinsters, thirty-three were married, but all the latter were sterile, though married for periods varying from one to sixteen years.

(3) *The Menstrual Loss*.—Only a minimum had an average loss lasting three or four days. Most were under three or over five days. More were associated with excessive rather than diminished loss. Some had fortnightly excessive loss, others intermittent menstruation. Menorrhagia was most common, and was usually associated with a moderate degree of pelvic hypoplasia, a condition of arrested development of the genital organs.

(4) *Pain*.—This was not at all constant in character, position or duration. Sometimes iliac in position, sometimes hypogastric or sacral, it occurred before, during or immediately after the flow, sometimes all three, and was commonly associated with the passage of clots, the pain diminishing or ceasing when the clots were passed. It was impossible to say that one type of pain characterized the cases in regard to time of onset, duration, site or character of the pain. Classification according to pain characteristics was found to be impossible.

(5) *State of Bowels*.—In half the cases constipation was severe and constituted a definite complaint.

(6) *Type of Patient*.—The majority of the patients were poorly developed, with weak abdominal muscles, faulty posture and breathing, anæmia or visceroptosis.

(7) *Associated Symptoms*.—Most of the patients had one or several of the following symptoms: Headache, nausea, vomiting, frequency of micturition, and sometimes diarrhoea.

(8) *Pelvic Examination*.—Rectal examinations were made on all unmarried, and vaginal examinations on all married women. In rectal examination one is able to reach beyond the bases of the broad ligaments and explore the pelvis more satisfactorily than is often possible by vaginal examination, especially in nervous women. In half the cases there were signs of arrested development of the genital organs; a small acutely anteflexed uterus, or a small retroverted uterus, with a short anterior lip to the vaginal cervix, and a short anterior vaginal wall; poorly developed labia and breasts, and the male type of pelvis and pubic hair. Though sometimes associated with scanty menstruation, menorrhagia or fortnightly losses were far more frequent accompaniments. It is not commonly recognized how common in the young adult female is this association of dysmenorrhoea and menorrhagia with pelvic hypoplasia.

(II) TREATMENT IN GENERAL.

(a) Fifty cases were treated with extracts of ductless glands either alone or in combination with antispasmodics.

(b) Forty cases were treated with antispasmodics alone.

(c) Ten cases were treated with sedatives.

Only ten of the hundred failed to be relieved and eventually came to operation.

Details of Treatment.—Fifty cases were treated by organotherapy. Extracts of thyroid, corpus luteum, anterior lobe of pituitary, and mixed gland products were used separately and effectively. The majority were treated with hormotone as follows:—

(1) Mist. cascara co. daily throughout the month.

(2) Hormotone, 1 tabloid t.d.s. for seven days before and during the period.

(3) If unrelieved the following every three hours till relief was obtained (up to four doses).

Phenazone	5 gr.
Ext. caulophyllin liq.	15 minims
Caffeine	5 gr.
Aq. chlorof.	ad	1 oz.

or—

Phenazone	}	4 gr. of each. Repeated three-hourly up to four doses in cases where pain was severe
Aspirin		
Pyramidon		
Caffeine		

Sometimes Tinct. Belladonnæ 5 minims was added to the above mixture, and sometimes other antispasmodics such as benzyl benzoate were substituted. Sometimes atrop. sulph. $\frac{1}{100}$ gr. as a tabloid was used.

Forty-six of the fifty cases so treated were relieved. In some cases pain was not abolished, but diminished, and in all the forty-six cases the patients were enabled to work throughout the period in comparative comfort. Often when the tabloids were omitted the painful periods returned, only to disappear at the next period when the hormotone was re-administered.

The amount of relief obtained varied with the individual, but it was always definite and in the majority of cases sustained. Of the forty cases treated with antispasmodics alone, thirty-four were benefited, and only six came to opera-

tion. Ten cases were treated between the periods with bromides and salicylates combined with laxatives; and during the periods small nightly doses of luminal were given. All were relieved.

In treating these cases an attempt was made to correlate and interpret the available information and to evolve a plan of treatment based on the existence of definite clinical types.

(III) DISCUSSION OF FOUR CLINICAL TYPES AND THEIR APPROPRIATE TREATMENT.

Clinical Type (1).

Dysmenorrhœa is frequently a disease of faulty hygiene, upbringing and surroundings. A large proportion of these patients conformed to this definite type. They were thin, anæmic subjects, poorly developed, with weak abdominal muscles, absent abdominal breathing and faulty posture; they were constipated, had visceroptosis or a bad circulation, and followed sedentary lives with little exercise and fresh air. Naturally in such cases the uterus is often arrested in its development, and has a feeble musculature easily exhausted if given much expulsive work to do. An exhausted muscle forced to continue contractions gives rise to cramp and colicky pain, true alike of the heart, the gastrocnemius and the uterus. Constipation necessarily has a bad influence, for a mass of fæces in the pelvis acts very much as a pelvic tumour, increasing congestion.

The Treatment of these Cases.—Often complete relief can be obtained by improving the faulty conditions above outlined.

(1) *Establish the correct Mental Attitude.*—The patient must realize that the period is natural and normal. She is not ill, and must bathe and take exercise as usual, unless the pain is very severe.

(2) *Correct Clothing.*—No constricting garments likely to impede the pelvic circulation should be worn, and stays (if worn) must not do the work of the abdominal muscles.

(3) *Correction of Constipation.*—Plenty of water to be drunk, and fruit to be eaten. Over-eating is bad, but enough food should be taken to give bulk to the intestinal content. Exercise in the fresh air, abdominal kneading night and morning, and an attempt to empty the bowels at the same time every day should be aimed at. Purgatives only *temporarily* if possible.

(4) *Exercise and Exercises.*—Light Indian clubs, tennis, hockey, walking. The object is to increase the tone of the muscle in the abdominal wall, the bowel, and the uterus, and by improving the general and local circulation to produce improved nutrition and muscular development. Many patients do not realize that the abdominal wall contains muscle. They allow their corsets to do the work of the recti and obliques, inducing visceroptosis, constipation and defective abdominal breathing. They must be taught: (1) To produce hardening of the abdominal wall at will; (2) breathing exercises night and morning before a mirror, standing in the correct posture; (3) exercises to develop the abdominal muscles, such as lying flat on the back and raising the body into the sitting posture, maintaining the legs and heels in contact with the bed all the time. The patient's leisure must not be spent in emotional recreation, such as the theatre, kinema, or novel reading, but in walking or exercising in the fresh air. Make this type of patient healthy and strong, and as a rule the dysmenorrhœa is cured. It is not so common to see a robust girl suffering from dysmenorrhœa. Too much emphasis cannot be laid upon the

importance of faulty upbringing, surroundings and development, as a causative factor in dysmenorrhœa, and the need of improving these as a first step in treatment.

Clinical Type (2).

There is a type of dysmenorrhœa case in which the disturbance is functional and allied somewhat to migraine. In addition to menstrual pain these women complain of general rather than local symptoms. Headache and nausea are common accompaniments of the pain. They are frequently sensitive, nervous, worried women with eye-strain and constipation, and the dysmenorrhœa is one symptom in a complex which Freud would have designated as the expression of an unsatisfied desire, and which lately has been described as an anxiety-neurosis. Whenever the general symptoms are more marked than the local, general treatment is more likely to be successful. Ten such cases in this series were treated successfully by sedatives as follows: (1) Bromides and salicylates between the periods; (2) luminal, 1 to 1½ gr., in cachet or powder nightly during and just before the period; (3) general hygienic measures as already outlined, with attention to bowels and eyes.

Clinical Type (3).

There is a third type of case in which the symptoms and signs suggest some form of obstruction as the chief causal factor. Though one of the earliest ætiological theories of dysmenorrhœa, it has been abandoned by many, because it is maintained that flexions and narrow canals cannot obstruct the flow of one-third of a drop of blood per minute—the rate of flow, if 4 oz. of blood is lost in four days. It has been shown that in cases of dysmenorrhœa a sound can often be passed quite easily, whereas this may be impossible in subjects without dysmenorrhœa.

But as a set-off against these arguments it must be remembered that in cases of dysmenorrhœa the blood may not exist *in utero* as fluid but as clot, when obstruction is possible even with a normal internal os and cervical canal. We know that flat, thin casts are passed without pain, whereas solid rolled up casts cause considerable pain.

Secondly, in some cases of dysmenorrhœa the pain diminishes or ceases entirely when clots are passed, though intense pain exists before the clots are passed.

Thirdly, the pain resembles ureteral and biliary colic in its intensity and sudden onset, subsiding rapidly when the clots are expelled just as ureteral and biliary colic suddenly subside when the stones are expressed into the bladder or duodenum. Finally, though obstruction as a cause is decried, gynæcologists continue to employ cervical splitting operations such as anterior hysterotomy, for the relief of pain, though it is reasonable to assume that these operations are based on an obstructive view.

Fourthly, in three cases of hysterectomy for dysmenorrhœa (in this series) performed during the menstrual period the uterus was found to contain clots. Whether the intra-uterine clot is normal but is not dissolved because of some endometrial defect, or whether clotting *in utero* is primarily pathological, are problems which must be solved before the cause can be removed and the pain cured by rational treatment. But it is reasonable to assume that a uterus with poor musculature may be unable to expel rolled up casts or clots through even a normal canal and os, without contractions which ultimately become painful, ceasing when the solid body is expelled and the tired organ rests, and

recurring once more when the uterus is again given the same task. It may be that the imperfectly developed uterus, so commonly found in these cases, is responsible in some way for the formation of the clot before expression or for its failure of dissolution.

Clinical Type (4).

There is a type of patient who complains of pain in one or other iliac region either alone or before the central pain, and in whom there exist *signs of arrested development of the genital organs*. Menorrhagia is just as common as scanty flow, because an undeveloped endometrium and musculature are linked up with a normal ovarian stimulus. Often there is nothing in the history to suggest obstruction, and clots may be absent. The obvious treatment in such cases is to stimulate development of the uterus, and this is often successful in abolishing the pain.

In addition to the general hygienic treatment already outlined, good results can be obtained in these cases by: (1) Organotherapy; (2) electrical treatment.

(1) *Organotherapy*.—Ovarian extract, corpus luteum, thyroid, anterior lobe of pituitary, mixed gland (B. W. and Co.), and hormotone have each been used. Why the latter should have proved the most valuable in this series of cases it is difficult to say. It consists of extracts of ovary, thyroid, pituitary and testis. We know that hypo-thyroidism, hypo-pituitarism, and hypo-oöphorism may all be associated with pelvic hypoplasia, a condition observed in one-half of the cases in this series. It may be, that in a "Gatling gun" prescription such as hormotone, containing all three extracts—one extract may hit the mark where the others fail. By estimating the basal metabolism rate it is possible to ascertain the existence of hypo-thyroidism even in the absence of clinical signs, but moderate ovarian and pituitary deficiency may be more difficult to determine, and herein lies the value of combined extracts. It is interesting to speculate upon a possible part played by the extract of testis, for this extract alone or in combination with extract of prostate is sometimes successful in dysmenorrhœa cases.

Some years ago Bland-Sutton pointed out a histological resemblance between the lining cells of the uterus and those of the large intestine. One of the chief functions of the large intestine is absorption.

Arthur Thomson, in an admirable paper in the *British Medical Journal*,¹ last year, marshalled a formidable array of arguments in support of the view that the endometrium is mainly absorptive in function. It has often been maintained that seminal fluid is drawn into the cervix or body of the uterus and it may be that such fluid is absorbed and exercises a beneficial influence on genital development and conception. If this is so, it would throw some light on the action of testicular and prostatic extracts.

(2) *Electricity in Dysmenorrhœa*.—Electro-therapeutists frequently use electrical stimulation as a treatment for dysmenorrhœa. Gynæcologists, as a rule, prefer a method of treatment which does not take the patient out of their hands. A few of the cases in this series were benefited by electrical treatment, not personally administered. High-frequency currents act by heating up the deeply seated organs, so that there results: (1) Dilatation of the vessels; (2) relaxation of spasm and inhibition of tone; (3) improvement in blood supply and consequent improvement in nutrition and growth.

¹ "Problems involved in the Congress of the Sexes," *Brit. Med. Journ.*, 1922, i, p. 5.

Where an obstructive element is suspected relaxation of spasm can be induced by intense currents over a short period. Where pelvic hypoplasia exists, improved nutrition and development can be secured by less intense currents over a longer period. Courses of electrical treatment sometimes result in enlargement of the uterus, in disappearance of the dysmenorrhœa, and in married women, in conception.

Of the ten cases in which medical treatment failed, two were cured by curettage, and one by anterior hysterotomy. The latter is a very difficult operation, as the cervix cannot be pulled down sufficiently as a rule.

Hysterectomy was performed in two cases. Histologically, both uteri showed marked arteriosclerosis. The cavities, it was observed, contained clot. The remainder—very severe cases—were sterilized by radium (100 mg. for twenty-four hours).

A follow-up of 100 cases treated by operation was instituted as a comparison with this series of 100 cases treated medically. It was found that 25 per cent. were cured, 25 per cent. relieved and 50 per cent. were unaffected. The best results were in cases treated by curettage. The follow-up results are certainly inferior to those obtained in this series of 100 cases treated medically.

It is not suggested that the above figures are necessarily representative of the usual results of surgical treatment, but I believe most gynæcologists feel that the surgical treatment of dysmenorrhœa without physical signs is on the whole disappointing.

DISCUSSION.

Dr. T. W. EDEN (President) thought that this was an admirable piece of clinical work, and the author deserved thanks for the patience and pertinacity with which he had attacked this very difficult problem. To treat with success 90 per cent. of cases of dysmenorrhœa by therapeutic measures only was a record of which Mr. Phillips might well be proud.

Mr. VICTOR BONNEY said he demurred at the frequency with which a developmentally small uterus was diagnosed. In actual fact it was extremely uncommon to find a uterus up which a sound could not be passed the normal distance, or to find, on inspection through an abdominal incision, a uterus which was really small. Consulting-room methods of examination had very finite limitations. The really small uterus was not infrequently associated with the most violent menorrhagia. He had no case for or against organotherapy, but it was as well to remember that a number of treatments had from time to time been vaunted as a cure for dysmenorrhœa. Pioneers in a new treatment always seemed to achieve phenomenal success, which unfortunately was rarely maintained by those that followed them. The late Dr. Herman's advocacy of guaiacum as a cure for dysmenorrhœa was a case in point. Mr. Phillips had, nevertheless, taken much pains in treating a very difficult class of case.

Dr. H. RUSSELL ANDREWS said that Mr. Phillips' very interesting paper contained many points for discussion, but there was not time to touch upon more than two. He asked what were the ages of the three patients treated by radium. Was Mr. Phillips absolutely satisfied with the result of treatment in these cases? Mr. Phillips was to be congratulated on his high percentage of successes. He (Dr. Andrews) thought that this was due to his patient treatment of girls with dysmenorrhœa for many months not only with drugs but with good advice with regard to exercise in the open air, bedroom exercises, abdominal massage, and management of the bowels. Improvement of the general health by these hygienic measures took time, and most people were too impatient and advised operative treatment much sooner than Mr. Phillips did.

Dr. EVERARD WILLIAMS asked Mr. Phillips whether any cases of "middle pain" were included in his series, and, if so, what had been the response to medical treatment? He also asked Mr. Phillips whether he had observed any marked difference in response to organotherapy in these cases depending upon the age of the patient?

Dr. BRYDONE thought dysmenorrhœa occurred mainly in those backward in development, but not from underfeeding. In his experience the large majority of adolescent cases, or those aged under 22, got well, but over that age operative treatment was eventually required. He ascribed the success of the many and constantly varying treatments in some measure to suggestion, but could endorse the success in treatment by pluriglandular extracts.

Mr. LEONARD PHILLIPS (in reply to the President) said that one half of the cases were associated with clinical conditions suggesting a moderate degree of genital hypoplasia, 40 per cent., with the painful passage of clots, and 10 per cent. with symptoms suggesting a functional origin. The majority of the cases required and had hygienic treatment along the lines already mentioned. There was naturally some overlapping in these types—but such a classification allowed of an attempt at rational treatment. Sometimes the reaction of the patient was immediate and most surprising, but sometimes six months elapsed before much improvement was noticed, and prolonged and detailed treatment was absolutely essential.

In reply to Mr. Bonney, he stated that he was opposed to the operative treatment of congenital retroversion associated with dysmenorrhœa. Operative replacement of the uterus failed to cure the pain. He thought the tendency now was to regard these small retroverted uteri in nulliparous women as of little significance as displacements, but of some importance as conditions of arrested development. Treatment founded on this assumption was more likely to be successful than operative treatment of the displacement.

Dr Russell Andrews had asked the age of the patients treated by radium, and the nature of the results obtained. The women so treated were over 32 years of age, and had received prolonged and unsuccessful treatment both medical and surgical; 100 mg. of radium had been inserted into the uterus for twenty-four hours. The periods had ceased entirely, and no pain had occurred since. The practice of using radium in the case of young women carried with it the risk of inducing a subsequent sterility; but in severe and intractable cases of dysmenorrhœa this was a justifiable risk to incur if the patient was already aware of it, and was willing to take it. Moreover, dysmenorrhœa was associated with sterility in a fair proportion of cases.

Dr. Everard Williams wished to know if cases of middle pain were included, and how the response to treatment varied with the age of the patient. No cases of middle pain were included. The earlier the patient sought relief and the younger she was, the better were the results obtained. The cases requiring operation were all patients over 30 years of age, and who had been sufferers for years.

In bringing this subject before the Section, he had hoped that it would produce a discussion from which interesting facts would arise, possibly of value in the treatment of this very common and distressing ailment. So little was known of its pathology and so much still required to be done before a rational treatment could be instituted.

PROCEEDINGS
OF THE
ROYAL SOCIETY OF MEDICINE

EDITED BY
SIR JOHN Y. W. MACALISTER
UNDER THE DIRECTION OF
THE EDITORIAL COMMITTEE

VOLUME THE SIXTEENTH

SESSION 1922-23

SECTION OF ODONTOLOGY



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1923

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SECTION OF ODONTOLOGY.

CONTENTS.

October 23, 1922.

W. R. ACKLAND, M.R.C.S., L.D.S.Eng., M.D.S.Brist.	PAGE
President's Address: Some Considerations for Preventive Dentistry ...	1

January 22, 1923.

DISCUSSION ON "DENTAL SEPSIS AS AN ÆTIOLOGICAL FACTOR IN DISEASE OF OTHER ORGANS."

Sir WILLIAM WILLCOX, K.C.I.E., C.B., C.M.G., M.D., F.R.C.P. (p. 7), Sir KENNETH GOADBY (p. 17), Dr. WILLIAM HUNTER (p. 19).

February 26, 1923.

ADJOURNED DISCUSSION ON "DENTAL SEPSIS AS AN ÆTIOLOGICAL FACTOR IN DISEASE OF OTHER ORGANS."

Mr. WILLIAM HERN (p. 22), Dr. C. KEMPSTER (p. 23), Mr. COLIN KEAY (p. 24), Mr. J. G. TURNER (p. 24), Colonel McKECHNIK (p. 26), Mr. CHARLES LEONARD GIMBLETT (p. 27), Dr. GRAHAM LITTLE (p. 27), Mr. W. R. ACKLAND (President) (p. 28), Mr. A. T. PITTS (p. 29), Mr. CRIBB (p. 30), Sir FRANK COLYER (p. 31), Sir WILLIAM WILLCOX (in reply) (p. 32).

November 27, 1922.

DISCUSSION ON INFECTIONS OF THE TEETH AND GUMS IN THEIR RELATIONSHIP TO THE NOSE, THROAT, AND EAR.

Dr. P. WATSON-WILLIAMS (p. 35), Sir JAMES DUNDAS-GRANT (p. 39), Mr. HERBERT TILLEY (p. 40), Mr. MARK HOVELL (p. 40), Mr. A. T. PITTS (p. 41), Mr. W. STUART-LOW (p. 41), Mr. E. D. D. DAVIS (p. 41), Dr. WATSON-WILLIAMS (in reply) (p. 42).

March 26, 1923.

BERNARD GRELLIER (M.C.), L.R.C.P.Lond., M.R.C.S., L.D.S.Eng., D.M.R.E.Camb.	
Case of Multiple Dentigerous Cysts	43
J. HOWARD MUMMERY, C.B.E., F.R.C.S.	
Case of Multiple Dentigerous Cysts	44

April 23, 1923.

EVELYN SPRAWSON (M.C.), L.R.C.P.Lond., M.R.C.S., L.D.S.Eng.	PAGE
On the Vascular Supply of the Enamel Organ of <i>Felis domestica</i> ...	47

June 25, 1923.

Professor ARTHUR HOPEWELL-SMITH, M.R.C.S., Sc.D.	
(1) Two Odontomes	55
(2) Some Observations on the Histology, Physiology and Pathology of the Dental Pulp	58

April 23, 1923.

GERALD HARBOROW.	
A Case of Unerrupted Incisors and Canines in a Male, aged 59 ...	73

March 26, 1923.

MAY MELLANBY.	
The Effect of Diet on the Resistance of Teeth to Caries	74

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Section of Odontology.

President—Mr. W. R. ACKLAND, M.R.C.S., L.D.S.Eng., M.D.S.Brist.

Some Considerations for Preventive Dentistry :

PRESIDENT'S ADDRESS.

By W. R. ACKLAND, M.R.C.S., L.D.S.Eng., M.D.S.Brist.

I HAVE have been thinking a good deal about preventive dentistry : a patient called my attention to a text in the Vulgate : Deuteronomy xxxiv, 7 :—

"Moyse centum et viginti annorum erat quando mortuus est ; non caligavit oculus ejus, nec dentes illius moti sunt."

"Moses was 120 years old when he died ; his eye was not dimmed, nor were his teeth loosened." The latter fact interests us especially. Obviously, from its being put on record, this was unusual. Indeed we may assume that the loss or loosening of the teeth in old age was the rule even in those early days. You remember that wonderfully poetical description of the advance of age in Ecclesiastes, "The grinders cease because they are few."

Sir Thomas Horder in his admirable paper on "Dental Sepsis," read before this Section in 1914, suggests that pyorrhœa is simply the pathological hastening of the natural loss of teeth in old age.¹

I wish we knew more about the life of Moses and his habits, so that we might have a chance of discovering why his teeth escaped even the "pathology" of old age. Some further light might thus be thrown on the causation of pyorrhœa. I think we may assume that he possessed a tremendous vitality. His history shows him to have been a man of strong character. I remember that our revised version says "nor his natural force abated," instead of "his teeth were not loosened."

You understand that I mention this curious text in the Vulgate simply because it started my thoughts about preventive dentistry, and as to how far we had fallen away from such a standard as this (whether set by Moses or by any of our prehistoric ancestors), or for that matter, from the standard in this respect of many uncivilized races of to-day. I find no evidence that Moses cleaned his teeth, yet presumably there was no caries. The same remark applies to prehistoric skulls. There is no evidence of cleaning, and only evidence of occasional caries or pyorrhœa. What a contrast with to-day—a distinguished novelist says there are more dentists and fewer teeth!

Then I tried to imagine what were the habits, mode of life, and environment of the possessors of these good teeth, in order to see how they differed from those of to-day ; and I came to the conclusion that the contrast in mode

¹ *Proceedings*, 1914. vii (Sect. Odont.), p. 66.

2 Ackland: *Some Considerations for Preventive Dentistry*

of life was just as great as the contrast in the condition of the teeth. I think therefore that we may learn something as to the causes of our degeneracy.

To begin with, take (1) *the question of their food*. I suppose a good deal of it was uncooked—fruit and vegetables certainly, and it was very fresh—so I assume there was a full supply of vitamins.

(2) *Their shelters or houses* were very well ventilated and constantly changed. They were mostly nomads, so that their habitat never grew stale. Those of you who keep animals, especially poultry, know how important this is.

(3) *Their clothing* was, to our modern notions, very inadequate, to say the least—so that the sun and air had good access to their bodies.

(4) They used little or no artificial light, so I assume they had much more sleep than we do. I lay great stress on sleep, first as a restorer of mind and body, and secondly because of the recumbent position, and the consequent removal of strain on the heart and circulation generally.

(5) *Early marriage* undoubtedly had its beneficial effect in two ways. First, in the production of healthy children, and secondly, on the parents themselves in promoting "the harmony of the hormones."

(6) *The children* were never "pent up" in schoolrooms, nor were their brains put under competitive pressure. So that the nervous system was free to exercise its trophic influence on the proper development of their bodies.

(7) Finally, we must take into consideration the fact that the rigorous conditions of life brought into uncompromising action the law of the *survival of the fittest*—since there was little or no medical skill to keep alive the weak and ailing. As a corollary, too, *hereditation* had the best material to work upon.

What is the difference to-day? Just the difference in all these details of life, which civilization has brought about.

(1) *Our food* is cooked to the loss of vitamins, till the necessity for exercising our teeth is gone.

(2) *Our houses* are permanent habitations, very frequently with a long history of previous tenants. Very often ill-ventilated, and generally grouped together on stale soil. We change the living-ground of our animals (we say it goes "sour" in Somerset) but we do not take the same precautions in our own case.

(3) *Our clothing* it is difficult to criticize. We can scarcely get back to woad! though I remember how much better my horses were when they were out at grass, than when they were "rugged up" in the stables!

(4) *As to the importance and value of sleep* I hold very strong opinion, especially in regard to children. The children of the poor in cities do not get sleep enough, one sees them about the streets at all hours. And even in their houses, the noise and overcrowding can scarcely conduce to proper rest.

(5) *Of late marriage and resulting children* I shall not say much, nor do I propose to go into the subject of the "harmony of the hormones." But I shall hope to hear something, during my year of office, of the influence of the ductless glands on the calcification of teeth.

(6) I have already hinted at the effects of the pressure of education on the growing child. There is not the slightest doubt in my mind that the pre-occupation of the nervous system in ill-nourished children is mischievous.

(7) And I need scarcely remind you that the doctors' skill to-day is pre-

venting the full operation of Nature's law of the *survival of the fittest*. Hence hereditation is perpetuating the ill-effects of civilization.

So far my remarks seem to be dealing with the effects of habits, mode of life, and environment on general health, rather than on the teeth in particular. Still, I am sure you realize from your knowledge of the causes of caries and pyorrhœa, that my observations are not entirely irrelevant.

We learnt at the outset that so far from suffering from pyorrhœa, Moses died with his teeth unloosened. The marginal reference in our Revised Version, offers the alternative translation "nor his moisture fled." So we have the three versions: (a) "his teeth were not loosened"; (b) "nor his natural force abated"; (c) "nor his moisture fled." It is a picture of man at his best, with his vascular system perfect. His terminal capillaries, whether of eyes or dental periosteum, were in full working order. I am inclined to attribute this to the excellent general health resulting from his mode of life. For my clinical observation leads me to lay more stress on the systemic than on the local causes of pyorrhœa. It may be simply the question of the amount of vital resistance, but it comes to the same thing. You will see later that I am inclined to think that deficiency in vitamins plays its part too in the production of pyorrhœa.

It is difficult to retrace our steps and undo the ill-effects of civilization altogether. But can we do anything at all? Well, preventive medicine is doing its best, and I hope there is a chance for preventive dentistry.

The two evils we have to eradicate are *caries* and *pyorrhœa*, and the forces fighting against us are: First, *hereditation*, which may predispose to caries by a legacy of (a) poor-quality teeth, (b) crowded jaws leading to irregularity, (c) congenital diathesis such as syphilis, gout, rheumatism. Secondly, *habits, mode of life, environment*. Of these two, the latter group is obviously the most important. It is the real evil, for without it hereditation would hand down no legacy of trouble. And conversely, if we tackle successfully the evils of our environment, heredity will not only right itself, but be on our side!

I have great hopes for the future based on an experience I had years ago. A rich patient arranged for us to form a dental clinic on her model estate. We were impressed at the start, by the fact that the teeth of the children, though showing the usual neglect and want of cleaning, were fairly free from decay; even though the teeth of the parents were in many cases bad. They lived under the best conditions, their parents being a picked community of sober habits, with good wages. So they had good homes, good regular food, good air and plenty of sleep. Pickerill would say they probably walked to school munching apples after their meals. I expect they did, as there were orchards all round them. The best of milk was available, and plenty of eggs and fresh vegetables.

So far I have been giving you some impressions of my own—the ideas of a mere clinician—gleanings based on the experience of every-day work, and not derived from scientific investigation. Now let us turn to the scientists and see what has been done to help us.

The experimental work of Pickerill was, I consider, the most serious attempt up to ten years ago, in the direction of preventive dentistry. In the introduction to his "Prevention of Dental Caries" (1912), he boldly attacked the assumption that dental disease was inevitable, and then proceeded to upset a good many of our established ideas as to care of the teeth and as to dietaries.

Broderick¹ has been demonstrating the share which the ductless glands

¹ *Proceedings*, 1921-22 (Sect. Odont.), p. 22.

4 Ackland: *Some Considerations for Preventive Dentistry*

take in the calcification of teeth, and that dental decay therefore is due to endocrine insufficiency.

The investigation of ultimate causes has been carried more deeply by Dr. and Mrs. Mellanby and others, in their experiments with the accessory food factors or vitamins. Mrs. Mellanby's¹ investigation of the influence of diet on teeth formation, has led to some astonishing results. Her summary is as follows:—

(1) A diet containing vitamin A will produce sound teeth in a puppy; (2) conversely, a diet deficient in vitamin A produces the following effects: (a) delayed loss of first set; (b) delayed eruption of second set; (c) overcrowding and irregularity from want of jaw development; (d) hypoplasia; (e) low calcium content; (3) this is an effect produced from within, and not the result of oral secretions.

McCarrison in his experimental work on deficiency diseases,² shows that deficiency in the accessory food factors leads to endocrine insufficiency.

Hence the work of Pickerill, Broderick and Mrs. Mellanby is all intimately linked up. They have all tackled the problem of dental caries from a different standpoint, but their united efforts have led us a long way on the road to preventive dentistry.

The accessory food factors are a most fascinating study. The part that the elusive vitamin plays in diet, is comparable to the effect of the spark on petrol in a motor car. I had long felt that the association of the effect of the accessory food factors with preventive dentistry was only a question of time, and I had already been led to connect the *trench mouth* cases, of which we had so many during the war, with a deficiency in vitamin C—the anti-scorbutic vitamin. I regarded the presence of Vincent's bacillus as a secondary matter. You will remember that vitamin C is destroyed by heat or drying, and is absent in animal and vegetable oils and tinned meats, and practically so in all cereals and pulses. Hence the rations were often deficient in vitamin C.

We had a *dysentery* ward or two during the war, and we found most of the cases suffering from an acute or subacute form of periodontitis—one might call it a form of pyorrhœa. McCarrison says that dysentery is practically a deficiency disease, arguing from the fact that on the one hand a defective diet is necessary as a favouring factor; and on the other that a vitamin diet is necessary to produce a cure. Of course it is a fact that many individuals are "carriers"—i.e., are infected without developing dysentery till the deficiency in vitamins has prepared the way.

I therefore look upon pyorrhœa as a disease of deficiency.

What is the practical outcome of all these disjointed remarks? What is the plan of campaign in other words?

We must start with the mothers, because the teeth begin to be laid down in the second month, and to be calcified in the fifth month of pregnancy.

I am called upon to lecture occasionally at women's institutes and clinics. Our first care is to see that the mother has a diet rich in vitamins—butter, whole milk, eggs, meat, and especially meat fats—liver, kidney. *Of the fishes*, herring and salmon are good (most white fish are useless in this respect). *Of the vegetables*—cabbage, spinach, lettuce, tomatoes, bananas, nuts—most vegetables in fact are good, and better raw than cooked.

Crude cod-liver oil is very rich in vitamins. It has been given for years

¹ *Lancet*, 1918, ii, pp. 767-770.

² "Studies in Deficiency Disease," 1921.

by farmers in my neighbourhood to pregnant animals, to enable them to produce healthy young.

The infant should be breast fed for nearly a year, and from a properly fed mother the child will derive the only ideal food. No substitute can quite take its place. In addition, as dental surgeons, we know that such children escape certain jaw deformities, due to teats employed in artificial feeding.

The growing children should get whole milk, butter (not margarine), eggs, fish, oils, meat, especially fats, and liver. No tinned meats should be eaten by them. They should take wholemeal bread—but excess of the carbohydrates, such as sugar, biscuits, &c., should be avoided.

Pickerill condemns tea as a beverage, and also as a meal, on account of the carbohydrates generally accompanying it. He recommends that fruit should be eaten after every meal, and especially before going to bed, and that the teeth should be brushed with an acid potassium tartrate mouth-wash.

None of the observers have referred to sleep and its beneficial effects, on children especially. I am convinced that a sufficiency of sleep should rank with good food and hygiene in importance, and that the want of it is one of the factors in producing much of the ill health, and incidentally the bad teeth, seen in our slums.

The housing problem, with which are bound up overcrowding, want of ventilation and hygiene generally, is unfortunately being tackled very slowly, especially in crowded cities, where it matters most. The changing of the site of our dwellings would be a "counsel of perfection"—an impossible suggestion; but the provision of garden cities, plenty of space in the houses and around them, seem to be meeting the needs of the case.

I may be reminded that much of what I have been saying also savours of "counsels of perfection," but after all, it is for us to point the way.

I began with a text, and on reflection, I find I have inflicted on you the tedium of a sermon, both in matter and in length. But the text gave us something to think about. And my final reflection is, if our labour in preventive medicine and preventive dentistry meet with success, will our occupation be gone?

Section of Odontology.

President—Mr. W. R. ACKLAND, M.R.C.S., L.D.S.Eng., M.D.S.Brist.

DISCUSSION ON "DENTAL SEPSIS AS AN ÆTIOLOGICAL FACTOR IN DISEASE OF OTHER ORGANS."

Sir WILLIAM WILLCOX, K.C.I.E., C.B., C.M.G., M.D., F.R.C.P.

THE consideration of the rôle of dental sepsis in relation to disease of other organs is one of the most important problems in medicine. It is certainly true that infection of the teeth and gums, by reason of the streptococcal infection arising from them, is one of the greatest sources of disease of adult life.

The exact part played by dental sepsis in the causation of disease of other organs demands in each individual case the most careful scientific investigation, and the decision as to the best course to be adopted in the interests of the patient calls for the exercise of great judgment.

The wholesale extraction of teeth without adequate supporting evidence is to be strongly condemned, since it is generally injurious to the patient, and does harm by bringing into disrepute, not only with the dental and the medical professions but also with the public, an important factor in the causation of disease. On the other hand the retention of harmful foci of sepsis in the mouth in order to preserve teeth which are a source of infection is bound to lead to impairment of health and disease of other organs. In every case of dental sepsis one must consider not only the local conditions connected with the teeth and the effects they have produced on other organs but also the patient himself. There is no problem in medicine for which careful scientific investigation and well balanced judgment are more required. It should not be difficult to come to a decided opinion in every case of dental sepsis as to what is the best course of procedure. It is most important that a true perspective of the situation should be obtained, and in each case neither too much nor too little done in the way of dental treatment and extraction.

There is no department of medicine in which co-operation between the dental surgeon and the medical practitioner is more essential.

Bacteriological Considerations.

The researches of Professor Miller, of Berlin, published in 1900, on the bacteria found in dental infections, formed the foundation of our knowledge of to-day. He found in twelve cases of pyorrhœa no less than twenty different varieties of bacteria, amongst them streptococci, staphylococci, bacilli of various kinds, and leptothrix.

In this discussion we are mainly concerned with the organisms occurring in dental infections, the absorption of which or of their toxic products gives

rise to general disease. There is no doubt that it is the streptococcal infections which are mainly responsible.

The streptococci found in dental infections are usually classified into three groups, from their behaviour when grown on media containing blood, viz.:—

(1) *The Hæmolytic Group*.—These decolorize blood-agar culture medium, and lake-red blood corpuscles; they cause severe toxæmia, and are found in the anæmia resulting from dental sepsis.

(2) *The Viridans Group*.—These produce a greenish coloration when grown on blood agar owing to the formation of methæmoglobin. *Streptococcus salivarius* is the most important member of the group as regards dental infections, and it is constantly found in the mouth. *Streptococcus faecalis*, another member, is occasionally found in dental infections. Both of these organisms are toxic, and may produce general toxæmic symptoms, and even malignant endocarditis. They are usually found in association with arthritis, fibrositis and rheumatic affections. They produce less severe tissue reactions than *Streptococcus hæmolyticus*.

(3) The indifferent group of streptococci are not toxic to guinea-pigs, and their association with rheumatic conditions is doubtful. Gram-negative cocci are found associated with dental infections, but they are not usually toxic, and some of these types have been described as staphylococci in earlier writings. Staphylococci are not usually found in dental infections, but often in the post-nasal space.

In the infections of the teeth and gums the same streptococcus is not necessarily found in different cases, nor can the local disease be constantly transmitted from an infected patient by inoculation of the healthy gums of another person, so that Koch's postulates of a specific infection are not satisfied. The infecting organisms are variable, and the infection may be mixed, so that the problem of dental sepsis is a complicated one.

The streptococci commonly associated with dental infections belong to the *viridans* group. These usually produce only mild local tissue reactions and a slight leucocytosis. They commonly cause general effects, such as disease of other organs.

Staphylococcal infections contrast with those of the *Streptococcus viridans* group in usually causing severe local tissue reactions such as boils, carbuncles, and a high leucocytosis. They much less commonly cause disease in other organs, but may occasionally do so, as for example in osteomyelitis and malignant endocarditis.

Any of the organisms of the three streptococcal groups may be found in the mouth of persons in *normal health*.

Dental sepsis forms a good illustration of the manner in which an organism normally present in the body may cause disease when it gains access to the tissues. Similar examples may be cited. Thus the pneumococcus found in the saliva in health may cause pneumonia. The *Staphylococcus aureus* constantly found on the skin may cause boils or carbuncles. The *Bacillus coli communis* may cause cystitis or enteritis. It is probable that local tissue damage and possibly an increased virulence of the streptococci found in the mouth determine the occurrence of dental sepsis.

General Factors influencing the Effects of Dental Sepsis.

These are:—

(1) *The Virulence of the Organism*.—Just as in other pathological infections, diphtheria for example, a small lesion may produce very severe effects if the

organism is virulent, while an extensive lesion with an organism of low virulence may give rise to little constitutional disturbance. It is possible that streptococci in passing from one person to another may gain increased virulence, and also the entrance of streptococci into damaged tissues may lead to an increase in virulence of the organism.

(2) *The Amount of Toxic Absorption.*—A most important factor. Everyone who is familiar with hospital practice has been impressed by the appalling dental sepsis observable to the naked eye in patients with no constitutional symptoms arising from it. The reason must be that there is free discharge of toxic products. On the other hand, an invisible deep-seated lesion with healthy gums may be associated with the most severe constitutional effects.

The "time factor" is important. If there is only slow absorption, the toxic effects will be slighter than with rapid absorption. The whole question is one of dosage with toxic products.

In a person in apparent health in whom slight dental sepsis is present the toxic substances produced by the streptococci must be neutralized by the body fluids, and the organisms must be ingested and destroyed by the leucocytes. It is the unneutralized toxic substance, and possibly also the organisms themselves, which cause by absorption the effects of dental sepsis in the sick person.

(3) *The Resistance of the Patient.*—A most important factor. The power of neutralization of toxic products depends on the resistance of the patient and his power of forming protective substances. As Mr. Stanley Colyer has put it, the two important factors are "the seed and the soil."

Some patients, from constant absorption of toxic substance, become extremely sensitized, and a condition of anaphylaxis results. The recent work of Sir Almroth Wright on septicæmia is most interesting in this respect, since he has shown that in certain cases of septicæmia the patient is incapable of developing protective substances, and the leucocytes lose their property of ingesting and destroying the virulent organisms.

Julius A. Toren, of Chicago, showed (1921)¹ that in certain cases of dental infection and gingivitis a leucopænia occurred, and he regarded this as an anaphylactic phenomenon and a signal of danger. He concluded that in this condition extraction of many teeth was dangerous, and advised removal of not more than one at a time. The observation has a very important practical bearing.

In severe cases of dental sepsis a vicious circle becomes established. Thus the absorption of toxic products by its constitutional disturbance lowers the resistance of the patient, and so leads to spread of the local infection and the disease of other organs resulting from this infection. The importance of general hygienic conditions which will improve the resistance of the patient will be appreciated.

Family predisposition or diathesis plays an important part in dental sepsis. Thus a resulting streptococcal toxæmia will produce in different persons various respective manifestations, such as arthritis, gastric ulcer, anæmia, gout, &c. It is difficult to understand exactly the determining conditions of the particular disease occurring, but in many patients family predisposition appears to be an important ætiological factor.

(4) *Local injury* may also be an important determining factor if dental sepsis is present, as, for example, the occurrence of progressive arthritis of the

¹ *Dental Cosmos*, 1921, lxiii, p. 492.

hip-joint after an injury, or the development of infective streptococcal endocarditis on the damaged valves of a rheumatic heart.

(5) *Symbiosis* is an important factor. Thus, the presence of another disease such as scurvy or dysentery may lead to the rapid progression of dental sepsis. On the other hand, the presence of dental sepsis is well known to have an adverse effect in patients suffering from other diseases. Thus a septic mouth adversely affects the prognosis in any acute disease such as typhoid fever, pneumonia, &c. In cases of pulmonary tuberculosis it has been shown by Dr. R. C. Wingfield that dental sepsis if untreated may frequently turn the balance against the patient.

(6) *Potential Health*.—The consideration of this is very important. If dental sepsis is present and no apparent harm is resulting, it does not follow that the condition may be disregarded. In such cases danger is always present. Thus in the condition of "apparent health" there is an equilibrium between the protective power of the body and the toxic absorption. Should, however, this balance be upset by the occurrence of some other disease, or some depressing influence such as chill, injury, &c., then the presence of "dental sepsis" is almost sure to assert itself and cause disease. In other words a condition of potential health, not apparent health, is to be aimed at.

Evidence for the Conclusion that the Teeth and Gums are the Source of Infection.

Dental sepsis causes a streptococcal toxæmia, and it is this which produces its manifold disease effects.

A streptococcal toxæmia of exactly similar nature may result from many causes other than dental sepsis; thus the tonsils, nasopharynx, intestine and urogenital tract are common foci of infection in streptococcal toxæmia.

Because there is an association of certain diseases such as arthritis, pernicious anæmia, &c., with dental sepsis it must not be assumed that in any such case dental sepsis is the cause. For example, there is at present under my care at St. Mary's Hospital a patient (aged 58) suffering from pernicious anæmia. His teeth and gums are, on clinical and radiographic examination, found to be perfectly healthy. There is in this case a chronic antral and nasopharyngeal infection of streptococcal origin which is the undoubted cause of the illness. Careful examination must always be made to determine if any foci of infection other than dental are present, and every case should be approached with an open mind and no preformed opinion.

The Clinical Macroscopical Signs of Unhealthy Conditions of the Teeth and Gums.

These when present will indicate dental sepsis. It must be borne in mind that little or no evidence may be apparent from an external examination of the teeth. Sir Frank Colyer rightly says in his book on "Chronic General Periodontitis":—

"It is not safe to judge the extent of the disease from clinical appearances only, and it is necessary to call in the aid of skiagraphy in order to ascertain how far bone destruction has proceeded."

The gums may appear healthy, and yet there may be an extensive disease of the alveolar process around the roots of the teeth, and considerable bone destruction, the involved area being heavily infected with pathogenic streptococci. The grave clinical effects resulting from infected bone are well known,

and from observation of a large number of cases which have been carefully investigated by bacteriological and radiological methods, I am strongly of opinion that the general clinical effects produced by dental infections are accounted for by the extent and nature of the disease of the bone in the neighbourhood of the teeth, rather than by disease in the gums or the teeth themselves, though these latter are the primary causes of the bone disease.

It cannot be insisted upon too strongly that in every case of illness in which the teeth may be primarily responsible—even if external appearances of the teeth and gums are healthy—a radiographic examination should be made to ensure that the alveolar process around the teeth is also healthy.

Radiographic Evidence.

This will disclose the extent of the disease of the alveolar process. The periodontal membrane of a tooth may be swollen, and the alveolus may show superficial erosion, such as occurs with advanced age. Rarefaction of the alveolus round certain teeth may be present; but the most important evidence of all consists in the presence of necrosis of bone in a more or less spherical area around the apex of the tooth. These apical lesions are commonly called "apical dental abscesses." The term is a bad one, because there is no pus present, and, most important of all, they give rise to no pain. The term "apical granulomata" has been used; this also is bad, because the microscopical appearances are not those of a granuloma. When a tooth is extracted with an apical dental lesion, a mass of solid gelatinous substance is found adherent to the apex. This contains pathogenic streptococci and necrotic substance with very few leucocytes. The term "peri-apical bone necrosis" would accurately describe the condition actually present.

In my opinion, the "peri-apical bone necroses," the so-called "apical dental abscesses," are the most serious lesions found in connexion with dental sepsis, and it is these which give rise to the gravest general disease resulting from them. It is probable that from these lesions there is a constant flow into the blood-stream of either virulent streptococci or their toxins, and the anatomical position of the lesions prevents an adequate supply of leucocytes and bactericidal body fluids to the part. If lesions of this kind are present the maintenance of health is obviously impossible; some serious general disease is bound to result if it has not already appeared. If a lesion of this kind be present no compromise is permissible; the affected tooth must be extracted.

An interesting point for discussion is the possibility of the dental radiograph showing the existence of active disease, and whether the presence of a clearly defined margin round the affected area of bone indicates protective resistance.

Secondary Infections.

Where dental sepsis is present the streptococcal infective process may spread by *direct transference* of organisms to the tonsils, nasopharynx or gastro-intestinal tract.

Hurst has shown in cases of anæmia associated with achlorhydria that in 100 per cent. of cases streptococci are to be found in the duodenum, and in a large proportion of these cases the streptococcal infection had a dental origin.

In cases of dental sepsis the colon almost always becomes infected with streptococci sooner or later. Then, again, the organisms may be conveyed by the blood-stream to other parts—as, for example, in malignant endocarditis of dental origin. These secondary infections are usually progressive, and may carry forward the same toxic process, causing disease in other organs.

It can be readily understood why a disease of dental origin, for example arthritis, may progress when the primary forms of infection or even all the teeth have been removed. In many of such cases the intestine is acting as the secondary toxic focus. It does not, therefore, follow that if dental sepsis is removed, the disease which has resulted from it will necessarily clear up. This consideration emphasizes the great importance of the early recognition and removal of dental sepsis.

Dental Sepsis may be Secondary to some other Disease or Toxæmia.

An excellent illustration of this is scurvy, in which marked dental sepsis is one of the earliest symptoms, and restoration of the patient to a dietary rich in antiscorbutic vitamins will lead to rapid improvement and perhaps disappearance of the dental sepsis.

The effect of mercury in causing dental sepsis is well known, and the withdrawal of the drug may be followed by disappearance of the dental sepsis.

Bacterial diseases such as bacillary dysentery may be followed by dental sepsis, which will abate and perhaps clear up when the primary condition is cured.

In cases in which dental sepsis is secondary the removal of the primary causal factor should be aimed at before an estimate can be formed of the amount of dental sepsis present which will require treatment.

The General Diseases caused by Dental Sepsis.

Dental sepsis is a serious condition and may give rise to all the manifestations of disease which are produced by a streptococcal infection. Thus *acute streptococcal septicæmia* and *septicopyæmia* have not infrequently arisen from dental sepsis. The risk of this dangerous complication must always be borne in mind in connexion with the extraction of infected teeth in patients whose resistance to streptococcal infection is low. As already mentioned, a leucopænia is a danger signal.

Toxæmia is commonly associated with dental sepsis. It may be chronic, subacute, or acute. Chronic toxæmia is present in a great many of these cases, and in all cases where some general disease has resulted from the primary dental infection. There is a feeling of malaise and general ill health, a tendency to exhaustion on slight exertion, a pale and muddy complexion is common, and often some general pains in the hands and feet indicate an irritation of the peripheral nerves; frequently some symptoms of fibrositis or threatening arthritis occur. Insomnia, headache, and dyspeptic symptoms are common. Sometimes severe wasting occurs, associated with general weakness, the patient having an appearance similar to that in chronic phthisis. *Subacute toxæmia* may give rise to intermittent pyrexia, with general constitutional symptoms of ill health, extending over months and years. In some cases pyrexia and general symptoms of ill health are followed by profound *acute toxæmia*, and a condition of stupor, delirium, and coma results.

Local Infective Conditions resulting from Dental Infections.

Such conditions as stomatitis, tonsillitis, nasopharyngeal infections, infections of the maxillary antra, cervical adenitis, and Ludwig's angina have been observed.

Blood Conditions.

A secondary anæmia, mild or severe in type, to which Dr. William Hunter has given the name "septic anæmia," is a common result of dental sepsis. The more severe types of anæmia are associated with streptococci of the

hæmolytic type. In many cases of pernicious anæmia a severe dental infection with hæmolytic streptococci is present, and is one of the most important causal factors.

Leucocytosis is commonly present, and usually the differential count approximates to the normal. It may show variations in different cases, and in the same case at different stages of the disease.

Some acute cases show a marked relative lymphocytosis. This may be a true lymphocytosis, or simply an apparent lymphocytosis, due to a polymorphonuclear leucopænia. In cases of lymphocytosis it is the large mononuclear cells which are increased.

An increase of the eosinophilia is uncommon. Two cases, however, have come to my notice.

Cardiovascular Complications.

Streptococcal infections of dental origin may cause phlebitis and venous thrombosis, and also arterio-sclerosis, which is not necessarily associated with an increase in the blood-pressure. The changes in the arterial wall may give rise to narrowing of the lumen with symptoms of intermittent claudication, or even arterio-thrombosis.

Cardiac Conditions.—Tachycardia of toxic origin is often to be observed, and pericarditis, myocarditis, and myocardial degeneration may occur. Endocarditis when present may be of the simple type, such as occurs in acute rheumatism, but not infrequently dental sepsis gives rise to ulcerative endocarditis. Five such cases have been under my care during the past three years; in these the origin of the infection appeared to be definitely the teeth.

Blood-Pressure.

In some cases, especially those associated with gouty symptoms, dental sepsis may cause a raised blood-pressure. In other cases of the asthenic type the blood-pressure may be lowered below the normal. Attacks of angina pectoris may occur in those cases with lowest blood-pressure just as in those with raised blood-pressure.

Respiratory Complications.

The streptococcal infection may give rise to laryngitis, tracheitis, and bronchitis. Pleurisy and empyema were described by Hunter in 1900 as possible complications. Septic broncho-pneumonia is a serious and not uncommon complication, and it may be followed by bronchiectasis, or lung abscess.

At the present time a patient is under my care in St. Mary's Hospital suffering from abscess of the lung the result of very severe dental sepsis. Attention has been called to the adverse influence of dental infections in cases of pulmonary tuberculosis.

Asthma is not infrequently due to a streptococcal bronchial infection and dental sepsis may be an important ætiological factor in some such cases.

Gastro-intestinal Complications.

Dental sepsis is one of the commonest causes of gastric and intestinal dyspepsia. Hunter, in 1899, laid stress on the frequency of toxic or infective gastritis, and he then expressed the opinion that dental sepsis was the probable cause of some cases of that rare condition "phlegmonous gastritis."

Gastric and duodenal ulcer probably result from septic infection, and the trend of opinion at the present time is that dental sepsis is a most important cause. In my experience it is rare to find a case of gastric or duodenal ulcer in which an adequate explanation of the cause cannot be found in the condition of the teeth and gums, and I should say that dental sepsis was much the most important ætiological factor.

In some cases of dental sepsis hyperchlorhydria results, whilst in others, especially those associated with pernicious anæmia, achlorhydria is present.

Appendicitis, in many instances, is due to streptococcal infection, and several cases have recently been under my care in which the primary infection appeared undoubtedly to arise from the teeth.

Enteritis, with symptoms exactly like those of paratyphoid fever, including enlargement of the spleen and similar pyrexia, may undoubtedly result from streptococcal dental infections. The differential diagnosis can only be made by complete bacteriological investigations, and by the recognition of the dental disease for which often radiological examination is necessary. Three cases of this kind have been under my care.

Colitis, simple and ulcerative, is frequently due to a streptococcal infection, and in many of these cases the primary focus is undoubtedly connected with the teeth. Peri-apical dental necroses will often be found in intractable cases of colitis. Dr. N. Mutch has called attention to the frequency (84 per cent.) of pathogenic streptococci in the colon, in an analysis of 200 cases of arthritis. In 52 per cent. of these cases he concluded that the primary focus was a dental infection.

Renal Complications.

Nephritis has been described by many writers as sometimes resulting from dental sepsis. Pyelitis may also occur.

Liver Complications.

Toxic conditions of the liver are well known to result from streptococcal infections and frequently these are accompanied by jaundice; also evidences of hepatic disturbance are observed in cases of dental infection. There can be no doubt that hepatic efficiency is often impaired as the result of the toxæmia from dental sepsis. Catarrhal jaundice (so-called) may be caused by dental sepsis setting up a hepatitis from streptococcal infection which leads to obstruction of the smaller bile ducts.

Skin Complications.

Rashes of an erythematous, urticarial, papular, and eczematous type have been observed. Purpuric rashes may occur, especially where the streptococci are of the hæmolytic type.

Lupus erythematosus has been recorded in association with dental sepsis, the removal of which has resulted in marked improvement and in some cases in cure of the skin condition.

Dr. Graham Little and Sir Frank Colyer have recently had under treatment a case of alopecia areata in which the condition appeared to be definitely due to dental sepsis.

Eye Complications.

Conjunctivitis, iritis, irido-cyclitis, episcleritis, keratitis punctata, retro-bulbar neuritis have all been described as resulting from dental infections. Undoubtedly vascular lesions, such as thrombosis of the central artery or vein of the retina, may be so caused. Of special interest is retinitis.

Dr. Batty Shaw¹ has recently called attention to the great importance of the toxic factor in this condition and undoubtedly dental sepsis is not infrequently the cause.

Nervous Diseases.

The toxæmia from dental infections may give rise to cerebral conditions such as abnormal mental states, melancholia, &c., and it is possible that inflammatory conditions, such as meningitis, may be so caused.

Diseases of the spinal cord, such as combined sclerosis with its associated anæmia, disseminated sclerosis, &c., are often due to toxic causes, and dental sepsis must be included amongst these.

Peripheral neuritis may occur from the streptococcal toxæmia of dental origin, and the sensory symptoms, tingling in and numbness of the hands and feet, are of common occurrence. Local neuritis, such as sciatica, brachial neuritis, &c., is a common result of dental sepsis, but these are better included under the fibrositis group, because the cause is rather a perineuritis than a primary involvement of the nerve fibres.

Rheumatic Conditions.

In a paper published in the *British Medical Journal* (June 4, 1921, p. 805), I called attention to the great importance of infection of the teeth and gums in the causation of rheumatic conditions such as fibrositis and infective arthritis, commonly called "chronic rheumatism." "Chronic rheumatism" appears from the Ministry of Health Reports to be the commonest disabling disease at the present day.

Fibrositis.—The streptococcal infections so arising may give rise to the various forms of fibrositis—namely, panniculitis, inflammatory conditions of fasciæ, and aponeuroses—such as occur in lumbago and myalgic conditions; inflammations of tendons and ligaments, such as stiff neck, and tender heels due to involvement of the plantar ligaments. Dupuytren's contractions of the palmar fascia; inflammations of tendon sheaths, arterio-synovitis; bursitis; Heberden's nodes; finger pads; fibrous nodules in subcutaneous tissue; local perineuritis and neuritis, as in sciatica and brachial neuritis. Fibrositis in some of its forms is the commonest occurrence in cases of dental and gum infections.

Non-specific infective arthritis, which includes the forms known as rheumatoid arthritis, arthritis deformans, osteo-arthritis, and chronic villous arthritis, is generally due to a streptococcal infection. Dr. Beddard, in a discussion on the "Morbid Anatomy and Histology of Rheumatoid Arthritis," held at the Medical Society in October, 1918,² expressed the opinion that 90 per cent. of these cases were due to infection arising from the teeth, and my personal experience approximates to this view.

It is well known that in cases of non-specific infective arthritis numerous bacteriological examinations have shown that no living organisms are to be found in the joints. It is probable that the streptococcal toxins give rise to the inflammatory conditions. The recent work of Dr. W. E. Gye and Dr. E. H. Kettle has shown that marked proliferative changes can occur in organisms such as the spleen, through the action of colloidal silica, without the presence of living micro-organisms.

Acute rheumatism is not commonly caused by dental infections, but several

¹ *Proceedings*, 1923, xvi (Sects. Med. and Ophth.), Discussion on "The Significance of Vascular and other Changes in the Retina in Arterio-sclerosis and Renal Disease," pp. 1-5.

² *Trans. Med. Soc., Lond.*, 1919, xlii, p. 20.

cases have been described. The organism found by Poynton and Paine in numerous cases of acute rheumatism closely resembles the *Streptococcus salivarius* and *Streptococcus faecalis* found in dental infections.

Gout.

In some cases of gout dental sepsis is important, and should be removed, as far as possible, in every gouty patient. Dr. Llewellyn has expressed the opinion that gout is the result of a toxic idiopathy to certain toxic protein substances. Undoubtedly, the streptococcal toxins of dental origin are in not a few cases the important causative factor.

Diabetes.

Streptococcal and other toxins may cause a toxic glycosuria, probably by impairment of the endocrine function of the pancreas. Dental sepsis is, undoubtedly, a factor in the causation of glycosuria in some cases, and it should always be removed in cases of diabetes. A rise in the carbohydrate tolerance has often been observed by me after the removal of dental sepsis in early cases of diabetes.

In cases of glycosuria a toxic factor should always be sought for, and frequently dental sepsis is found to be the cause of the condition. In such cases removal of the dental sepsis is essential and should form one of the first elements of the treatment. In a number of cases under my care this procedure has been followed by a disappearance of the glycosuria. It seems probable that diabetes mellitus is caused in some cases by the irrecoverable damage to the islets of Langerhans by the toxic absorption from dental sepsis.

Hyperthyroidism.

An excessive activity of the thyroid gland may undoubtedly result from a streptococcal toxæmia. In one case under my care a tonsillar streptococcal toxæmia gave rise to enlargement of the thyroid gland, rapid pulse and other symptoms of hyperthyroidism: Enucleation of the septic tonsils was followed by a complete disappearance of the symptoms. In an analysis of 100 consecutive cases of dental sepsis giving rise to disease in other organs, hyperthyroidism was observed by myself in four cases.

Dental sepsis is usually regarded as one of the ætiological factors in exophthalmic goitre.

Scurvy.

This disease, due to vitamin deficiency, usually shows marked dental sepsis on its earliest appearance. During the war over 20,000 cases of scurvy occurred amongst Indian troops in Mesopotamia, many of which I examined personally, and in almost all very marked infection of the teeth and gums was present. The result of careful observation showed that if pre-existing dental sepsis was present in a marked degree, then such patients were very predisposed to develop scurvy.

As an illustration of the respective frequency of occurrence of disease in other organs in cases in which dental sepsis was marked and appeared quite definitely to be the important ætiological factor the following table is given. It represents the last 100 consecutive cases I have seen (rheumatic cases are not included).

TABLE I.

Colitis	26	Carbuncles	1
Toxæmia (mild general)	15	Appendicitis	1
Toxæmia, acute	1	Venous thrombosis and phlebitis	1
Septicæmia, acute	1	Enteritis	1
Glycosuria	12	Salpingitis	1
Gastritis	8	Asthma	1
Gout... ..	6	Malignant endocarditis	1
Duodenal ulcer	5	Retrobulbar neuritis	1
Hyperthyroidism	4	Nephritis	1
Severe anæmia	3	Melaena	1
Skin rashes	3	Arterio-sclerosis hæmorrhage	1
Cardiac irregularity (extra-systoles)	2	Combined sclerosis	1
Boils	1	Broncho-pneumonia	1

As illustrating the frequency with which dental sepsis appears as the cause of the rheumatic affections "fibrositis" and "arthritis" the following table represents the last 100 consecutive cases I have seen.

TABLE II.—100 CONSECUTIVE CASES OF ARTHRITIS AND FIBROSITIS.

Source of Infection.		Types of Rheumatic Cases.	
Dental sepsis	72	Arthritis	56
Intestine	13	Fibrositis	40
Tonsils	10	(Including 3 of brachial neuritis, 3 of sciatica, 1 of tender heel)	
Urethra (gonococcal)	5	Acute and subacute rheumatism	4

Treatment.

The treatment of infections of the teeth and gums, and the disease arising from them, does not come within the scope of this discussion. The most important general principles in the treatment of such infections have, however, been indicated, namely:—

(1) Removal of the focus of infection, either by extraction of teeth, or suitable treatment.

(2) It must be remembered that intestinal infections very frequently result from dental infection, and that these may require treatment by such methods as Plombières colon irrigations, or an autogenous vaccine prepared from the streptococci found in the teeth and intestine.

Prophylaxis.

The early recognition of dental sepsis and its appropriate treatment would be one of the most important factors in greatly improving the health of the nation.

Sir KENNETH GOADBY

observed that, as Sir William Willcox had said, dental sepsis became really a discussion of diseases which arose from streptococcal infection. He (Sir Kenneth) showed long ago that the staphylococcus was not a common inhabitant of the human mouth, and that in only about 15 per cent. of cases was *Staphylococcus aureus* found present. But with regard to streptococci, the late Dr. Washbourn and himself, as long ago as 1896,¹ read a paper before the Odontological Society showing that the streptococcus was present in all normal mouths. He would now sound a note of warning in regard to the question of streptococcal infections. It was well known, by analogy, that the colon-bacillus lived in the colon and that it was responsible for many infections; and it was known that, in certain instances, the *Streptococcus salivarius* was found in lesions. But he could not subscribe to the idea that the streptococcus

¹ *Trans. Odonto. Soc.*, 1896, xxviii, pp. 251, 252.

found in every mouth, and on every epithelial cell shed from the oral mucous membrane, could be accused of causing the large number of diseases likely to arise from infection of the jaws. Holman showed some time ago that if one took a streptococcus of the hæmolytic type and grew it in conjunction with a streptococcus of non-hæmolytic type, in a very little time the hæmolytic streptococcus had grown down, and was no longer to be found. The *Streptococcus viridans*, mentioned by Sir William Willcox, was a secondary type of streptococcus of the hæmolytic type. But he could not agree that the ordinary streptococcus of the mouth was the *viridans*, or that the ordinary common streptococcus obtained from average normal mouths belonged to the *viridans* group. It was one of the third group, which he (Sir Kenneth Goadby) had shown was not one of the common infecting streptococci. He had recently published some ideas on this question of disease of the mouth, and in his chapter on diagnosis of diseases of the mouth, he had scheduled the streptococci he had met with in many of the cases.

With regard to the arthritis, he had been able to show that arthritis was directly caused by mouth lesions, and that one could produce definite lesions of a rheumatic nature by the injection into animals of streptococci obtained from the jaws, not the streptococcus of the mouth, but one which had some other existence, and which he called a strepto-bacillus at the time, because it took on a bacillary form. It was one of the streptococcal group, of the *viridans* type, and should be properly so called.

With regard to some of the other diseases to which Sir William Willcox referred, he was glad to hear him sound a note of warning as to the extraction of the teeth being considered the only method, or *the* method of cure. Of course, the local infection must be removed, as in any other infective condition, but when once the organisms had gained entrance, when the balance phase was depressed in favour of the bacteria and against the resistance of the patient to the organism, the removal of the cause was not, in itself, sufficient to take away the disease itself, unless the particular body had great powers of resistance.

He was interested to see the large incidence of colitis in relation to mouth infection, and that the *Streptococcus salivarius*, i.e., the one found on ordinary epithelial cells, was found. It was uncommon to find hæmolytic streptococci in the fæces examined. It had also been shown by several observers, especially by Dible, that hæmolytic streptococci disappeared quickly if injected into a loop of normal intestine; in seven or eight hours the hæmolytic streptococcus had been grown down by other organisms. He had made many searches for hæmolytic streptococci in the fæces of persons who had hæmolytic streptococci in their mouths, but in only 1 per cent. was it possible to demonstrate those organisms in their fæces. In regard to cultures made from any normal stomach much depended on what time of the day the culture was made. If made the first thing in the morning, streptococci were always found in the stomach. But if a culture were made later, or some hours after a meal, when a good quantity of gastric juice had been secreted, hæmolytic streptococci were rarely found; certainly they were uncommon.

His experience of mouth infections was, that one found, in the very virulent types, the streptococcus for which Holman suggested the name "*sub-acidus*."

There was another point which he was very glad had been brought out in this paper, and it was one which ought to be recognized as a matter of general importance. In the ordinary routine of practice one was met by the question

as to whether or not any given case might be said to be due to an infection. It was sometimes very difficult to answer that question, and Sir William Willcox had quoted Toren, of Chicago. It was in 1920 that he (Sir Kenneth)¹ read a paper, in Boston, on the subject, and Toren had made use of some observations he then scheduled of the association of leucopænia and leucocytosis, which he showed were common in oral infections. He (Sir Kenneth) found that in the majority of dental infections, or of infections from the jaw, leucocytosis was uncommon unless there was a direct infection of the blood-stream with the bodies of the organisms, but that the prodromal symptom was polymorphonuclear leucopænia; and he took it that Sir William Willcox was referring to septicæmia and toxæmia as if they were synonymous terms. Toxæmia, however, by itself, was a different condition, from the pathological point of view, from septicæmia. With *Streptococcus salivarius* no toxin could be demonstrated, though it was possible it might have an endotoxin which was pathogenic. He was inclined to the view that streptococci were the organisms which gained entrance and caused damage by the presence of streptococci in the blood-stream, and that had been demonstrated by a large number of positive cases of streptococcaluria in these people; the streptococci grown from the urine often showed the same cultural reactions and were of the same type that were isolated from the patient's jaws. Therefore he was very pleased to find that the suggestion he had made of the importance of blood examination was now adopted as a guide towards the right method of treatment in cases of mouth infection. Finally, with regard to the cardiac lesions referred to: in his experience, the average cardiac lesion met with in chronic infections was myocarditis, rather of the auricular than of the ventricular type. The signs shown by polygraph or electro-cardiogram frequently indicated damage in the upper, rather than in the lower portion of the heart.

The whole question of jaw infections, as the opener of the discussion said, was one of streptococcal infection; but it was necessary to be certain of the type of streptococcal infection with which one was dealing, and it was impossible to regard the ordinary streptococcus existing in everybody's mouth as the cause of arthritis, or of any other of the long category of diseases mentioned. He demurred to the morphological classification of all streptococci as one species, and much more to the statement that the whole of the group caused disease. The streptococcus could be obtained from anyone's mouth, a vaccine prepared from it and the patient treated, but this did not constitute a demonstration that the streptococcus was a cause of the disease.

Dr. WILLIAM HUNTER

said that Sir William Wilcox had given an admirable summary of the rôle of sepsis in medical diseases. He (Dr. Hunter) looked back twenty-four years to a meeting of the Odontological Society² at which he gave the first account of his own work on the subject. That discussion was adjourned to the following evening, and, as an American writer said, it had gone on ever since. Since that date, the profession was divided into two camps: those who realized that sepsis in the mouth was of great importance in medical diseases, and those who considered that such importance was exaggerated. The issue really was streptococcal infection, prevalent, potent and effective, as the greatest disease-factor in medicine. It was immaterial to him as to what varieties of strep-

¹ *Journ. Nat. Dent. Assoc.*, May-June, 1922.

² *Trans. Odonto. Soc.*, 1899, xxxi, p. 92.

tococci were responsible for the infection, because twenty-five years of careful observation had shown him that it was the streptococcus found in connexion with the teeth that was chiefly responsible for the various septic infections met with in medicine. The question of the varieties and reactions of streptococci was most difficult, and that was shown by the long-continued and careful work of Sir Kenneth Goadby and others, for there were twenty-six varieties of streptococci known to bacteriologists. The important point was, that this infection was present in the mouth; and the matter of moment was, that it was not the infection which was there in a free state, but in foci, in diseased bone, and intensified in virulence by reason of this contact with diseased bone. Twenty-five years ago he showed that the infection extended to the tonsils and pharynx, from the pharynx to the stomach, causing septic gastritis; to the intestine, causing enteritis; to the colon, causing septic colitis; the effects extended to the blood, causing septic anæmia, which in turn produced effects on the nervous system. He would like to deal briefly with two subjects: (1) effects on the blood; (2) effects on the kidney and nerves. The effects of oral sepsis were first brought forward by his work in connexion with anæmia. It was not easy to know what had been the effect of oral sepsis upon such conditions as functional nerve disease. The anæmia caused by dental sepsis was not pernicious anæmia; it was a condition which he had named "septic anæmia." The evidence of the part played by sepsis was best shown in connexion with the blood. It was in such cases as when a patient came in fainting and suffering from profound anæmia, and was found to have only 23 per cent. of the normal quota of red cells. It was often found in association with only a mild degree of dental sepsis. He found that the sepsis extended to the antrum, the patient having muco-pus there. Three or four teeth were removed, and the antrum washed out, and the result was that in six weeks the patient went out with 73 per cent. of red corpuscles, as against 23 per cent., and in nine weeks 91 per cent., feeling quite well. He saw that patient recently—the treatment referred to having been carried out ten years ago—and there had been no return of the anæmia. This effect on the blood could be judged by actual percentages. In all cases of septic anæmia there was a distinct leucopænia, so that instead of the white cells being 100 per cent. of their normal number they were only 33, 55 or 60 per cent. On removal of the septic focus, the percentage of white cells rose from 33 per cent. to 81 per cent., the chief constituent change being in the polymorphonuclear leucocytes, which rose from 44 per cent. to 70 per cent. Perhaps the best illustration of the action of sepsis was afforded by eye diseases, patients having lost their sight as a result of iritis, choroiditis, or other conditions, as shown by Mr. Lang, in work dating from 1913, and by many other observers.

His third group was that which he called the nephritis group. The rôle of chronic septic absorption in causing kidney changes was described by him in 1903. It had during the last few years engaged the attention of many clinicians—especially in America, with convincing results. Thus in the Mayo Clinic, a study of sixty-four cases showed that forty-three had evidence of tonsil infection, and forty-five had one or more abscessed teeth. Forty-five had possible foci removed, with definite improvement in thirty-five cases, in striking contrast to the results formerly obtained under ordinary measures of treatment.

A further point concerned the relationship of dental sepsis to mental disease—actual insanity. When a man was a chronic dement and had to be confined in an asylum, there was definite evidence of trouble. Work in this

connexion had been carried out in the New York State Hospital for the Insane, by Dr. Cotton, and it was one of the most important developments of the whole subject of dental sepsis in medicine. Twenty-one years ago he (Dr. Hunter) described cases of toxic neuritis, of paralysis of shoulder muscles and arms, directly connected with oral sepsis, the ailments disappearing upon the removal of the oral sepsis. Before that time there were no data of any such connexion. In the ensuing twenty-five years he had seen oral sepsis responsible for all degrees of neurasthenia. In 37,000 cases of insanity the presence of chronic infection and the resulting toxæmia was a constant and most important factor. For all groups of the insane, the conditions including dementia præcox, manic-depressive insanity, general paralysis, senile dementia, alcoholic insanity and other psychoses, the spontaneous recovery in ten years up to 1918 was 37 per cent.; since 1918 it had reached 77 per cent. For ten years up to 1918 the average mental discharges monthly in comparison with admissions was 43 per cent. In 1918 and since, the monthly discharges rose to 80 per cent. The recognition of this factor shortened the period of residence in hospital. Previously the average stay had been ten months; during the last three years it had been three months. He would give a concrete case of such a condition. The kind of case was that of a person, aged 55, who had been two years in hospital as a chronic dement, without having had any treatment directed to her mouth. She was excited, talkative, depressed, agitated, and self-accusative. Up to 1918 she remained in this state, but in September of that year she had eleven teeth extracted. She improved rapidly, and four weeks later she was discharged cured, and had since remained quite well. Case after case was recorded among the insane on these lines, and the effect of the removal of the septic foci was so great as to leave no doubt in the mind as to the causal connexion. Twenty-one years ago, when dealing with the subject, he emphasized what he still insisted upon, that the disease was so important that it was not merely the province of the dental surgeon; that the observation and treatment of conditions in the mouth rested largely in the hands of the physician, even though the problem as to how to treat those conditions devolved upon the dental surgeon. In that connexion he exhibited two charts. The first showed the immediate effect of removal of sepsis in a case of septic anæmia. The blood count rose from 23 per cent. to 71 per cent., then to 80 per cent. in six weeks. The next chart showed the effect of sepsis on pernicious anæmia of a severe type, the life of the patient usually lasting one to three years. The effect of the removal of the septic disease was an improvement from 20 per cent. to 30 per cent., and this amelioration increasing to 60 per cent., 80 per cent., and finally, 100 per cent. in the course of five or six weeks. His last chart showed the effect of the removal of oral sepsis on the subsequent progress of that grave disease pernicious anæmia, namely, that instead of ending life in the first year or the second year, it now took the following course when the sepsis had been dealt with: Great improvement, 60 per cent. to 80 per cent., which extended into the next year, with the possible continuance of the patient's life to the fifth year. In two cases, as shown by the chart, the disease went on for thirteen years and twenty years respectively. With those two classes of facts in view, and taking into consideration the effect on the blood and the nervous system of treating the sepsis, the causal connexion seemed to be quite clear.

(The Meeting was adjourned to Monday, February 26.)

Section of Odontology.

President—Mr. W. R. ACKLAND, M.R.C.S., L.D.S.Eng., M.D.S.Brist.

ADJOURNED DISCUSSION ON "DENTAL SEPSIS AS AN ÆTIOLOGICAL FACTOR IN DISEASE OF OTHER ORGANS."

Mr. WILLIAM HERN

said he quite agreed with the opening sentence of Sir William Willcox's address, in which he said that dental sepsis was one of the most important problems in medicine. He (Mr. Hern) looked upon the mouth as the ante-chamber of the whole intestinal tract; as an incubating chamber for organisms it was one of the most important cavities in the body. But Sir William was largely preaching to the converted when he brought this subject forward. The list of diseases the author gave was so formidable, that it seemed to confirm the statement, "We dig our graves with our teeth."

But the term "dental sepsis," as used by Sir William Willcox, included two quite different pathological conditions, with regard to both origin and course. He referred first to the apical infections, due to the death of the pulps and the decomposition of the organic contents in the root canals. The other kind comprised the cases of growth of micro-organisms about the gum margin, spreading into the surrounding alveolus, leading to pyorrhœa. Assuming that Sir William Willcox was correct in his contention, that the streptococcal group were the chief enemies in both these conditions, it was important to assess the comparative potency for harm played by these as ætiological factors. The worst part he considered was played by the gum infections, for the area of toxic absorption was more extensive, the dose of toxic products was larger, and the position of the focus of infection was more favourable for dissemination of toxic products to the nasopharynx and to the gastro-intestinal tract by swallowing. But he agreed as to the harm done by apical infections, especially when the infection was deeply seated in bone, where leucocytes were few, and phagocytes, because of their scarcity, not active.

He agreed as to the importance of radiographs of the teeth in all cases, to determine the extent of the mischief and the correct treatment for the case. If the plate taken showed a definite area of bone infection around the apex of a dead root, extraction was the proper course. But he wished to emphasize the importance of a true reading and deduction from the X-ray plate. There was a tendency to regard all gum and alveolar absorptions as pathological, but he did not think that was justified. There was a normal or histological, as well as a pathological absorption. The alveolus was subject to variation; it was built up with the temporary teeth, and later with the permanent teeth, and was swept away with them when they went. It was therefore a variable quantity. But the absorption was pathological when associated with pyorrhœa and other disease conditions of the gums.

[February 26, 1923.]

There was also a tendency to regard all dark shadows in the negative as pathological. One often saw a dark shadow near the apex of the lower premolars, but this was the mental foramen; there were dark shadows also seen about the roots of upper molars, showing the dipping downwards of the floor of the antrum. Further, the appearance of exostosis could be produced by a curved root, i.e., when the plane of the photograph was oblique to the root curve. Some slight rarefactions were also sometimes seen on the skiagram, which he did not think meant disease. For instance in cases of straight-rooted teeth which, after an attack of periostitis due to death of the nerve, had been drilled into, and the pulp chambers and canals had been thoroughly cleared and cleansed. He had watched such for years, and they continued quite healthy, there had never been a gumboil or periodontitis or anything of that kind.

With regard to the prevention of apical troubles in dead teeth, more care was needed in thoroughly clearing pulp chambers. Short-cuts in treatment, such as mummifying pastes, &c., where contents of pulp chambers were left, should be abandoned. He was aware of the difficulty of being quite sure that curved and inaccessible canals were quite clear of decomposing tissue; in some cases such complete clearance was impossible. In a paper he read many years ago before the Odontological Society the author laid stress on the importance of opening the canals in the long axis and of enlarging and reaming them well out. This ensured thorough clearance and it gave a better chance of following along a curved canal. With straight roots, however, it should not be impossible to get the pulp canals quite clean.

The prophylaxis of gingivitis was a more simple matter; it consisted of the inculcation of shampooing and frictionizing the gums with a fairly stiff tooth-brush, with or without an antiseptic; the latter being of only secondary importance.

He agreed with Sir William Willcox that the earlier recognition and treatment of dental sepsis would be one of the most important factors in improving the health of the nation. As already said, the list of diseases Sir William Willcox gave as due to dental sepsis was somewhat appalling; he had stated that 72 per cent. of the cases of arthritis were of dental origin, and he (Mr. Hern) would like to know what percentage of these were apical troubles of teeth, and in what proportion gingival or pyorrhœal troubles were the cause.

Dr. C. KEMPSTER

said that radiograms not only demonstrated the existence of a positive condition, but also proved a negative condition. When the teeth and surrounding bony structures were not responsible for the condition of the patient, that fact would be revealed by the skiagram. Hitherto it had been the common practice to extract all the teeth, in the hope that some septic area would thereby be opened up, and when neither the teeth nor alveolar processes were at fault and this extraction had been done, the patient had suffered irreparable loss. Where, however, teeth or alveoli were at fault, the skiagram showed it, and defined the limits of disease, thus giving the dental surgeon precise directions as to his treatment. He (Dr. Kempster) was frequently having patients sent to him by dentists for radiographic examination because they were supposed to be suffering from pyorrhœa alveolaris. He had found an issue of pus between the teeth and gums, and often there was seen to be only a peri-apical septic area, involving not more than two or three teeth; in this

way pyorrhœa was simulated. Wholesale extraction in such a case would be obviously unfair to the patient. For many years he was radiologist to a well-known orthopædic hospital, in which there were many cases of arthritis, some complicated with intestinal troubles. In all these cases he radiographed the teeth-bearing area, and in most of the patients he found pyorrhœa absent. The trouble was frequently due to peri-apical sepsis, the treatment of which led to very satisfactory results, for in 70 per cent. of the cases the arthritis was either cured or much relieved. Sir William Willcox had suggested that all cases of peri-apical infection should be spoken of as cases of peri-apical bone necrosis—a term he (Dr. Kempster) did not altogether like; in many cases with peri-apical lesions there was no evidence of bone necrosis. Neither did he agree with Sir William that septic absorption took place from all peri-apical lesions. For instance, in the case of peri-apical cyst, the dense line of demarcation often observed suggested that a barrier had been erected against septic absorption, and that idea was borne out in practice.

Mr. COLIN KEAY

pointed out that a very small area of infection sometimes kept up systemic mischief for a long time. Recently a patient came to him who had been treated for gastric ulcer for a year on and off. A radiogram showed a small abscess at the apex of a lateral tooth. He removed the tooth and curetted out the abscess thoroughly. In a month, without anything further being done, the patient was well. There had been no outward sign of the abscess.

Mr. J. G. TURNER

said those who, like himself, had preached for twenty years and more the importance of dental sepsis, would find, in Sir William Willcox's paper, a welcome confirmation of their views.

But there must necessarily be points of divergence, and some of these he would touch upon. He thought peri-apical infection was not more than half the danger. There was an equal danger from the more superficial infection, which meant the presence of putrid food and germs among the teeth, before any pocketing took place, and actual ulceration on the toothward side of the gum flap. The danger was as great as that from peri-apical disease. The following was the history of a case in point :—

A patient, aged 38, had been suffering, since her 21st year, from violent migraine, on the average once a fortnight; in her worst attacks her temperature rose to 103° F. She came to him at the end of very careful medical and surgical attention in hospital for six weeks. When she came from hospital she was seen by a dentist, who told her she had pyorrhœa, and must have her teeth out, but before agreeing the patient was advised to see the speaker. He found she had foul pyorrhœa, but no peri-apical lesions. He took out four teeth, molars, for drainage, excised two or three small gum flaps, scaled and cleaned her teeth, and taught her how to clean the teeth herself. She had been free from migraine ever since, and she said life was now worth living.

In practice he thought the more dangerous condition was the superficial one, which was liable to be overlooked. The danger began in early life, in childhood, and that was the second point on which he joined issue with Sir William Willcox, for he saw no reference in the opening address to dental sepsis in childhood. There were certain conditions in childhood, such as myopia at school age, which he believed to be mainly due to dental sepsis. Dental sepsis in childhood might sow the seed of diseases of later life.

He would also controvert Sir William Willcox on the streptococcal question. Sir William, like others, found that the streptococcus was practically the only offender. When Drew and he were first able to stain germs in the gums and granulosomatous masses from the apices and roots, it was a surprise to them that they did not find the streptococcus in as many cases as they had expected, though it could be found superficially. From the deeper parts a diphtheroid bacillus was often obtained; and in at least two cases they had specimens of the jaw and of the stomach from people who had died of septic poisoning. Both in the bone and—in large quantities—in the stomach walls they found a diphtheroid bacillus. There was a difficulty in being sure when making the bacteriological examination that one was not merely examining a contamination. It was very easy to infect at the moment of extraction; and after, if the tooth were put into a test-tube or laid on a flat surface, fluid was readily drawn from the crown to the root and to the granuloma. The more careful he was the less likely was he to recover streptococcus in his bacteriological examination.

Why should not this tissue be called granuloma? Many of the cases were actual granulomata, showing multiplication of the fixed tissue cells which could form repair tissue, though the larger number of them showed an infiltration with round cells, almost to the exclusion of other histological structures.

And there was a small point which, he thought, might sometimes cause confusion. Sir William said dental sepsis might be secondary to some other diseases, such as scurvy. But surely the sepsis was the external and original part in the mouth, and it could not be secondary to any disease, except for the fact that the patient could not clean his mouth during the continuance of that disease. If there were no sepsis in the mouth, he would get no dental symptoms even though he had scurvy. There seemed to be confusion between what was sepsis and what was the result of sepsis.

Sir William also said it should not be difficult to give a decided opinion in every case of dental sepsis. But in practice it was difficult, especially in such instances as eye cases, in which one was sure there was a certain small amount of infection going on, and yet one could not be sure that by sacrificing the teeth good would result. The only thing to do was to take the plunge, and fortunately the procedure of taking out the teeth, even those that seemed sound, seemed justified. In one case, in which the patient had only one eye left, he took out the last four (apparently harmless) teeth, and the man had remained well ever since.

He (Mr. Turner) agreed that the results of dental sepsis covered about half the diseases of civilization. He had a comprehensive formula for use when lecturing to students: there was no disease—ranging from illness due to defective drainage or following a broken leg, to acute mania—which might not be influenced by dental sepsis, and the diseases directly due to it included about 50 per cent. of the ills of civilization—beyond enumeration in the few minutes left for discussion. Of course, there were classic, almost sacrosanct, diseases, and one heard complaints (such as that from Lord Dawson) that port and parentage were being ousted from pride of place in the production of gout. But, if a gouty man's teeth were cleaned and, where necessary, extracted, he could be permitted to go on drinking his port or his beer.

What was wanted was a new view of medicine and surgery, which should take into account infection first. If treatment directed against "sepsis" failed, a covering diagnosis might be necessary.

Colonel McKECHNIE

related the history of the following case, which was under his observation while he was in India:—

The patient was an American dentist, who had been in India two years. Shortly after arriving there he had an attack of fever, which was diagnosed as malaria. After a year, he had another attack, which lasted a long time. Blood examinations had been negative. When the third attack came on he (Col. McKechnie) was called in. It looked like a case of typhoid; the temperature was 103° F. on the second day of the disease. When there was a suspicion of typhoid, he always took blood from the arm at once, for if the diagnosis was delayed it was the worse for the patient. In this case the blood slide was negative. But the pathologist incidentally remarked that there was a contamination with staphylococcus. He (the speaker) had taken many samples of blood without contamination, therefore he was suspicious, and he asked the pathologist to make subcultures. Meantime, he searched the patient for any septic focus. He was healthy and active, but pale. In twelve hours the report was received from the pathologist that another germ had been found, streptococcus, and his idea now was that it was not a contamination, but that the patient had septicæmia. The patient's temperature was still 103° F. He seemed to have a perfect set of teeth, but one lower molar had been stopped; he said it had never troubled him. He (Colonel McKechnie) examined the tooth and percussed it. The patient admitted that his fever did not come on until after the tooth had been stopped. As no other cause for the condition could be found, he advised the extraction of that tooth, and it was removed, and a medium was inoculated with it, and it was sent to a laboratory. The two organisms previously obtained from the blood were found—staphylococcus and streptococcus. After that the patient rapidly recovered health.

Probably in some such cases the patients died without the conditions from which they were suffering being diagnosed.

Sir William Willcox was very vague on the question of sepsis, and he said that in one case a local efflorescence of a carbuncle resulted from sepsis. He (Colonel McKechnie) believed that carbuncle often resulted from conveyance of staphylococci by the blood-stream. The amount of toxin which would be absorbed from the apical infection at one or two roots would be very small; but if it was a question of absorption of the living germ, the case became more serious. When Sir William talked of toxæmia, he often apparently meant septicæmia. He (Colonel McKechnie) strongly advocated taking blood from the arm for examination at the beginning, for if this were delayed till the third or fourth day, the germs became more difficult to find, perhaps because the antibodies had largely killed them off.

A further deduction from what he (Colonel McKechnie) had said was, that every tooth in which a pulp cavity had been opened should be watched for some months. When a pulp cavity had been stopped, it must be recognized that the task did not end there. If the patient suffered from any ill-health, the tooth should be skiagraphed, and if there were any indications of disease, the tooth should be extracted. He imagined that the chief danger was apical infection, causing septicæmia.

While in India his chief practice was in eye cases, and in every case which came before him he examined the teeth. In India, among the class he saw, practically every person over 30 years of age had pyorrhœa. Not that by any means all beyond that age were in bad health, but they would be ill if they were suffering from bad apical infections.

Mr. CHARLES LEONARD GIMBLETT

spoke of the bearing of dental sepsis on the work of the ophthalmic surgeon. All admitted the importance of dental sepsis in regard to infection of the nose and the nasal accessory sinuses; and it was not a very far cry from the antrum, ethmoid cells and frontal sinus to the orbit. The sphenoidal sinus was in close relation, by way of its roof, with the optic chiasma, and an empyema of the sphenoidal sinus had, in many cases, resulted in retrobulbar neuritis and blindness. But there were three conditions, not so well recognized, which also seemed to be intimately associated with dental sepsis. The first was iridocyclitis—infection of the uveal tract—often very chronic, and difficult to be sure about. There were but few symptoms: it gradually brought about blockage of the spaces of Fontana, and increased the tension of the eye—glaucoma—causing blindness. Dental sepsis was especially important when this condition was unocular; one eye having glaucoma, the other eye being normal. If these cases of unocular glaucoma were examined, it was found that there were floating vitreous opacities, and in some inflammation of the uveal tract as evidenced by the condition known as keratitis punctata. In one case of the kind he saw in hospital, the old lady had very foul teeth, and was recommended to have them extracted. After this had been done the state of her eye became much worse. It had to be recognized—and the patient should be warned about it—that when the teeth were taken out the eye condition might temporarily become worse. Another condition was macular degeneration, which might occur for no apparent reason, in one eye, perhaps in both, but it was difficult to prove actual connexion between this condition and dental sepsis. The third condition in the same category was blepharitis and, to a less extent, chronic conjunctivitis. The first of these conditions was often very obstinate indeed, and in some instances the fact that the patient had very septic teeth was quite overlooked. He mentioned the case of a patient with very bad teeth indeed, in whom the blepharitis was practically well within six weeks of the cleaning up of his mouth. He promised attention to the interesting point made by Mr. J. G. Turner about myopia at school age. He saw children of school age regularly several times a month, and some of them were unaccountably myopic, though their parents, sisters and brothers were not. He would now have them examined for dental sepsis. With regard to the treatment of eye conditions by removal of teeth, one was, here, in the same unfortunate position as those who had to treat osteo-arthritis: was one justified in recommending that the patient should have his teeth extracted? In some cases that measure produced no effect, but this should not deter extraction, for although it was not certain that the teeth, at a late stage, were the primary focus, there might be secondary foci, resulting from the dental sepsis—foci which were now lighting up the eye condition. An acute case could be cured comparatively easily, but in many chronic cases it was difficult under treatment to make any impression.

Dr. GRAHAM LITTLE

spoke of the importance of dental sepsis in relation to diseases of the skin; he said it was astonishing how frequently dental sepsis was found in cases of skin disease of obscure causation. The history of one case under his observation was quite sensational in this respect.

The patient was a lady who had very extensive sclerodermia, an atrophic disease of the skin which caused large areas of the body to be immobile, and for which very little could be done. She was under the treatment of Sir Thomas Barlow, who sent her to Berne, to undergo an implantation of thyroid at the hands of Professor Kocher, who had performed implantation in her case in two successive years. The amelioration seemed sufficient to warrant a third visit, but Kocher refused a further operation, and she returned with the condition practically unaltered. She was so crippled that she could not feed herself, and had to be carried up and down stairs. Within a fortnight of having her teeth extracted she was able to walk; she soon became able to feed herself, and she was now able to type her own novels.

There were also some other diseases which, after persisting for a series of years, reacted to removal of the teeth. One of the chief of these was that formidable disease lupus erythematosus, which in rare instances had proved fatal owing to practically universal septicæmia. Little could be done for the condition. Cases of this disease were now improving remarkably after removal of the teeth. A woman, aged 34, had an extensive, advancing lupus erythematosus, so rapid that he (Dr. Graham Little) had feared a fatal result. A skiagram of her teeth showed them to be extensively diseased. They were removed, a few at a time, and within three weeks of the first removals the skin condition had notably cleared up.

Alopecia came into the same category in this connexion; he had a case in which after the removal of diseased teeth there was a definite return of hair, beginning within a fortnight of extraction of teeth.

There was also the large group of toxic eruptions, passing under the name of erythema multiforme; these were often most satisfactorily treated by examination and removal of teeth. It was unfortunate that all dentists were not themselves alive to the importance of dental sepsis; the speaker had had several cases in which dentists refused to remove teeth which ultimately proved to be diseased.

Alopecia of the beard he regarded as always due to dental sepsis. Lichen planus was one of the most recent diseases to be associated with dental sepsis; he was getting some cases which seemed to respond very specifically to treatment of the teeth.

In regard to the number of diseases in which this connexion was being established, it was difficult to know where to stop, but it was very evident that dermatology was one of the latest specialities to recognize the fact.

Mr. W. R. ACKLAND (President)

said that the discussion did not start off with a definition of the exact meaning of oral sepsis or of its causes and sources. He assumed that any pathological conditions of the mucous membrane of the mouth might, and did, contribute towards it. There were three conditions pertaining more particularly to the teeth: (1) Caries, in which there might not be exposure, but in which sepsis might result as a product; (2) pyorrhœa alveolaris; and (3) apical sepsis. He wished to add to these, cases of ill-fitting dentures, for they led to a great deal of illness. He had had three cases in which the patients suffered from general malaise, with wasting, for a long time before the cause was discovered. In one case the plate had ceased to fit, and the sides cut into the gum; in two others the trouble was due to the plate going too far back and the soft palate moving against it.

These cases were so baffling, that he had laid down certain rules for himself in examining them: (1) Dentists should not be over-impressed with the importance of their own calling and expect that a case was entirely one for dentistry. A case might be found to be due to a certain cause, and when another case seemed like it one was apt to think the same cause was at work, but often this was not so. (2) Occasionally cases were seen which were attributable to two causes, and both had to be dealt with before the case cleared up. (3) He believed that occasionally pyorrhœa was a result, not a cause. There might be oral sepsis and general disease in a patient, both having a common origin, for instance—pyorrhœa and iritis, the cause in both being rheumatism.

Many years ago a boy who had been seeing an ophthalmologist came to him (the President) and said he was very disappointed as he would not be able to get into the Army, because his sight was defective. Incidentally, he had to put his teeth right; the sight improved, he got into the Army, but was, unfortunately, killed in the Great War. Apparently it was myopia due to oral sepsis. Recently he saw a case which was sent to him by Dr. Burdon-Cooper, of Bath. The patient had a central colour scotoma, and the skiagram he took showed apical trouble on the left canine. This tooth was removed, and the patient lost his visual defect.

Recently again he had had a case in which there had been obstinate lacrymation for two years, with inability to face the light or to read. The patient's mouth was in a terrible state. He found five apical abscesses, and removed all those teeth, then the case cleared up almost at once. The punctum was not blocked, but the lacrymation and eye weakness were very definite.

The last case he would mention he had reported before, a lady with eyes of two colours, one grey, the other brown. After some years' acquaintance he had the occasion to extract from her mouth a pivoted left upper lateral, and within three months of that the brown eye had become grey. He inquired into her history, and learnt that at 14 years of age, when at school, she had had intense trouble in that eye; immediately after, this tooth was crowned. Then came the change of colour of the eye, and for fourteen years it had persisted, but it got right within three months of taking out the lateral.

Mr. A. T. PITTS

said that from his student days he had adopted the views held by Mr. Turner, and had always felt strongly that dental sepsis was of very great importance. But his difficulties in any given case had increased as time went on, and he was often perplexed to know what to do. In spite of all that had been said on the subject, the problem appeared to him more obscure than ever. It was easy to make the problem appear easy by assuming the various factors to be simple, when they were not so. In nearly every case one had to depend on clinical proof, for pathological proof was lacking. Thus the extraction of teeth, because of some general condition assumed to be related to dental sepsis, was an experiment which sometimes "came off," but in many cases did not. When a striking cure resulted it was supposed to be a case of cause and effect which afforded an argument for similar treatment of like conditions. But if the result was negative the significance was usually glossed over. He had been much interested by a recent paper on achlorhydria in relation to other diseases in the *Lancet*, by Dr. Hurst,¹ which suggested that there might be other factors

¹ *Lancet*, 1923, i, p. 111.

which, conjoined with dental sepsis, made the latter a more serious event. Dr. Hurst pointed out that achlorhydria was of frequent occurrence and in some cases was of congenital origin. The stomach had an antiseptic as well as a digestive function, the former being due to the presence of free hydrochloric acid. Dr. Hurst thought that in most cases the swallowing of infected products from pyorrhœa did not do much harm. But if achlorhydria was present the bacteria passed undiminished into the intestine and set up excessive protein decomposition which in turn might set up various infective conditions elsewhere. Dr. Hurst suggested that this might account for some of the cases of osteoarthritis which did not improve after the septic teeth had been removed. In these cases a secondary infection in the intestine had been allowed to become established, and achlorhydria was often present. He (Mr. Pitts) said that Dr. Hurst went on to explain the association of achlorhydria and dental sepsis in pernicious anæmia in a similar way, and had stated that it was of the utmost importance to remove every particle of sepsis from the mouth in pernicious anæmia, which usually meant the extraction of all teeth. In striking contrast to this conclusion, Panton, Maitland-Jones and Riddoch, in a review of pernicious anæmia which appeared in a recent issue of the *Lancet*,¹ said that they had failed to find any evidence of a causal relationship between dental sepsis and pernicious anæmia. They strongly condemned the wholesale extraction of teeth in this disease and remarked that they had known many patients made miserable by the loss of all their teeth, some of whom had lived long enough to contemplate, but not to use their dentures. What was the dentist to do in face of this divergence of opinion? He (Mr. Pitts) said that Sir William Willcox seemed to assume that all dental lesions had a similar infective value, but we knew little about the significance of apical infections. The X-ray might show an area of rarefaction around a root but it might be impossible to say what was the pathology of the condition. It might be a granuloma, or an epithelial root tumour, or an abscess. It might even represent a quiescent infection which had been walled off by the resistance of the tissues. Clinically it might not be possible to differentiate between these possibilities, yet, according to the author, all were equally to be condemned and treated by extraction. This was a counsel of despair and suggested that dental surgery was of little avail. Sir William had said that no crowned tooth should be left in the mouth because apical rarefaction was so often seen in such teeth. If this meant anything it meant that every tooth which had had the root canal opened should be extracted, for it was this part of the operation and not the fixing of the crown which might be followed by an apical infection. The rôle of dental sepsis in general disease was undoubted, but much more pathological work was needed to place the matter in its right perspective. It was humiliating to reflect that our only means of testing the relationship in any given case was to make an experiment and deprive the patient of his teeth, which could not be replaced if the experiment failed.

MR. CRIBB

reminded members that in the case of the teeth one was dealing with vessels of very minute calibre, and any material resulting from bacterial activity circulating in the blood would be felt there. It had been too lightly assumed that the dental focus was the primary focus. In a number of cases in which the teeth "went wrong" he was able to find that there had been such a

¹ *Lancet*, 1923, i, p. 274.

condition as colitis at some previous time ; in such a way what was spoken of as a vicious circle was established. From the presence of these products in the pulp various effects ensued ; he referred to decomposition, pressure, absorption of bone. Necrosis of bone he did not think was so frequent as was often thought. He did not agree with Sir William Willcox that in the case of these teeth extraction was the only course ; one could cut off the apex and get a sound, workable tooth, the cavity could be curetted, and so the trouble be ameliorated or cured. Pictures had been shown in one of the dental journals in which regeneration of bone had occurred, and he did not think that would have happened had there been necrosis. He agreed with Mr. Turner that "granuloma" was a better name than necrosis. There seemed to be a process analogous to that in dental cyst ; a capsule was formed which afforded protection. In extraction granulomata did not always come away with the teeth. What should be done when they did not ? Curettage should be done, as he did not think extraction would clear up the trouble. The statement was freely made that such and such conditions were dental in origin, but in the absence of proof such statements should be accepted only with reservations. Scurvy had been mentioned. According to the latest ideas, scurvy was due to the patient being deprived of certain antiscorbutic vitamins, and from experiments made on animals fed on dried hay, &c., it was clear there was a fairly immediate dental result ; the tissues of the pulp seemed to lose all character, and there was a mass of fibrous tissue, with loss of any line of demarcation. Other experiments were made on guinea-pigs in the way of depriving them of antiscorbutic vitamins, and if those elements were absent from the food there was the same fibrous tissue formation. Later the teeth became loose, particularly the upper molars. Glycosuria had also been mentioned. When this occurred and sugar circulated in the blood, the teeth would be affected by it first of all. People with diabetes often found their teeth loosening. Much tooth trouble had its origin in the intestinal condition. Patients might seem to have healthy gums and teeth, but afterwards one found marginal gingivitis, and they were found to have had colitis or other intestinal trouble. Some years ago Mr. Turner had cited the case of people who took too much whisky and who complained of their teeth feeling loose and feeling uneasy. At present the large consumption of imported and prepared foods, as to which not enough care was taken, was not a good thing. A surgeon who had neuritis of severe degree in the arm had been sent to him. He had marginal trouble in the gums, and there were pockets of pus. But he only took one tooth out as it was somewhat loose ; the others seemed to be all right. After removal of the appendix the small amount of pus disappeared, and he got well. Another patient contracted infective jaundice in Gallipoli and was sent to Malta. His teeth became very loose, with pus welling up around them. The R.A.M.C. surgeon suggested extraction of all teeth, but eventually it was decided to keep the teeth if possible. The jaundice was cured, and nine months later the teeth and gums were sound and healthy.

Sir FRANK COLYER

remarked that the attention of the profession at the present time was so fixed upon the question of apical infection that there was a little danger of their losing sight of the far-reaching effects of septic absorption from the gum margin. He pointed out that people might have septic mouths and yet show no signs of ill-health ; nevertheless, in his opinion, the septic mouth must be

regarded as a source of potential danger. He drew attention to certain facts about the physiology of the gingival trough and also to some investigations which had been carried out on teeth removed from cases in which there had been gingivitis; it had been found that the apices of these teeth were infected, as well as the pulp cavities, and as far as the pulps were concerned, they all showed a certain degree of fibrosis, which definitely pointed to the fact that the infection of the pulp was of sufficient intensity to lead to certain tissue changes. These facts would suggest that in all cases of septic conditions about the gum margin active infection of the tissues around the teeth was taking place, although for a time the defences in and about the teeth were sufficient to prevent the infection passing into the general blood-stream; sooner or later, however, these tissue defences broke down, with resultant damage in other parts of the body. There was an important paper by H. Waller in the *Lancet* (November 4, 1916), in which it was shown that in the case of mothers nursing their infants—the children failing to gain in weight and even vomiting the feeds—there was evidence that the trouble was attributable to the septic condition of the mouth of the mother, for with the removal of the septic teeth the children rapidly gained in weight. If the secretion of the mammary gland could be so profoundly affected by dental sepsis, he (Sir Frank Colyer) saw no reason why the internal glands should not also be influenced. He quite agreed with Mr. Turner that one must not lose sight of the importance of dental sepsis in children and expressed the view that a large amount of the infection from which they suffered in later years originated in the early days of childhood. Radiographs of septic teeth frequently showed very widespread infection of the bone. If good results were to be obtained from the removal of the dental sepsis, the dental sepsis should be removed when the tissues had a chance of repairing—in other words, before the regressive stage of life. He personally took out teeth much more freely for patients aged under 50 than for patients over 70.

Sir WILLIAM WILLCOX (in reply)

said he hoped he would not be regarded as an invader of the dentists' territory, but rather as one who was accompanying them in exploring a region about which neither he nor they understood everything, one over which the dental profession held a mandate. In this matter there was need of cordial co-operation between physician and dental surgeon, almost more than in any other subject. The final decision as to extraction should rest with the dental surgeon.

He explained that he was regarding the subject entirely from the clinical standpoint. He could give chapter and verse for all the conclusions he had drawn. As Mr. Pitts said, judgment entirely from the clinical side might lead to some errors, perhaps to some mistakes.

With regard to Sir Kenneth Goadby's remarks, referring to the question of leucopænia in a paper read before the National Dental Association in 1920, Sir Kenneth had given accounts of cases of dental sepsis in which leucopænia was present. This was at a date previous to the paper of Julius A. Toren of Chicago mentioned in his (Sir William Willcox's) opening address.

Dr. William Hunter's contribution to the debate was most interesting, and the profession was greatly indebted to him for his pioneer work.

Mr. William Hern asked what proportion of the cases of arthritis in the series given in the paper were due to apical infection, and the reply was that fifteen of the seventy-two were apical infections. Some of the cases, at the

time he saw them, had had all their teeth extracted, and he did not know what was the condition of the teeth before they were taken out. He would say apical infection was under fifty per cent. He agreed with Mr. Kempster that peri-apical lesions revealed by the skiagram did not necessarily mean they were the source of sepsis; such an appearance might be due to a cyst which was not infective, or there might be an area of disease which was effectively shut off. That, however, was the exception. He agreed also with Mr. Colin Keay that a small lesion might cause a good deal of harm. He (the speaker) had a skiagram of an apical abscess under a lower incisor tooth which Mr. Herbert Smale had extracted; this was the undoubted cause of malignant endocarditis, to which the patient succumbed. He confirmed Mr. Turner's remarks as to the importance of dental sepsis in childhood: he himself had had so little experience of dental sepsis in childhood that he did not care to entrench upon that subject. He agreed that streptococci were not the only organisms which were the cause of the trouble, and, as Mr. Turner said, diphtheroid organisms might play a part. The very interesting case mentioned by Colonel McKechnie confirmed that view.

With regard to the question of scurvy, of people who suffered from scurvy those who had dental sepsis in addition would have the worst degree of scurvy, and dental sepsis was a predisposing factor if the diet was inadequate. It was an instance of symbiosis, one disease leading to the development of another.

Mr. Gimblett and others emphasized the importance of dental sepsis in various eye conditions; it was a sphere in which the significance of dental sepsis was only now beginning to be realized.

He agreed with Colonel McKechnie that often what were called toxæmias were really septicæmias, as there was a constant incursion of living organisms into the blood-stream. In what were known as toxæmias the organisms could be destroyed; in septicæmias they remained in the blood much longer.

Dr. Graham Little's contribution from the point of view of skin diseases was very interesting. Perhaps when the causation of many skin diseases had been narrowed down, the profession would not be so much bewildered with the extraordinary long names in which that speciality abounded.

He also had been glad to hear the President's contribution, especially in regard to ill-fitting dentures, and he agreed with him that all possible sources of infection should be looked for, not only in the teeth, but in other organs.

Mr. Pitts gave a very critical review of the subject, and he could assure him that he (Sir William) did not think he knew all about dental sepsis, and he agreed that much more pathological work on the subject was needed. His (the speaker's) condemnation of crowned teeth was based on clinical experience. A fortnight ago a patient who had been feeling ill with vertigo came to him. He was about 50 years of age, and apparently had a sound mouth and beautiful teeth. As he could not find any cause for the ill-health, he suggested an X-ray examination of the teeth. Every one of the crowned teeth had an apical abscess under it; and if Mr. Pitts would look at the series of photographs exhibited, he was sure he would pardon him (Sir William Willcox) for having taken a strong view on the matter. He frequently, as a physician, found serious systemic lesions due to crowned teeth, and he was sure that at the present day there was a large amount of careless crowning of teeth done, causing much ill-health.

Sir Frank Colyer, who had done so much on the subject of dental sepsis, asked why the removal of dental sepsis caused glycosuria to disappear, as it did in many cases. The explanation appeared to be as follows: a patient who was

continually receiving streptococcal poison into his system became sensitized to the infection, and the islands of Langerhans became poisoned and paralysed, so that they did not pour out the internal secretion, and glycosuria resulted. If the teeth were removed in these early cases of glycosuria, the glycosuria did disappear. And if septic teeth were removed in bad cases of diabetes, if the glycosuria did not permanently disappear—which it did in some cases—almost always the carbohydrate tolerance was considerably raised. He believed the same applied to the causation of hyperthyroidism from dental sepsis.

Sir Frank Colyer's advice about removing infected teeth in people of various ages was very wise. Sir Frank had told him that in elderly people, with some retraction of gums and signs of gingivitis and dental sepsis, more harm than good was done by wholesale extractions; with old people one should go warily and gently. Sir Frank had dealt so forcibly with apicectomy, that he (Sir William) had nothing further to add.

Section of Odontology.

President—Mr. W. R. ACKLAND, M.R.C.S., L.D.S.Eng., M.D.S.Brist.

DISCUSSION ON INFECTIONS OF THE TEETH AND GUMS IN THEIR RELATIONSHIP TO THE NOSE, THROAT AND EAR.

Dr. P. WATSON-WILLIAMS.

THE discussion involves, on the one hand, our consideration of nose, throat and ear conditions, resulting from infections of the teeth and gums, and conversely, the influence of anatomical or pathological conditions of the ear, nose and throat, in determining the incidence and course of infections of the teeth and gums.

What constitutes clinical infection in one region does not necessarily apply to other regions. The term infection is to be understood as implying an invasion of infective organisms with abnormal tissue reaction, for as in the colon, so in the mucosa of the mouth and gums and the surface of the teeth and pharynx, sepsis is normal, and even a degree of septic symbiosis is probably normal and possibly beneficent. The degree of dental sepsis determining abnormal invasion and commencing septicaemia varies within limits, but on the other hand the nasal accessory sinuses are normally sterile.

NASAL AND AURAL NEURALGIA OF DENTAL ORIGIN.

Neuralgia dentalis may be referred to the nose, suggesting *antral*, *ethmoidal* or *frontal sinusitis*,¹ and particularly when a wisdom tooth is the source, *deep-seated earache* may be the chief complaint. Dental pain referred to the nasal sinuses usually originates in the upper teeth, through communications of the dental nerves with the second division of the fifth nerves; while earache is due to the communication of this second division of the fifth with the tympanic plexus through Meckel's ganglion, or of the third division of the fifth nerve through its communication with the otic ganglion.

PERIODONTITIS AND PERIAPICAL DENTAL INFECTION.

Without entering into the debated causes and pathology of periodontitis, I propose to use the terms—(a) *Cervical* infection to connote all the varieties of septic infection commencing about the necks of the teeth; (b) *apical* infection to connote all processes arising primarily in or around the periapical space, including granuloma, blind abscess or dental cyst formation.

¹ Thoma, "Dental Disease in Relation to Diseases of the Nose and Throat," *Boston Med. and Surg. Journ.*, 1918, clxxix, p. 17.

Cervical Periodontitis (Pyorrhœa Alveolaris).

The incidence of cervical periodontitis, or pyorrhœa alveolaris, I believe to be determined by two variable factors: (1) Lowered resistance from any cause whatever, and (2) local trauma and infection. A high degree of oral sepsis resulting from septic tonsils or infective nasal discharges may apparently cause cervical dental sepsis, yet even under such conditions the incidence of "pyorrhœa" is largely due either to the lowered vitality of the patient or to local trauma resulting from abnormal dental conditions.

Nasal obstruction resulting in buccal respiration appears to increase the liability to cervical periodontitis¹ involving especially the anterior teeth rather than the molars, and the importance of recognizing and treating the nasal defects, septic tonsils and adenoids for success in overcoming dental sepsis is emphasized by Colyer.

"Cervical sepsis" appears to affect the patient either (a) by direct toxic absorption, or (b) by breeding pyogenic organisms which find their way into the oral secretions, and, being swallowed, may cause gastro-intestinal infection, or passing into the respiratory tract, cause infective laryngitis, bronchitis, &c.

The lymphoid aggregations in the fauces, namely, the tonsils, and those in the pharyngeal mucous membrane, may become infected and septic, causing acute or chronic tonsillitis or pharyngitis. Laryngologists realize the immense importance of having septic teeth and gums treated before proceeding to operate on the larynx, but it is well not to disregard dental sepsis before any operation on the pharynx, and perhaps particularly the tonsils, since a sloughing base after tonsillectomy (involving removal of the capsule) is fraught with special dangers, e.g., purulent bronchitis, septic pneumonia, or septic pulmonary infarct. In these conditions special care should be directed to the exclusion of Vincent's fusiform spirillum organisms, as they cause dirty and extensive sloughing of wounds.

The wisdom teeth are frequently a cause of infective disease in the corresponding tonsillar region. The eruption of a third molar is often irregular and difficult and liable to cause gingival traumatism; infection (i.e., pyorrhœa) follows and involves the tonsils. The infection may lead to an acute or chronic tonsillitis and hyperplasia. More rarely peritonsillar abscess results; of this Canuyt records three examples.²

Pyorrhœa sometimes gives rise to a spreading septic necrosis of the mucoperiosteum, tracking back from a molar to the fauces, and simulating peritonsillar abscess and sometimes involving the deep cervical tissues with pus burrowing in the neck; similarly angina Ludovici may arise from dental sepsis. Unless we keep such possibilities in mind, the dental source of a tonsillitis, peritonsillitis or septic cellulitis is liable to escape notice when these serious complications arise, and constitute the dominant symptoms.

Peri-apical Dental Infection.

We must refrain from entering upon alluring problems opened up by a general survey of apical sepsis in relation to infection of the dental pulp, which is outside our discussion, but apical sepsis of the teeth in relation to the maxillary antrum at any rate does concern the rhinologist, not only in the case of adults, but also in that of young children.

¹ Colyer, "Chronic General Periodontitis"; also Colyer, "Dental Surgery and Pathology."

² Canuyt, "Les affections amygdaliennes," *Archives Internat. de Laryngol., Otol. et Rhinol.*, 1922, xxxviii, p. 935.

Apical granuloma may be secondary to *maxillary antral infection*, though I understand that the prevalent teaching is that apical granuloma or abscess is always secondary to septic pulpitis or pericementitis.

More than twenty years ago I expressed the view that "some cases of antral empyema are undoubtedly secondary to caries of the teeth, but there is little reason to doubt that often chronic antral empyema is itself a cause of caries of the teeth corresponding to the affected cavity."¹ And the cases recently published by Dr. Glassburg,² of New York, appear to afford irrefutable proof of my contention that primary apical sepsis may be caused by antral infection. In each of Glassburg's three recorded cases, acute maxillary sinusitis occurred, and the teeth were reported sound by a dental surgeon; and, moreover, odontograms taken demonstrated the absence of any indication of apical abnormality. Yet after an interval, varying from a few weeks to a few months, during which, owing to the patient's neglect, sinusitis was allowed to persist untreated, when the teeth were again radiographed, primary apical abscess had obviously developed.

What is the percentage of cases in which the teeth infect the antrum, or *vice versa*, I do not venture to surmise, for at present reliable data are wanting. Various authorities on diseases of the nose have committed themselves to figures, and these vary so widely that no reliance can be placed on the figures given.

NASAL INFECTION AND THE DENTAL BUDS.

We have to realize that nasal sinusitis is by no means confined to adults, for antral infection, as well as other forms of nasal accessory sinusitis, occurs in young children or even in infants.

It would be interesting and useful to inquire whether acute purulent rhinitis in infants is particularly prone to be followed by malformation and abnormal dentition, for in early life the dental buds are in very close relationship with the developing antrum, ethmoidal cells and lacrymal sac and duct. Schmiegelow has recorded cases of acute osteomyelitis of the upper jaw in infants, with swelling of the cheek and hard palate, falling out of dental buds, and alveolar pyorrhœa.³ Two similar cases are recorded by Vernieuve in infants of 5 weeks and 2 months old respectively, though he considered the infection was derived from the lacrymal sac in his two cases.⁴

We have referred to acute or subacute maxillary antral sinusitis as a cause of acute apical dental sepsis, yet it would seem to be quite as likely that *chronic antral infection* should involve the teeth of the upper jaw in relation to the antral floor—a chronic antral sinusitis is far more common than is generally suspected, as it may exist without localizing symptoms or any notable purulent discharge except for a short period after the acute exacerbations which are generally regarded as severe colds in the head. Such cases are truly "latent," the pyogenic organisms finding a suitable habitat in the mucosa, though subject to inhibitory resistance of an otherwise healthy host.

But what of the dental apices, separated by a thin layer of bone and periosteum from the septic submucosa—and this thin bone capsule becoming more or less decalcified, affording evidence of the pathological influence of the chronic sepsis?

¹ Watson-Williams, "Disease of the Upper Respiratory Tract," 4th ed., 1901.

² J. A. Glassburg, "Dental Infection Secondary to Acute Maxillary Sinusitis," *Journ. Amer. Med. Assoc.*, 1922, lxxviii, p. 883.

³ *Archiv f. Laryngol.* 1896, v, p. 125.

⁴ *Rec. de Laryngol.* September, 1921, cited *Journ. of Laryngol.*, May, 1922, p. 254.

We lack the data necessary for definite conclusions, and a rhinological examination in a series of cases of apical sepsis of the teeth of the maxillæ would afford useful information. Recurring nasal catarrh, particularly if mainly one-sided, post-nasal discharge, pain or tenderness over the antrum, nasal polypus, subjective sense of bad smell or taste, all point to nasal sinusitis. But in the chronic latent cases, special methods of examination may alone suffice for the detection of a latent sinusitis of the antrum.

I must warn against too much reliance on transillumination; skiagrams are far more trustworthy, but here again, a one-sided, thick-walled antrum may mislead. The crucial test consists in sucking out the contents of the antral cavity back into a sterile syringe, observing the presence of pus, and submitting the specimen to the pathologist to determine whether there is definite evidence of an active infective process, as evidenced by polynuclears with phagocytosis, &c.

No dental surgeon to-day can fail to meet with radiographic evidence of apical sepsis which otherwise must have remained unsuspected, inasmuch as the involved tooth may seem sound and the apical sepsis without a local symptom. These we may term "latent;" they are the analogue of latent sinusitis. But the analogy between latent nasal sinusitis and latent dental apical sepsis is far more important than a question of terms. The clinical importance of these chronic quiescent infective foci, whether nasal or dental, depends on the degree of toxin absorption and consequent systemic blood poisoning; and inasmuch as the pathogenicity of the infecting organisms varies from time to time, the patients' symptoms are correspondingly inconstant. Often anæmic, the patient may exhibit a general lassitude and a general neurasthenic syndrome, or possibly periodic neuritis, lumbago, &c. More persistent manifestations may take the form of a canalicular optic neuritis or rheumatoid arthritis, &c. We must not further enlarge upon these systemic manifestations of sepsis in the nasal sinuses, in the periapical spaces, or in any other part of the body. But rhinologists must pay careful attention to the teeth whenever they find evidence of a latent nasal sinusitis with these systemic symptoms, and conversely in cases with signs of periapical sepsis the dental surgeon should not omit to investigate the possibility of an associated or dominant nasal source of these symptoms. In some of my own cases treatment directed to nasal sinus infection was more or less disappointing because I had overlooked the co-existence of apical dental sepsis, and in others where the patient had had all the teeth removed little benefit followed because the chief source of infection lay in the nasal sinuses. We want to collaborate in order to help patients more effectively.

One point I desire to emphasize is that a "latent," slightly purulent, septic focus may be a source of systemic symptoms as well as the more locally active, purulent infections, for pus consists essentially of polymorphonuclear lymphocytes which phagocyte or inhibit the infective organisms. We know that a dissecting-room or post-mortem infection is less dangerous when it becomes actively inflamed and rapidly breaks down from the very free invasion of pus cells, whereas the non-suppurating infection spreading up the lymphatics is much more dangerous and apt to end in acute general blood poisoning. On the other hand, locally active suppurative processes may cause widespread infective complications which may so overshadow the original and determining source that, whether it be dental or nasal, the surgical measures essential for the patient's cure may easily elude our notice.

Is it not true that patients whose septic gingivitis is most purulent and whose pockets of pus are numerous, often suffer less in general health than do a large group in whom the pyorrhœa is "latent" with no macroscopical evidence of pus? In these quiescent cases the term pyorrhœa is really misleading; there is little or no pus but inflammatory thickening of the periodontal membrane associated with a progressive decalcification and absorption of the alveolar bone and its coverings. The lesson is that *local inactivity of a septic focus is no criterion of its clinical import and danger*. Hence for the dental surgeon and for the rhinologist the general symptomatology is often the best guide for active therapeutic measures.

DENTAL CYSTS.

It seems to be generally accepted that dental cysts are formed (as a result of stimulation by the chronic infection of a periapical granuloma) by proliferation of the epithelial cells of Malassez, the mass forming a cavity filled with clear *sterile* fluid with a definite fibrous capsule. The cyst tends to increase in size and invade the bone. Thus a dental cyst is a result of periapical infection, almost invariably derived from septic pulpitis in a dead tooth. Seeing that apical abscess can result from primary antral infection involving a sound tooth, is it possible that chronic antral infection may also cause the formation of a dental cyst? (We make no reference to follicular odontomes as they are not due to infection of the teeth or gums, although they may call for treatment on much the same lines as a dental cyst.)

The stereoscopic radiograms which are shown have been taken for me by your President in cases of antral sinusitis. I think that some of these suggest that sometimes the so-called spaces are in reality traversed by canaliculæ which are only partly absorbed (osteoid tissue) and are not constantly indicative of true granuloma or abscess—in fact that they are due rather to a progressive halisteresis and absorption of bone, similar to what occurs so distinctly in the alveolar plates in pyorrhœa. To one of these I would invite attention as it was taken for me sixteen years ago by the late Sir James Mackenzie Davidson, and as an example of stereoscopic skiagraphy of the nasal sinuses and teeth it would be hard to excel to-day.

Much yet remains to be learnt for the correct interpretation of odontograms and in some cases the stereoscopic pictures are more helpful than single prints. The absorption of alveolar plates and progressive halisteresis may be but natural processes of senile degeneration, the analogue of bald heads. Even abnormal appearances in the periapical region cannot, *per se*, be held always to justify multiple extractions, in the absence of clinical symptoms or other evidences of pathogenetic focal infection of dental origin.

Sir JAMES DUNDAS-GRANT

asked for more evidence of extension of disease from the antrum to the teeth. He felt uncertain as to whether the presence of micro-organisms on aspiration justified a diagnosis of antral inflammation in patients who, as Dr. Watson-Williams stated, might still enjoy power of resistance to infection from them. The speaker narrated a case of pain in the nose, seated in the anterior part of the inferior turbinated body (supplied by the anterior dental nerve) accounted for by disease of an incisor tooth, cured by extraction of the tooth. In case of antral suppuration in children he deprecated operation through the canine fossa for fear of injuring the germs of permanent teeth in the superior maxilla.

MR. HERBERT TILLEY

emphasized the importance of otologists teaching students that earache associated with a normal tympanic membrane and good hearing was frequently caused by a carious upper third molar tooth—in other words it was a “referred” or “reflex” symptom. He quoted instances in which a combination of labyrinthine deafness, tinnitus and vertigo (Ménière’s syndrome) had been cured by the treatment of pyorrhœa. With regard to antral infection of dental origin, he reminded members of the Section that in 1903¹ he had read a paper before the Odontological Society in which this point was emphasized, and it was suggested that diseased conditions of the teeth other than a septic pulp cavity might induce antral suppuration. On that occasion the dental surgeons who were present were almost unanimously of the opinion that infection of the sinus could only take place when the pulp cavity was infected and the tooth “dead.” He gathered that present opinion was now in favour of the view which he expressed nearly twenty years ago. He would be glad to have an authoritative statement on the point from any of the dental surgeons present. His own experience suggested that at least three-fourths of the cases of chronic antral suppuration were due to intranasal infection. With regard to tonsillar infection, he thought that the removal of diseased teeth was often followed by a diminution in the size and in the septic condition of the tonsils. This was particularly noticeable in young children. Dr. Watson-Williams’ views as to the possibility of latent infections of the sinuses giving rise to symptoms were well known to rhinologists and his observations and logical deductions could not be summarily dismissed. All agreed that a purulent focus in a sinus should be drained and cured, but were they all equally unanimous that the non-purulent contents of a sinus, in the absence of other local pathological conditions, might be of a high degree of infectivity? If so, it would seem that many *apparently* normal sinuses must be opened and drained by more or less radical operations. This appeared to many to be rather an alarming proposition. Bacteriology might help to settle the question because if it could be proved that in normal conditions the secretions of the sinuses were sterile, then the presence of pathogenic organisms would go far to establish Dr. Watson-Williams’ views and the treatment based on them. He (Mr. Tilley) was not aware that any extensive “control” experiments of this nature had so far been carried out.

MR. MARK HOVELL

referred to the case mentioned by Mr. Tilley in which pyorrhœa was found to be the cause of Ménière’s symptoms—but the explanation was simple: Ménière’s symptoms were the result of the co-existence of gastro-intestinal derangement, generally sepsis, and middle-ear catarrh, or an abnormal state of the Eustachian tube. Pyorrhœa produced gastro-intestinal derangements and these produced Ménière’s symptoms. He agreed with Dr. Watson-Williams’ statement that transillumination was an uncertain test for antral abscess. He (Mr. Hovell) had discovered this many years ago and was surprised at the reliance still placed on this supposed test.

¹ See *Trans. Odont. Soc. Great Britain*, 1903-4, n.s. xxxvi, pp. 35-90.

Mr. A. T. PITTS

said that he was very interested in hearing what Dr. Watson-Williams had said about nasal sinusitis in infants. He had seen two cases, recently, of osteomyelitis of the maxilla in infants. In both cases there were sinuses present on the gum which led to the unerupted teeth. In one case he removed a molar which was hypoplastic, and in the other a canine and molar which were both deformed. It had seemed to him that the infection of the developing tooth germs must be secondary to some infection elsewhere, but Mr. E. D. D. Davis in conversation had informed him that some authorities considered that the teeth were primarily infected. He was also interested in Dr. Watson-Williams' statement, that in cases of periapical infection the sockets of the teeth should be curetted and the outer alveolar bone removed. Surgical extraction, as it had been called, had been extensively advocated and practised in America, though in this country most dentists did not think it necessary. In the maxilla there might be considerable risk of opening into the antrum. He should like to have heard some more from Dr. Watson-Williams as to the treatment of dental cysts in the maxilla. Many dentists (himself included) had thought that in most cases the cyst encroached on the antrum and did not actually invade it. The bony floor of the antrum became absorbed but the mucous lining remained intact so that the antral cavity, although it might become greatly diminished, remained shut off from the cyst. This had an important bearing on the method of treatment. Assuming it to be true, unless there was clear evidence that the antrum was involved or infected, the inner part of the cyst wall should be left clear—no attempt should be made to dissect it out, lest, in so doing, the antrum might be opened. He had followed this practice himself and had found that if a large external opening was made the healing was rapid and uneventful.

Mr. W. STUART-LOW

said that just as at the former combined meeting between the Sections of Ophthalmology and Laryngology, held some years ago,¹ it was agreed that an operation on the eye could not be safely undertaken until the rhinologist had freed the sinuses of sepsis, so at this discussion it would doubtless be agreed that operative treatment could not be safely undertaken on nasal sinuses until the teeth and mouth had been freed from sepsis. He was also of opinion that chronic antral sepsis was a result of neglected acute antral sepsis which almost always arose during the course of influenza. The practitioner not having the advantage of transillumination could not be expected to diagnose this trouble correctly, the result being that this was often allowed to drift on to a chronic condition. One proof that chronic antral sepsis resulted from derangement of anatomical conditions in the nose was that disease of the antrum almost always happened in a narrow nasal passage where a deviation of the septum existed, and while disease of the teeth no doubt aggravated antral trouble it was rarely his experience to find it the only cause.

Mr. E. D. D. DAVIS

said that Grünwald had stated that out of ninety-eight cases of antral supuration only fourteen were definitely due to dental infection. Out of 101 of his (Mr. Davis's) own cases, twelve had been of dental origin.

¹ "Discussion on Injuries and Inflammatory Diseases affecting the Orbit and Accessory Sinuses," *Proceedings*, 1919, xi (Sect. Ophth. and Laryng.), pp. i-lxvii.

Antral suppuration of dental origin could be distinguished clinically from that of nasal origin by the following points: (1) The presence of an alveolar abscess or pyorrhœa of certain teeth; (2) the antrum being the only cavity of the nose involved; (3) the character of the pus, which was very foul, smelling strongly of *Bacillus coli* and having a greyish sandy deposit, while antral suppuration of nasal origin was more chronic and produced muco-pus of slight or fusty odour; (4) the dental antrum being more easily cured by drainage than that of nasal origin.

Dr. Watson-Williams had spoken of nasal antral infection producing an apical abscess of the teeth. He (Mr. Davis) had never seen it and he would call attention to the fact that apical abscesses without caries occurred just as commonly in the mandible as in the maxilla. Neither did he believe that a dental cyst could arise from a nasal infection. A cyst of such an origin would certainly not be a dental cyst. He agreed with Mr. Pitts that dental cysts should be drained into the mouth, and opening into the nose and antrum should be carefully avoided because the cyst lay well below the floor of the nose, hence drainage into the nose was not good and a fistula between the mouth and nose might result. The cyst encroached on but did not invade the antrum.

The question of mouth-breathing and nasal obstruction was perhaps too big a subject for present discussion, but he (Mr. Davis) had frequently seen patients with as free and as patent a nose as a Cunard funnel, but yet they were mouth-breathers, and dental surgeons had been more successful than the laryngologists in treating the habit of mouth-breathing by the use of the rubber mouth screen.

Dr. WATSON-WILLIAMS

(in reply) pointed out that the question of the relative value of various clinical tests in cases of sinusitis was outside the discussion, but as the question had been raised, he would say that the reliability of the methods in use would have to be decided by personal experience. Cystic degeneration of the nasal mucosa, especially that arising in the nasal floor, might resemble dental cysts although of very different origin.

Of course nasal sinusitis very frequently had nothing to do with the teeth either in cause or effect; on the other hand, it was doubtless true that many, if not most, diseases of the maxillary teeth were unassociated with nasal sinusitis. Nevertheless, the purpose of the meeting had been to discuss and consider cases and conditions in which there was such a connexion, in order that some of the interesting problems arising in that way might be cleared up.

In reply to Mr. Mark Hovell, Dr. Watson-Williams said that the vibrissæ of the nasal vestibule caught most of the inspired organisms and were therefore always "septic," but in the nasal passages proper there were very few organisms in health and none in the sinuses. They wanted more reliable guidance for extraction of teeth that appeared sound, and the abnormal appearances at the apical space revealed by odontograms required cautious interpretation. Radical operative procedures for slightly septic tonsils, slight infections of the sinus and doubtful apical and dental sepsis, were to be deprecated, but when clinical symptoms could be definitely connected with such local infections operative relief for their removal was indicated.

Section of Odontology.

President—Mr. W. R. ACKLAND, M.R.C.S., L.D.S.Eng., M.D.S.Brist

Case of Multiple Dentigerous Cysts.

By BERNARD GRELLIER (M.C.), L.R.C.P.Lond., M.R.C.S.,
L.D.S.Eng., D.M.R.E.Camb.

EARLY in May last year I saw at the Royal Dental Hospital a boy, aged 13, with multiple cysts involving the maxilla and mandible. When I removed them I sent them to Mr. Mummery, who had kindly consented to make a pathological examination. Mr. Mummery and I are bringing this case to your notice this evening. I propose before Mr. Mummery gives us an account of the thorough investigation which he has made of these cysts, to preface his account with a few brief remarks on the clinical aspect of the case.

The case was diagnosed as one of multiple dentigerous cysts. When the boy first attended the hospital his face showed marked swelling on both sides in the region of the malar prominences and a slight fullness on the right side over the mandible. The history given was that a swelling on the left side of the face was observed eighteen months previously; it had increased since then. Otherwise nothing had been noticed and no pain had been felt, till about a fortnight before he attended the hospital, when a throbbing pain was felt on the right side of the maxilla, and this was followed in a day or two by a discharge into the mouth. He was anæmic in appearance, and looked somewhat delicate, but did not complain of ill-health.

His past history was that at birth there had been some inflammation of the eyes; when 1 year old he suffered from meningitis, pneumonia, and an inguinal hernia; when 3 years old he had had measles and whooping cough, and when aged 11 he was attacked with broncho-pneumonia following influenza. His tonsils and adenoids had been removed. With regard to the teeth, eruption had been delayed till he was nearly 2 years old. There was no other dental history, and there was no family history forthcoming.

On examination of his mouth it was found that the teeth present were the permanent upper and lower incisors, all the deciduous canines, the first and second deciduous molars, and the four permanent first molars. The tip of the left second upper premolar was just showing on the outer side of the alveolus above the second deciduous molar. All the deciduous molars were badly diseased; in fact only the roots remained, except in two of them. The other teeth were healthy.

In each region of the diseased deciduous molars there was a swelling. The left upper swelling was the largest. It extended from the mid-line to the first permanent molar on the outer side of the alveolus, passing high into the buccal sulcus. Over its most prominent part it fluctuated, and it also yielded the so-called egg-shell crackling sensation. There was some corresponding

swelling in the palate, but it was not so extensive. There was no sign of inflammation.

The right upper swelling was similar, but not so large. On its outer side there was a sinus discharging pus. In the mandible on the left side there was a slight bulging of the outer alveolar plate, and two hard nodular swellings on the inner side, which were probably the two unerupted premolars forced inwards by the cyst. On the right side of the mandible the swelling extended from the deciduous canine to the first molar, and fluctuation could be felt on the outer side.

In removing each cyst the portion of tissue holding the deciduous molars was dissected away, attached to the cyst without disturbing the relation of the deciduous teeth with the cysts. With regard to the unerupted premolars it was not found possible, when dissecting out the cysts, to ascertain their relation to the cysts. In the cysts of the mandible no tooth was found, and the premolars could not be seen in the cavities left by removal of the cysts. In the right upper cyst the second premolar was removed with the cyst, and was lying loosely in it. It was well developed, except for the upper part of the root, and in his examination Mr. Mummery found its relation with the cyst. In the case of the left upper cyst both premolars were removed with the cyst, but the second premolar had been driven inwards during the operation—the tip of the crown was just showing through the gum. With regard to the first premolar, I could not say what position it occupied in relation to the cyst. The cyst on the right side was the only one which had suppurated.

The course of healing was uneventful. Deep cavities resulted from the operations, but they are diminishing in size. About a month after the cyst on the left side of the maxilla was removed the canine formed a swelling in the anterior wall of the cavity and later erupted into it. In the mandible the premolars were just erupting after about two months, and in the maxilla on the right side the tip of the first premolar was showing in the cavity.

I hope to see the patient again soon, and I am anxious to learn how far the cavities may be obliterated by new bone formation, and how the teeth will erupt. (Radiographs were shown.)

Case of Multiple Dentigerous Cysts.

By J. HOWARD MUMMERY, C.B.E., F.R.C.S.

AN interesting case of multiple suppurating dentigerous cysts was described by Mr. Sprawson in a paper read before this Section in 1922,¹ and by a curious coincidence this very similar case came under the care of Mr. Grellier, at the Royal Dental Hospital very shortly afterwards. It is remarkable that cysts in association with deciduous teeth had not been described and when Mr. Sprawson wrote his paper there did not appear to have been any case recorded, but before he read the paper he found and showed at the meeting a dental cyst containing epithelium in connexion with the root of a deciduous molar. Sir Frank Colyer recently sent me a cyst attached to a deciduous tooth in which I found a large quantity of epithelium and a distinct epithelial lining to the cystic cavity, and I prepared similar sections from the cysts on the deciduous teeth in Mr. Grellier's specimens, so that it would appear that cysts containing epithelium arising from deciduous teeth are not so rare as had been supposed. It is probable that

¹ *Proceedings*, 1922, xv (Sect. Odont.), p. 56.

from their usually small size and the early absorption of the teeth they had been overlooked. These were true dental cysts, in every respect conforming to those found on the roots of permanent teeth.

The first cyst (fig. 1) removed by Mr. Grellier was a suppurating cyst—it came away in a very complete condition and showed some very interesting points. It was removed from the premolar region of the right maxilla and showed a double sac attached to the deciduous molar. There was a slight constriction at the junction of the sac attached to the deciduous molar with the dentigerous cyst below it, but otherwise they appeared continuous. A premolar came away at the same time; it did not remain attached to the cyst, but the tooth fitted accurately into the opening in the lower part of the cyst

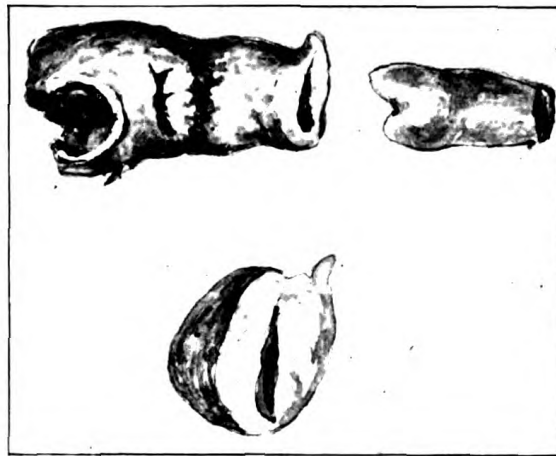


FIG. 1.—Cyst No. 1.

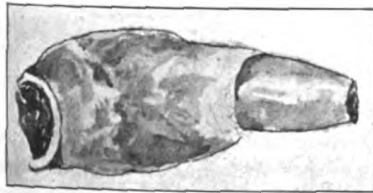


FIG. 2.—Cyst No. 1.

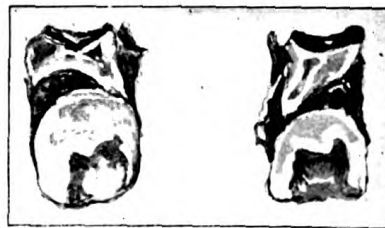


FIG. 3.—Cyst No. 1.

(fig. 2). Without doubt the crown of the premolar had been within the cystic cavity as the impressions of the two cusps, one longer than the other, were plainly seen within it and the tooth fitted quite accurately into these depressions. As in dentigerous cysts generally, the cyst wall only surrounded the crown of the tooth, the incomplected root being altogether outside it. A vertical section, and also microscopical sections of this case, showed the line of separation between the dentigerous cyst and the granuloma on the root of the deciduous molar, if it could be called a granuloma, for we can only say that the dentigerous cyst was separated from the deciduous molar by bone, connective tissue and numerous blood-vessels and a large quantity of fat (fig. 3). In one part, however, there were a few clumps of epithelial cells

but very little granulation tissue. It is difficult to say if this is a true cyst as it appears to be made up principally of connective tissue. It might, perhaps, be looked upon as a cyst which has undergone further fibrous degeneration. The dentigerous cyst showed an epithelial lining, but the epithelium was not very abundant.

In the second case, the cyst, which was non-suppurating, could not be removed in connexion with the dentigerous cyst, but this large cyst attached to the lower right deciduous molar had all the characters of an ordinary dental cyst. Sections showed enlargement of the epithelial rests, abundant epithelium lining the cystic cavity and cholesterine crystals. The dentigerous cyst in this case was removed separately but it can be joined up and appears to form a complete closed sac, and as the cyst on the deciduous tooth was also closed there cannot apparently have been any direct communication between the two. Its connexion with the permanent premolar was not evident as the tooth did not come away (fig. 4).

The third case was very interesting and the cyst was removed entire. It is



FIG. 4.—Cyst No. 2.

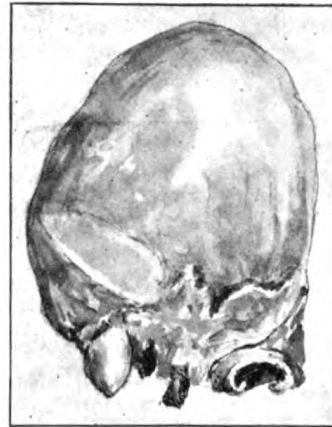


FIG. 5.—Cyst No. 3.

large and involves the roots of the deciduous canine and first and second molars on the left side of the maxilla. As it appears important that the whole of this specimen should be seen as much as possible in its original condition, I have not made any microscopical examination of the portion beneath the deciduous teeth although it may possibly be desirable to do this later. I have, therefore, placed the deciduous teeth in position and filled the sac with cotton wool so that the size and connexions of the growth may be clearly demonstrated (fig. 5).

There is a small cyst apparently connected with the root of the deciduous molar and having no connexion with the large growth beneath it. Two premolars came away in this operation and the radiograph appears to show that one of these teeth was within the sac, but it is impossible to be certain if it lies within it or external to the cyst wall. The other premolar appears to have been pressed aside by the growth and probably had no connexion with it.

The cyst in the fourth case could only be removed piecemeal and does not give any definite information regarding the origin of the dentigerous cyst, but there is a cyst in connexion with the deciduous molar.

All these cases appear to point to the fact that the dental cysts on the deciduous teeth have no direct connexion with the dentigerous cysts. The question arises whether in these cases the irritant which gave rise to the dental cyst has not at the same time been the cause of the proliferation of the epithelial rests surrounding the forming permanent tooth, and whether the two cysts have been formed independently yet are due to the same cause. It seems difficult to understand the symmetrical formation of these growths in both maxilla and mandible.

Mr. EVELYN SPRAWSON said the authors had recorded a very interesting case. Mr. Mummery had remarked on the symmetry of the cysts in his case; whether coincidence or not the cysts in the case he had himself recorded last year were also symmetrical, but here they were all in the mandible. He noted with great interest that in Mr. Mummery's case a deciduous tooth was still *in situ* over each cyst and that in each case it was dead and infected, this being a point upon which he had laid some stress in describing what he believed to be the pathology of such cysts. Since he had read his own paper he had had occasion to cut sections of some six or seven granulomata on deciduous teeth, and in each case he had found *masses* of epithelial cells present in them. He thought that they differed in this respect from granulomata on permanent teeth where epithelium was by no means always to be seen. Mr. Mummery had somewhat stressed the point that he thought the cells giving origin to these cysts were always those in the coronal portion of the permanent tooth follicle. This he (Mr. Sprawson) had himself mentioned as being a possible source, but did not conceive it possible to say that it was the only source, seeing that epithelial cells from the three possible origins which he had given were so condensed into a small area—frequently in life measuring 0.5 mm. or less in diameter—that the source of origin was necessarily lost, particularly as this area moved with the growth and eruption of the tooth. Mr. Mummery had also referred to the cysts having the histological characteristics of dental cysts; he (Mr. Sprawson) himself held that, histologically, both dental and dentigerous cysts were identical except that in dental cysts squamous epithelium occurred with greater frequency as a lining. Lastly, he thought it quite conceivable that multiple cysts might originate on a tooth with many roots such as a deciduous molar, and that by the process of absorption of the molar roots the cysts might become separated from the deciduous tooth from which they originated. Since he had read his paper he had collected some six cases in which the overlying deciduous tooth was still *in situ*, and in each of these it was dead and infected, and so far confirmed his statements.

On the Vascular Supply of the Enamel Organ of *Felis domestica*.

By EVELYN SPRAWSON (M.C.), L.R.C.P.Lond., M.R.C.S.,
L.D.S.Eng.

THE slides shown are photographs of transverse vertical sections of the developing mandibular second premolar of *Felis domestica*. The sections from which they were taken, which are also exhibited, show, in addition, the preceding deciduous tooth *in situ*. They were prepared by decalcification and freezing, staining with Ehrlich's acid hæmatoxylin and mounted in Farrant's medium, any shrinkage due to dehydration and other non-aqueous reagents being thereby avoided. The developing premolar has a large part of the dentine of the crown formed, also a portion of that which goes to form the root. An appreciable thickness of enamel has also been laid down, but not

[April 23, 1923.]

fully calcified, in that the matrix of the portion so laid down still remains after decalcification; though at the coronal portion this matrix has been broken away in the manipulation of the sections.

Fig. 1 is a photograph, at a magnification of 109 diameters, of the coronal portion of the first section shown. The tissues seen, from above downwards, are, I take it, as follows: Uppermost there is a layer of alveolar bone forming the roof of the crypt of the developing tooth; bordering on its inferior aspect there is seen an irregular layer of large multinucleated cells, each cell of which fits into a roughly crescentic excavation in the bone; from their position, the stage of growth of the specimen, and the type of cell, they are evidently osteoclast cells engaged in absorption of the roof of the crypt preparatory to the commencing process of eruption of the underlying forming tooth.



FIG. 1.—Coronal aspect of enamel organ and adjoining tissues. ($\times 109$.)

Below this is a layer of connective tissue, moderately rich in cells, the deeper portion of which I think represents the tooth follicle and, blending with the superficial portions of the next layer, it can be traced laterally into direct continuity with the tooth follicle. I say "represents," because in different specimens, during its development, and particularly at its coronal portion, the follicle presents a very variable thickness, and an appearance which may be quite different from that of the adult follicle as usually cut and described; the thickness largely depending on the proximity and amount of hard tissue, bone or tooth, which lies in the immediate path of the advancing tooth, and whether active absorption is going on or not. I shall refer to this again shortly.

This tissue is occupied by numerous capillary blood-vessels, which are

seen in its middle and more particularly its deep portions, several vessels in this latter position lying in immediate contact with the next layer, the external epithelial layer, but none are seen to penetrate it. Most of these vessels are cut transversely, and here form a very definite plexus immediately external to this, the coronal portion of the enamel organ. Vascularity at this point is a well recognized feature.

We now come to the enamel organ, which has, at this stage of its greatest activity, already undergone certain changes from its original definitely outlined



FIG. 2.—Same as fig. 1, but $\times 455$.

structure, in that the external epithelial layer no longer exists as a definite layer of polygonal cells, but rather as a layer of flattened cells, very similar to those cells of the sheath of Hertwig found at the point where a fully developed tooth follicle joins the neck of the tooth. They are not unlike fibroblasts and, except by analogy and comparison with the layers on either side of it, not easily recognizable as being epithelial cells. They merge into the adjacent and external connective tissue (follicle) layer on one aspect, and into the stellate cells on the other, but in spite of this still maintain themselves as a very definite layer, typical of the form assumed by this layer when condensed by the growth

of the crown of the forming tooth. Deep to the external epithelial layer there is a layer of cells, some nine or ten cells deep at the coronal portion, but dwindling away on either side, and there becoming flattened, consisting of the typical normal cells of the stellate reticulum, as shown when they are prepared by any histological method which eliminates shrinkage during the preparation and mounting of the section. Apparently there are no blood-vessels among these cells, but they are present on its deep aspect. The typical structure of these cells shows a good deal better under the higher magnification of 455 diameters, as seen in the next two slides (figs. 2 and 3). The stratum inter-

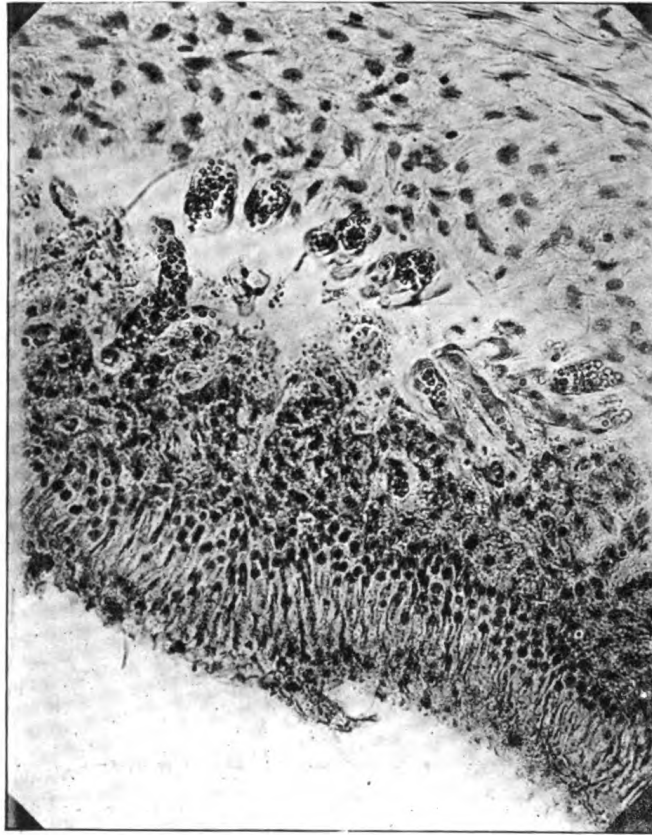


FIG. 3.—Same as fig. 1, but $\times 455$.

medium lies deep to these stellate cells, and consists of the typical cells of this layer, though the layer seems denser than one usually sees it, possibly owing to some obliquity in cutting. Beneath these is a normal ameloblast layer, some of the cells of which are cut slightly obliquely.

A great many capillary blood-vessels, mostly cut obliquely or longitudinally, and containing blood corpuscles, are seen lying on the superficial aspect of these coronal cells of the stratum intermedium. Several of these blood-vessels apparently penetrate the stratum intermedium layer of cells and meander

among them as far as midway in the thickness of this layer, but in some parts of this section and in others these capillary vessels may be seen nearly, if not quite, in contact with the ameloblast cells. Apparently this is a capillary plexus within the enamel organ occurring at the coronal portion of the tooth. I do not know the exact age of the animal from which the sections were cut, but from data since furnished me I gather that it must have been about $3\frac{1}{2}$ to $4\frac{1}{2}$ months old—the eruption of its permanent mandibular incisors was just commencing.

It has been suggested to me that the layers which I have taken to be the stellate reticulum and the flattened remains of the external epithelium are the tooth follicle, and that if they be traced out laterally they will be found to be in direct continuity with it; I do not think this is so, and the next slide (fig. 4) taken at a magnification of about 55 diameters will throw some light on this.

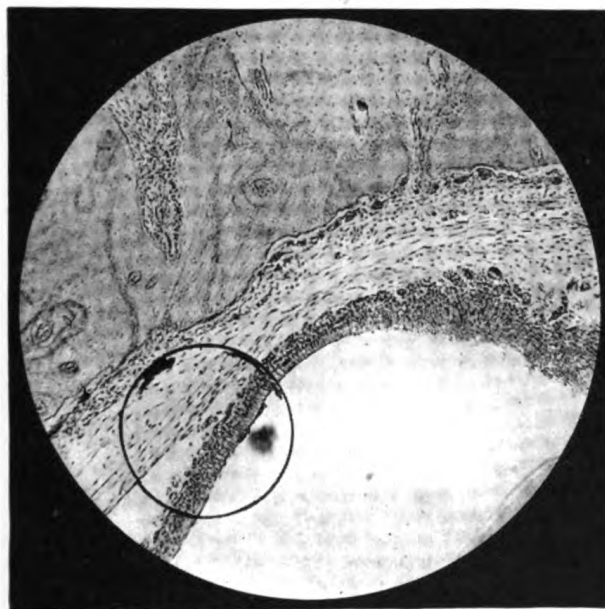


FIG. 4 —Lateral and coronal aspects of enamel organ and adjoining tissues, $\times 55$ (circ.).

In this photograph I would point out that the layers which I have called the external epithelium and the stellate reticulum—the last rapidly disappearing remains of which are found at the coronal aspect only, become condensed in their course down the sides of the tooth. The stellate cells disappearing, the external epithelium continues as a fairly definite layer of cells, which persists throughout the whole extent of the enamel organ, elongated and flattened, though not to the same extent as at the coronal aspect, which is what would be expected. But in those parts where manipulation has separated the follicle from the enamel organ (*see also* fig. 5) this layer has remained attached to the follicle, but still is differentiated from the remaining cells of the follicle—just in the same way as the several layers of epithelium which are the ultimate remains of the enamel organ are found lining the normal adult tooth follicle, and eventually go to form Nasmyth's membrane.

Further, the lateral cells of the follicle, where growth is comparatively slow, have the thinned-out appearance usually associated with and possibly due to condensation; but those cells which I have called stellate reticular cells have by no means this appearance, though growth being most active at that point where they are seen, one might well expect the evidence of condensation to be at its maximum in this position. Moreover it is not usual, as far as I know, to find two such different types of mesoblastic cell, or in two such definite layers composing the tooth follicle, and, in addition to this, avascular. It is also well known that immediately external to the enamel organ, and frequently emitting many capillary loops dipping into the external epithelium,

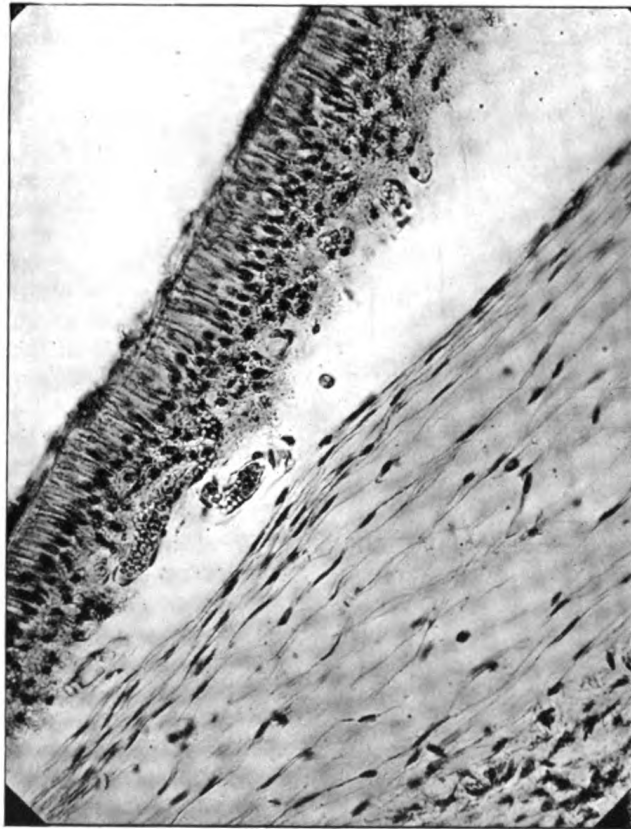


FIG. 5.—A portion of fig. 4, but $\times 455$.

there is a very dense vascular plexus, which, I presume, is represented in these slides, external to the enamel organ layers as I have named them; if these layers really represented the follicle there is no reason why this plexus should be so totally excluded from it, as it rather definitely is.

I mentioned earlier the great variability in appearance of the follicle in various stages of growth; this is confirmed by the next slide (fig. 6), which is a photograph of a section of another portion of the same tooth. Here it will be seen that the outer two layers of the enamel organ have entirely disappeared and that the developing tooth is very closely approximated to its deciduous predecessor. Very active absorption must obviously be going on, yet the

vascularity of the part external to the remains of the enamel organ is poor, compared with the vascularity shown in the other slide. I think one may conclude therefore that the outer vascular plexus shown in fig. 1 is that situated, normally, immediately external to the enamel organ, and that therefore the two layers are as I have named them. One might almost say that at the point shown in this slide the follicle hardly exists at all, it is so thin; in life the measurement from the ameloblasts to the dentine is rather less than $\frac{1}{800}$ in., and this area includes the stratum intermedium and absorbent organ layers. Incidentally this slide rather aptly illustrates a point I emphasized in describing the pathology of dentigerous cysts, for here, in this very narrow and confined space, might be found epithelial cells: (1) from the coronary portion of the permanent tooth follicle, (2) from the sheath of Hertwig of

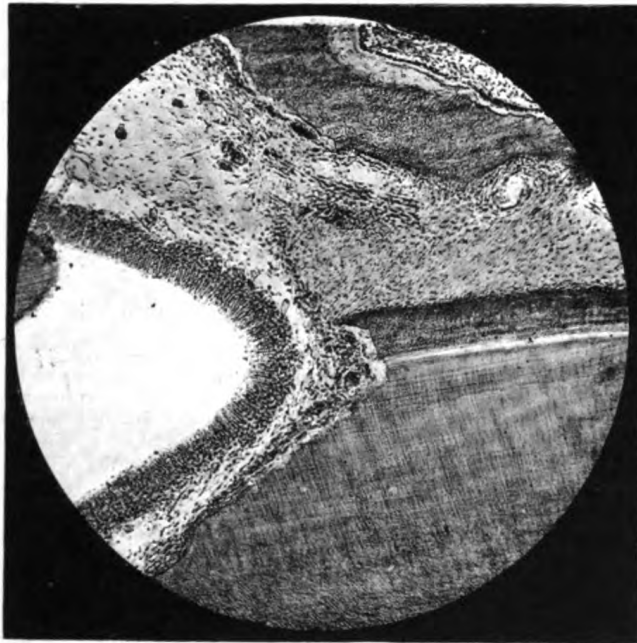


FIG. 6.—Part of the same enamel organ as shown in figs. 1-5, but at a later stage and nearer deciduous predecessor, $\times 50$ (circ.).

the deciduous tooth, and (3) from the tooth band. Considering how condensed they are at this point, I do not think anyone can say from which of these three possible origins any epithelial growth in this region of the apex of the deciduous tooth really starts, for besides being condensed it is not stationary, but moves with the erupting tooth.

In describing the external epithelium of the enamel organ, Mr. Howard Mummary [1] says that these cells

in later stages are often elongated, their axes lying parallel to the surface. The cells of this layer merge into those of the stellate reticulum.

This I think is shown. Still, one must bear in mind that he also says [1] :—

There is little doubt that many preparations said to show blood-vessels in the stellate reticulum, really showed them, not in this layer, but in the connective tissue outside the enamel organ, which, when the stellate reticulum has disappeared, lies in contact with the stratum intermedium and often very strongly resembles it in appearance.

It must, however, be admitted that the layers in these slides seem fairly well differentiated. Vascularity of the enamel organ has been described in varying extent in several species of marsupial by Marett Tims and Hopewell Smith [2], by Thornton Carter [3], and by Bolk [4]; in a monotreme and a rodent by Poulton [5], and in a foetal ungulate by Broomal and Fischelis [6]. In all the foregoing instances capillaries were described as penetrating the stellate reticulum, but only to its full depth, or beyond, and into the stratum intermedium, in the case of the two marsupials *Macropus rufus* and *Phascogale carterii*.

More recently Addison and Appleton junior [7] have described vascularity of the enamel organs of the molars of the albino rat, and the photomicrographs they show are in many respects similar to those I have just shown. These authors find that vascularity is most marked at the coronal aspect where the stellate reticulum is thickest, and that it becomes more intense as the active development of the enamel approaches its maximum. This appearance seems to be somewhat similar to what is seen in *Felis domestica*, except that vessels cannot be found in the young enamel organs of this animal at all; they would seem to be a later development only.

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Section of Odontology.

President—Mr. W. R. ACKLAND, M.R.C.S., L.D.S.Eng., M.D.S.Brist.

Two Odontomes.

By Professor ARTHUR HOPEWELL-SMITH, M.R.C.S., Sc.D.

(A) A COMPOUND FOLLICULAR ODONTOME OR COMPOUND COMPOSITE ODONTOME.

OF the two odontomes about to be described the first belongs to a type which is familiar to the general and dental pathologist, in that it probably represents a good example of a compound follicular odontome, or a compound composite odontome according to the classification of the British Dental Association's Special Committee. I am indebted to Mr. A. R. Walker, of Brisbane, for the specimen.



FIG. 1.



FIG. 2.

Fig 1.—Before operation. Shows above, the unerupted permanent canine, below, the deciduous canine, between, the denticles *in situ*.

Fig. 2.—After operation.

The clinical history states it occurred in the right maxilla of a girl aged 14. The deciduous canine had been unduly retained, a bony prominence over the position of its unerupted permanent successor marking the site of the odontome. The left permanent maxillary canine was fully erupted, and in normal alignment with the dental arch.

A skiagram of the jaw disclosed the presence of the permanent right canine. Between it and the deciduous tooth were shadows indicating abnormal formations of apparently dental tissue (fig. 1). The deciduous tooth

was removed, the cyst opened up (fig. 2). The contents consisted of fluid and a congeries of small denticles "implanted in a rudimentary kind of bone."

Seven in number, the denticles conform generally in shape with definite, primitive tooth forms reproduced on a miniature scale. Unlike the 500 osseous masses found by Sir John Bland-Sutton (*Transactions of the Odontological Society*, vol. xx), in the right antrum of a girl, aged 11, or the 109 denticles described by Mr. Ward Cousins in a paper in the *British Medical Journal* of June, 1908, taken from the jaw of a boy, aged 11, situated in the region of the second right mandibular molar, they more nearly resemble those observed by Mr. A. A. H. Johnson in a case recorded by him in the *Proceedings of the Royal Society of Medicine* (Section of Odontology) vol. iv, and more recently by Mr. Gerald Harborow three years ago.

Owing to their small dimensions I have considered it inadvisable to make sections. It is probable, however, that each is composed of enamel, dentine, and cementum. It was impossible to measure them with any degree of

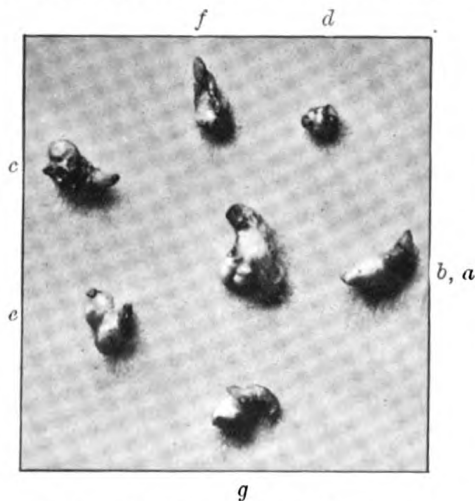


FIG. 3.—Denticles removed from cyst.

accuracy, but their weights, as computed by Mr. Frederic James, are as follow: (a) 0.0342 grm., (b) 0.0542 grm., (c) 0.0218 grm., (d) 0.0090 grm., (e) 0.0298 grm., (f) 0.0312 grm., and (g) 0.0266 grm. (fig. 3).

(B) A RADICULAR ODONTOME.

I have to thank Dr. Edward C. Kirk for submitting to me the second odontome for examination and report. It had been sent to him by Mr. E. C. Stephens, D.D.S., of Choteau, Montana, into whose possession it originally came, to whom my thanks are also due.

The clinical history notes that the tumour was removed from the jaw of a girl, aged 8, who presented a hard swelling on the left side of the mandible in the neighbourhood of the deciduous molars. Six months before the operation there had been no noticeable distension of the parts, but the second deciduous molar had become so loose that the child unsuccessfully tried to force it out with her fingers. No pain was complained of.

In operating, infiltration anæsthesia was employed, an incision made

through the "gums and periosteum from the crown downwards," and these retracted on both sides. The tooth with growth was easily detached, being connected to the mandible by "fibrous tissue at its base." The cavity was curetted, surgically treated, and the gingival tissues stitched together. "It readily filled and healed without complications." No skiagram was obtained.

The odontome exhibits the constricted neck and divergent roots of a typical carious deciduous molar with a large, hard, rounded, lobulated, fairly smooth mass attached to its roots. The relationship of the growth to the tooth is excentric. Its greater part is anterior, the distal side being free except at the extremity of the root. Both roots are exposed, the anterior slightly, the posterior on the distal side very greatly. The colour of its



FIG. 4.



FIG. 5.



FIG. 6.

surface is grey-brownish, this being due to the discoloured, very thick and tough fibrous capsule (figs. 4, 5).

Its greatest dimensions measure 24.5 mm., its smallest 15 mm. The dimensions of the crown of the tooth are: Length, 0.10 mm.; breadth, 0.8 mm.; and height, taken from the neck to the extremity of the highest cusp, 5 mm. The tooth appears to be a member of the deciduous series, and the above measurements correspond more or less with those which obtain in ordinary conditions.

The mass is solid, hence it cannot be considered to be a dilated composite odontome. On sections being made it is seen to be of fairly uniform texture with a central more heavily consolidated portion—a fact well brought out by the skiagram. Its upper part extends between the roots of the molar, with which it appears to be continuous (fig. 6).

The causation and pathology is difficult to determine. Wedl, in his "Atlas of the Pathology of the Teeth," gives drawings which resemble this odontome more closely than any other that I have found in dental or surgical literature. In his case a deciduous mandibular right canine and first molar were extracted from the mouth of a boy aged 11½. These consisted of a large, hard mass which microscopically was seen to be composed of a dentine-like, calcified material uniting the roots of the two teeth. It was evidently not considered to be made up of hyperplastic cementum, nor was there any osseous ankylosis present.

The same applies to the case under notice. The absence of pain so definitely stated in the history of the case, rules out the evidence of any reactions of an inflammatory nature. The mass may have been occasioned by an overgrowth of vestigial remains, but in the present state of our knowledge it is impossible to say.

Some Observations on the Histology, Physiology and Pathology of the Dental Pulp.

By Professor ARTHUR HOPEWELL-SMITH, M.R.C.S., Sc.D.

PROBABLY no subjects in the field of research have engaged the attention of the writer more than those of the minute anatomy, functions and diseases of the dental pulp. It might be conceived, from the title of this paper, that it was his intention to review the investigations made by himself and others in the structure and morbid conditions of this organ. Limitations of time exclude such an attempt. But while it is advisable to mention some facts already known, it is now necessary to bring modern knowledge to bear upon certain questions, and, by careful evidence and precise inference, to set forth in as concise a manner as possible some recent observations regarding it.

These remarks refer to the dental pulp of man, in many respects unlike those found in the mammalian lower vertebrates. Comparison between the two, for instance, shows some dissimilarity in the anatomy of the blood-vascular systems, and the extent and distribution of the nervous apparatus.

Omitting references to the teeth of fishes and reptiles, it should be noted that the pulps of the *carnivora*, *herbivora*, and rodents deviate considerably from that of man, and possibly of the anthropoid apes. These variations are not confined to anatomical characteristics only, but apply equally to function.

(I)

(A) ITS HISTOLOGY.

Mesodermic in origin, and formerly incorrectly described as resembling the structure of Wharton's jelly, the pulp is a community of connective tissue elements which are embedded in a transparent and slightly granular basic substance, retained in a delicate reticulum, partly formed by the interlacing processes of the cells themselves, and partly by numerous anastomosing independent connective tissue fibres, the peripheral parts of which bind the organ loosely to the dentine. Von Korff [1] especially studied these attaching fibres. I had previously noticed them, but did not emphasize their importance.

Occupying a cavity from which it is easily detached, the confines of which are composed of a unique calcified material, the tissue may reasonably be remotely compared with marrow, or soft tissue in the larger and smaller medullary spaces of bone. When the apical foramen of dentine—the homologue and analogue of the nutrient foramen in bone—is formed, the pulp does not disappear but remains *in situ*, and one of its functions—the building up of primary dentine—ceases. There is no evidence that in normal circumstances it continues to enclose itself in an ever-increasing thickness of dentine, but in nutritional and other disturbances this is a reaction which is constantly in operation.

(B) ITS CELLS.

Of all the cellular elements the most remarkable are those units of what was originally designated the *membrana eboris*—interesting not only on account of their extraordinary morphology, but also because they have excited some controversy as to their actual function.

Fifty-three years ago Waldeyer [2], writing in Stricker's "Histology," called attention to these surface cells which differed in their histological features from the other constituents of the tissue. Probably conceiving some distant analogy between them and the enamel-forming cells, then called adamantoblasts, he named them "odontoblasts," a term freely adopted and used to-day.

The Anatomical Term, "Odontoblast," a Misnomer.

It is obvious that, both etymologically and physiologically, this is incorrect—the word a misnomer. The dental structures are not the product of the same type of cell: ectoderm and mesoderm are both involved in their genesis.

In view, therefore, of the fact that a so-called odontoblast is not a tooth-former, as is implied, but is, without equivocation, the parent of the dentinal fibril which extends into each dentinal tube, I have for some time past entirely discarded the name, and introduced into dental histology, as more exact, the word "fibrilloblast." Further, regarding its function, observations point to it not being concerned in the formation of the matrix of the dentine. The fibrilloblasts, then, call for special notice and description.

The Fibrilloblasts.

Details regarding their shape, size, structure, relationships, have been fully given elsewhere and need not be repeated. Some full statement, however, must be made of further new studies concerning them.

Their number may be approximately computed. For as every fibrilloblast usually furnishes each dentinal tube with one protoplasmic peripheral process, and as the number of these tubes has been ascertained, a numerical estimate may now be recorded.

If dentine be examined in cross section in investigating the proportional occurrence of tubes in given microscopical fields, and in given areas in these same fields, it is as easy a matter to enumerate them as they appear in circular outline in one one-hundredths or one four-hundredths of a millimetre as it is to determine the ratio of erythrocytes in a normal blood count. Römer [3] has done this. I, too, have examined material to this end, and have found that on an average six or seven may be recognized in the former area—an observation checked and confirmed by one of my experienced laboratory assistants. Thus by a simple calculation it is evident that there may be no

less than 600 in a square millimetre, that is 60,000 in a square centimetre. Römer's figures are very much greater. He states that in a very densely distributed part of human dentine he has seen ninety-four in one four-hundredths of a millimetre, that is as many as 37,600 in one square millimetre, or 3,760,000 in one square centimetre.

It may therefore be surmised that certain regions of the dental pulp are covered with fibrilloblasts the numbers of which range between these figures.

As it is a matter connected with the determination of their function it must be remembered that they are highly specialized cells, different in every particular from the ectodermic epithelial ameloblasts which construct enamel, and the mesodermic osteoblasts which fabricate bone. Ameloblasts are long, cylindrical, regularly-arranged units, with conspicuous basal processes (Tomes's fibres), held together, as in the case of all epithelia, by means of a minute amount of intercellular substance. The latter are round, square, oblong, or oval in form, also possessing short processes. Of about the same dimensions they become, during bone formation, the bone cells, occupy the lacunæ in that tissue, and remain in continuity with each other through the medium of their processes which traverse the intervening canaliculi.

The Fibrilloblast Forms the Dentinal Fibril and not the Matrix of Dentine.

Each fibrilloblast possesses a very special peripheral process extending probably, but not with absolute certainty, through the greater part of the width of the dentine. As the coronal fibrilloblasts are larger than the radicular cells; as they vary in size when seen microscopically in sections cut in the same plane, being larger in the greater and shorter and smaller in the lesser diameters, it follows that not only in their diversities of size, but also in the variations of the lengths of their distal processes they are not anatomically comparable with osteoblasts. Fibrilloblasts vary from 5 to 3 microns in width and from 25 microns to 8-10 microns in length. Osteoblasts maintain, more or less, a uniform size. Hence it is permissible to assume that the physiological activities of the two are widely different and that the former perform a higher function than the latter.

Sir Charles Tomes is of the opinion that the primary function of the fibrilloblast is to keep patent the lumen of each dentinal tube and with this I fully agree. Each cell thus allows tissue fluid or lymph, filtered from the capillary or pre-capillary circulation, freely to penetrate the tube around its peripheral process. Metabolic and other vital processes taking place in the cytoplasm and karyoplasm of the cell endow the fibril with similar properties.

Dentine is not Innervated.

No evidence has been completely established to prove the presence of any structure other than the fibril, or any fluid other than lymph in the dentinal tube. It is too minute even at its largest pulpar extremity to accommodate a single red blood corpuscle. Blood never extravasates into dentine, but in certain circumstances plasma and the products of hæmolysis may do so with subsequent and inevitable discoloration of the parts.

This tissue is destitute of nerves, although appearances which may be interpreted as nerves are easily produced, by following certain methods of microscopical technique.

Many evidences, however, of a clinical, physiological, histological, and pathological character are forthcoming—if sought for—to show that dentine

is not innervated. To take two only out of more than a dozen: If one or more nerves occupied each tube there would, of necessity, be between 600 and 37,600 in every square millimetre of human dentine, including the *radicular* as well as the coronal regions—a two-fold mistake on the part of nature, first, in that dentine would be rendered more receptive of sensory impulses than the exquisitely sensitive cornea or conjunctiva or nasal mucous membrane, and secondly, that it would contain a nervous mechanism wholly illogical, unrequired, and absolutely unlike that of any other part of the body. Calcified material is not innervated: bone contains no sensory nerves whatever. They would serve no useful purpose there or in dentine or enamel.

Another illustration may be given. It is a well-known clinical fact that the formation of carious cavities in teeth with living pulps may be absolutely unattended by pain or other subjective sensation on the part of individuals. The usual *prodromata* of nervous disturbances in the pulp associated with dental "caries" are frequently entirely wanting. If nerves were present in the hard dental tissues this could not possibly occur. The excavation of a cavity most commonly produces pain in consequence of pressure on the inelastic columns of intratubular material and the softened (decalcified) inter-tubular matrix of the dentine transmitted to the surface of the pulp. The character of the pain varies according to the depth of the excavation and the amount of pressure used by the operator in removing the carious parts. It is inconceivable that this would be the case if nerve fibrils traversed the tubes of the parts involved, for then the intensity of the pain would be similar throughout their whole length.

The concept of the innervation of enamel and dentine may be traced to German sources. One of the original exponents of the theory has long since modified it. It can only be briefly told how Morgenstern [4] supposed that myelinic nerves entered the dentine in *special* tubes (which apparently existed only in his own preparations) and passed into the enamel; how Römer [5], twenty years ago, believed that sensory nerve fibres pierced the fibrilloblasts and ran up into the dentine and enamel "splindles" in the interior of their processes, which is contrary to any anatomical relationships in other parts of the body; and how Dependorf [6] described nerve fibrils in the matrix as well as the tubes—an impossible condition. It is significant that, of the above, Römer [3], in his last publication, recedes from this position, considering, and with good reason, that what he had previously described as nerves were, as a matter of fact, artefacts. In describing the nervous system he writes:—

"The nerve fibres lose their medullary sheath, and end, for the most part, in and below the odontoblastic layer. . . . I did believe, in former years, that a part of the non-medullated nerves penetrated into the dentinal tubules. At present, however, I am doubtful about this conception."

Dentine Insensitive.

Regarding, therefore, the question of the so-called sensitivity and "hyper-sensitivity" of the dentine with the utmost impartiality and absence of preconceived ideas, and analysing it by the most direct methods and with the widest vision, I do not feel justified in accepting as correct the evidences so far presented. I have come to the conclusion that this tissue, being nerveless, cannot be, *per se*, sensitive, though capable of conducting or transmitting, by reason of its unique physical and anatomical characteristics, thermal, tactile, surface tension and electrical impulses to the pulp. I am convinced that this

admits of irrefutable proof, and that it is the one and only interpretation to be placed upon the exhibition of certain physical phenomena; and my teaching is, therefore, firmly based on this deduction. A closer and fuller analysis of the whole subject is, meanwhile, reserved for another occasion.

When the fibrilloblast has fulfilled its rôle of elaborating a fibril from its cytoplasm, it begins to undergo morphological and physiological changes, and from being a large, somewhat columnar cell, shrinks considerably in size and merely maintains sufficient vitality to prevent complete stenosis of the lumen of the dentinal tube. Adult fibrilloblasts are distinguished microscopically from young cells by their changed form.

Two further observations concerning the fibrils may be made. Some writers, particularly those of the German school, regard them as being hollow. Such are the vagaries of optical refraction and of microscopical technique that it is not difficult to demonstrate their apparent tubularity. But no animal cell affords a parallel. *A fortiori* it can be stated that these functionally insignificant and moderately useful cells do not differ from other connective tissue elements elsewhere, and that their processes are solid and not in any sense canalized.

The length of the unbranched fibril, enormously out of proportion to that of the cell from which it emanates, may in the coronal parts of the dentine perhaps measure one hundred and fifty times the length of the fibrilloblast—in histological calculations as twenty-five microns to five millimetres. This fact alone renders the fibrilloblast and its process quite distinct from any other cell in the human body except the neurone, and at once raises it to a position of far greater importance than that of merely forming dentine matrix, which has hitherto been the more-or-less generally accepted theory.

Hastate Cells.

With the exception of a frequently-recurring type hitherto undescribed, shaped like a spear head, to which I have given the name "hastate," the other cells of the pulp need not be mentioned.

(C) ITS CIRCULATORY SYSTEM.

The uncommon nature of the blood-vascular system is of importance when the main function of the pulp as a whole is considered. So minute are the vessels that they are correctly described as arterioles, venules and capillaries. In section, the first named are recognized by their small size and the thick *tunica adventitia* composed of connective tissue and elastic fibres, the *media* being reduced to delicate ribbons of circularly-disposed involuntary muscle fibres. In the pre-capillary arteriole there is no musculature.

The large, thin-walled venules are provided with an inconspicuous middle tunic which renders them less contractile than the arterioles. They retain their circular outlines and do not collapse, as is the case with most other tissues. They are destitute of valves like those of yellow and red marrow of bone, and copy on a small scale in this anatomical feature the *venæ cavæ*, cerebral, pulmonary, portal, hepatic, renal, facial and other veins. This valveless structure is a highly predisposing local factor in the production of nutritional diseases of the pulp, as obviously the vascular circulation is slowed down and there is a great tendency to the induction of venous or passive hyperæmia.

Conforming to the usual type, the capillaries consist of a single layer of endothelial cells. They are very abundant near the dentine, obviously to supply it effectively with lymph.

(D) ITS NERVOUS SYSTEM.

The myelinic (non-medullated) nerve fibres in modern man are cellulipetal. Constituting the peripheral axones of receptive afferent neurones, they are essentially the distal teleodendrites of the peripheral sensory neurones, entering the pulp at the apical foramen of the teeth in company with the blood-vessels and terminating somewhat similarly to the ordinary sensory nerves, not in special anatomical formations as described by Mummery [7], but in minute endings around the fibrilloblasts on the anatomical threshold of the pulp.

(E) ITS ANATOMICAL PECULIARITIES.

The extraordinary, interesting and important anatomical features of the human pulp may be briefly summarized as follows:—

- (1) Absence of a collateral arterial circulation, which tends to nullify any attempts at reparative action or healing of parts or individual regions of the tissue.
- (2) The great number, large size and valveless character of the venules, factors contributing to probabilities of regurgitation of blood and the production of venous stasis and hyperæmia—a very common condition, terminating more or less in thrombosis, degeneration, calcification, and inflammation.
- (3) Inexistence of a definite lymphatic system, which would otherwise assist in removing inflammatory exudates and waste products. Lymph spaces abound everywhere, but no organized system is satisfactorily demonstrable.
- (4) Absence of direct nervous control over its environment.
- (5) Its unique mural limitations, which thus favour injury through the accumulation of transudates and end products.
- (6) Its constant subjection to rapid and extreme changes of temperature.
- (7) The early completion of formation of the apical foramina of the dentine, thus shutting off nutritional sources and favouring local hæmostasis.

To the above there may be added two abnormal conditions which act as local predisposing causes of disease, viz., (1) the frequent presence of fillings in enamel and dentine which may be thermal conductors and affect the physical properties of the hard tissues; and (2) the constant presence of adventitious dentines which, slowly accumulating, thus diminish the cubic capacity of the pulp cavity.

(II)

PHYSIOLOGY.

Formative and Nutritional Functions of Pulp.

From a comprehensive study of the comparative anatomy of the dental pulp the conclusion must perforce be reached that its main, if not its sole, uses are: formative and nutritional.

In its early phase as the dentinal papilla it is concerned in building up the calcified material which ultimately nearly completely surrounds it. This is accomplished, similarly as in the case of bone, by small cells of an osteoblastic type, the activities of which are directed towards the abstraction of the necessary salts from the blood and deposition of the same around dentogenetic fibres, which form the scaffolding of the dentine. Once its main energies are concluded, it merely exists to provide a source of modified form of nutrition for the dentine—again from the contents of the blood-circulatory system.

Lymph exudes from the capillaries and pre-capillaries and passes into the dentinal tubes for the purpose of keeping the matrix moist, and not to vitalize it in the fullest meaning of the term. Dentine is not alive, nor is it wholly dead like enamel. It serves as an intermediate tissue between the dead enamel externally and the living pulp internally. It does not generate sensations but transmits through its tubes impulses of various kinds to the pulp. That is one of the reasons why it is tubular. It is essential for the strictest requirements of the animal economy that dentine should remain moist. For, subsequent to its drying out, it shrinks, and the enamel—an inorganic, dead, solid, inert, calcified secretion, unable to contract, incapable of being influenced by metabolism—becomes cracked and thereby impaired. The dentine of a pulpless tooth becomes slowly desiccated and the enamel frangible and fissured.

If the pulps of the lower vertebrates be specially examined the scanty distribution of myelinic nerves can be soon determined. A sensory equipment is not required to such a great extent as in man, for the teeth of the domesticated animals, as well as those living *in ferâ naturâ* are not and cannot be subjected to the same thermal, chemical, pathological and physical changes which obtain in the human oral cavity.

Domesticated Animals do not Experience Odontalgia.

Cats, dogs, pigs and other domesticated vertebrates do not suffer from odontalgia when the pulp happens to become exposed either by traumatism or "caries." Extensive cavities may develop, with no obvious symptom of discomfort or pain. The sensory susceptibilities of these pulps are very limited in range of action—if they exist at all. Vasomotor nerves distributed to the coats of the blood-vessels predominate, and maintain normally very definite and well-balanced trophic influences on the parts.

It would appear reasonable to postulate that the sensory apparatus in man is concerned with translating the various forms of stimulation just mentioned into the special sense of pain, the sympathetic system being only associated with the vasomotor mechanism of the unstriped muscle fibres in the walls of arteriole and venule. The sensory nerves do not possess any discriminative sensibility.

It may further be safely affirmed that this unusual, somewhat extensive supply is a sign of a process of evolution, in which a defensive machine has been developed and permanently established to meet the singular needs of modern human dental organs. For prehistoric man, as revealed by skiagrams of the molars of the Krapina and Mauer mandibles, possessed pulps of a relatively larger size than those of present day individuals. As his jaw in the course of several chiliads became smaller, the dimensions of the teeth diminished, and the pulp chambers greatly reduced, the sensory nervous supply in the meantime evidently undergoing concomitant increase in the development of its exquisite perfection of distribution and importance. The perception of pain—the latest of the senses to be evolved—was intensified at the expense of the cubic volume of the whole organ to meet the newer and more complex and delicate necessities of civilization.

The chief operations of the fully-formed pulp in the lower animals would seem to be restricted to the supplying of dentine with lymph, thus keeping it moist and nourished. The human pulp, while possessing a similar function, is, in addition, a sentient organ, its nerves in their immediate response to various kinds of stimulation acting as a defensive, monitory and protective

mechanism. Of these two functions, however, the nutritive factor is the more important, and the pain-producing and pain-conducting properties of considerably less account.

The sensitivity of the human pulp diminishes as age comes on, not because of an increase in density or dimensions of the dentine, but because of intrinsic retrogressive metamorphoses of an unalterable character in itself. The pulps of the deciduous teeth exhibit old age changes exactly on similar lines with those of the permanent dentition.

(III)

PATHOLOGY.

The welfare of the enamel and dentine depends on the life of the pulp. After it has been destroyed either intentionally or by morbid conditions, the tooth becomes a foreign body and is liable, especially if the alveolo-dental periosteum is implicated by disease, to be exfoliated as a menace to the well-being of the body generally.

Threatened exogenetic disturbances will be resisted by the pulp in the mouth of a healthy person, and attempts will be made on its part, even in the cases of those suffering from general malnutrition and long-continued fevers, to attain the same end.

It may be stated that any acquired changes of a pathological nature that may occur in the hard tissues will act upon the soft tissues, which will respond more or less speedily, either by a definite demonstration of adding a thicker wall to the dentine at the point of attack, or by becoming hyperæmic or inflamed, or both. If the injury is slight, new protective material is laid down; if severe, the unequal contest ends in active hyperæmia and localized inflammation on the one hand, or in venous hyperæmia and thrombosis and degeneration on the other.

Reactions due to Non-operative Injuries.

Resistance is well exemplified in the attempts of this organ to counteract the effects of the centripetal lesions of attrition, erosion, abrasion, true caries, and the bacterial affection of enamel and dentine known as "dental caries."

If teeth which have undergone attrition be examined in longitudinal section it is evident that a large amount of secondary dentine has been deposited by the cells of the pulp opposite the area of attack, viz., its coronal and cornual regions. By this means the total bulk of the pulp is considerably diminished. In erosion and abrasion, adventitious dentines, the results of reaction to injury, are invariably present.

With regard to "dental caries" the pathological phenomena are consistently more complex, for, shortly after its inception, the pulp, as usual, attempts to defend itself by precipitating on the threshold of its threatened surface one or more varieties of adventitious dentine which I have described in detail elsewhere [8]. Thus the areolar, cellular, fibrillar, hyaline, laminar, and tubular types may be observed, the particular kind being probably dependent upon the rate of progress of the infection and the rapidity of response on the part of the pulp cells. If acute, the first named are most frequently noticed, if slow and chronic the latter varieties.

In spite of the attempts of the pulp to heal the lesion its energies are usually unavailing, for the new adventitious dentine often becomes likewise

infected and a hyperæmia of the soft subjacent organ thereby induced. (See figs. 1-4. Vertical sections of dental pulp *in situ*, which exhibit four phases of the process of infection of the adventitious dentine in one of the cornua of the pulp cavity, the usual condition associated with deep-seated dental "caries.")

Adventitious Dentine of Pathological Origin.

It must be noted that while it invariably reacts in the best possible manner to the above-mentioned injuries as an auto-defensive measure, its object is frequently defeated. For the mechanical addition of layers of

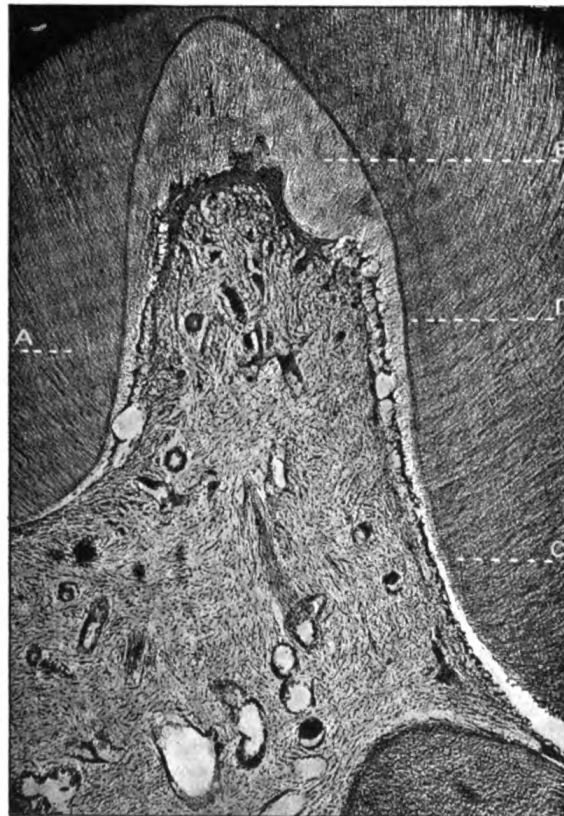


FIG. 1.—A, primary dentine; B, adventitious dentine slightly infected; C, fibroid fibrilloblasts; D, junction of primary and adventitious dentines.

adventitious dentine, or secondary dentine in the case of attrition (the last-named formation being physiological in contradistinction to being pathological in origin) means (1) the gradual reduction of the cubic capacity of the pulp cavity; (2) a restriction in the complete and perfect use of the physiological activities of its containing organ; (3) the limitation of its powers of healing; (4) an increase in its difficulties of overcoming the advancing destruction; and it ends in the lowering of the vitality of the issue to such an extent as to bring about a general predisposition to degeneration and inflammation. Also, all these changes are profoundly affected by the more or less complete centrifugal stenosis of the apical canal, which is of so common an occurrence

through hyperplastic cementum becoming deposited about its orifice. In addition it means a lessening of the trophic influences which it brings to bear primarily upon dentine and indirectly upon enamel. In a word, the fantastic, universal, well-intentioned exertions made by the pulp to repair damage are practically always doomed to failure on account of the anatomical peculiarities of its own surroundings in particular, and its immediate surroundings in general. Singular to relate, it is killed by the defensive measures it adopts to combat the disasters that may befall it!

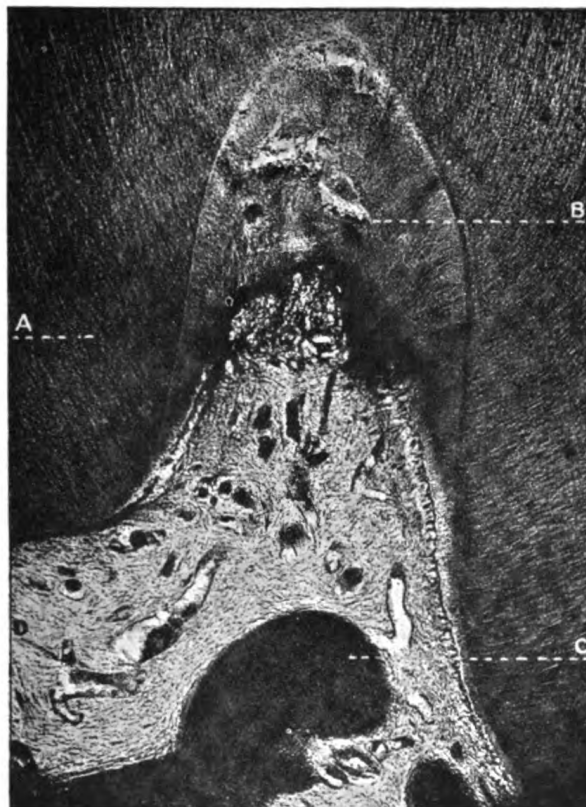


FIG. 2.—A, primary dentine; B, infected adventitious dentine; C, pulp nodule. Pulp is fibroid and hyperæmic.

Diseases of the Pulp.

It is not surprising that the pulp is so frequently a victim of disease when the numerous causes are taken into consideration. They may be classified as : (A) predisposing, and (B) exciting causes.

(A) Predisposing causes may be divided into : (1) general and (2) local. The general predisposing causes may shortly be stated as follows : (1) Sex, (2) age ; (3) marasmus (general nutritional failure), and (4) protracted fevers.

Of these the first has probably little influence on causation. But age appears to determine pathological conditions very largely. In general terms one may say that up to the age of 20 to 25 the pulp is in a normal state.

68 Hopewell-Smith: *Some Observations on the Dental Pulp*

After the age of 30 it is by no means always normal, being dependent upon the partial or complete stenosis of the apical canal and foramen. With regard to trophic neuroses and fevers the pulp suffers in the same way as other parts of the body. That the general welfare and health of the teeth is dependent upon the general health of the body is an incontrovertible axiom.

The local predisposing causes are found in the anatomical peculiarities already reviewed.

(B) The exciting causes may be (1) general, due to (a) vascular disturbances such as increased or diminished blood-pressure, or chemical constituents

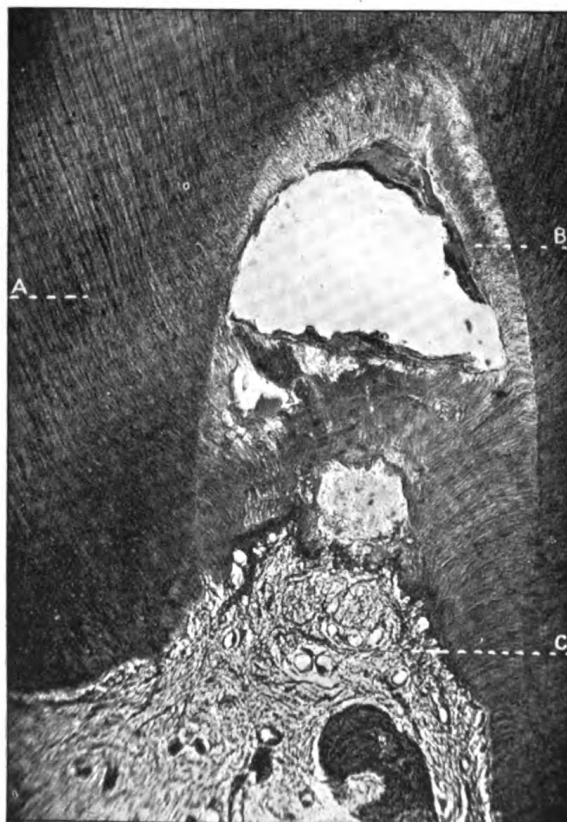


FIG. 3.—A, primary dentine; B, infected and broken down adventitious dentine; C, fibroid pulp.

of the blood; (b) nervous disturbances, ascribed to affections of the sensory and sympathetic nerves, and (2) local, bacterial invasion of the dentine.

Reactions due to Diseases of the Pulp.

Arterial and venous hyperæmia, followed by fibrous and calcareous degenerations as well as inflammation, are the direct reactions of the pulp to pathological conditions to which it is subjected during the course of "dental caries." In other words, infection of the dentine in its later stages produces the vascular changes just named. Of these, venous or passive hyperæmia is more usual than active or arterial hyperæmia for the reasons already recounted.

The histological phenomena observed here are identical with those seen in similar circumstances in all soft tissues. They are, however, modified by the rigid enclosure of the walls of the pulp cavity, by the lack of collateral circulation and the peculiarities of the venous system. It is, therefore, quite a common experience to find thrombosis going on in such a pulp. Indeed, in many, perhaps even the majority of the pulps, thrombi readily form in the veins, small hæmorrhages take place and the integrity of the parts becomes completely undermined and destroyed, either by the production of a fibroid or calcareous degeneration, generally with the formation of pulp nodules, or, if the injury is very great, infection, gangrene and death.



FIG. 4.—A, primary dentine; B, carious primary dentine; C, large cavity in adventitious dentine; D, pulp nodule; E, slight inflammation of pulp; F, inflamed nerves.

Fibroid Degeneration Extremely Common in Adult Life.

Fibroid degeneration is very general, not only in senile conditions and pulps affected by "caries," but in non-carious teeth of adult and young alike. Without entering into detail—there is no time—it may be stated that recent original observations, regarding this matter especially, tend to show that the change originates in the arterioles. The muscle fibres of the *tunica media* of their coats become more or less atonic; the velocity of the blood-stream is lowered; the venous hæmostatic pressure increased, with the result that the cellular elements quickly suffer. Their nuclei shrink, the cytoplasm becomes atrophied. The fibrilloblasts lose their usual outlines, become "sheaved" together and reduced to attenuated, elongated bodies. The venules become

larger, their walls thinner, and thrombosis and hæmorrhages occur. The sequence of events would be that the arterioles are first affected, then the pulp cells, then the capillaries, then the venules, and finally the nerve bundles.

Fibroid degeneration is often accompanied by the formation of one or more varieties of new adventitious dentine. This is deposited in the first instance at the upper portions of the cornua, is derived in a parallel manner to the old primary dentine from small lime-depositing cells in a menstruum around connective tissue fibres, and is exceedingly liable to infection by bacteria from the primary dentine.

Pulp Nodules Extremely Common.

Pulp nodules are said to be caused by calcification taking place around a collection of dead cells within the pulp. This is probably not so. It has never been explained why a few cells, say in the equatorial segment, should die. If, however, some should die and degenerate, they would at once be attacked by phagocytes that have migrated from the neighbouring bloodstream, acting as defensive agents. Cytolysis would immediately ensue and the source of the irritant be removed. A similar erroneous claim has been made with regard to dead bacteria.

Pulp nodules are extremely common, not only in pulps of the permanent dentition, but also of the deciduous series, in apparently sound teeth, as well as those affected by so-called dental "caries." They are most likely due to venous hyperæmia with thrombosis, when great physical and chemical changes take place in the walls of the vessels. Arising probably in a similar manner to phleboliths—the local effects of intravascular coagulation—but incomparable with tonsilloliths—the results of infiltration of mucous secretions, these concretions originate in the deposition of calcium salts by small round cells in and around the connective tissue stroma of the pulp. Evidence is not wanting that the process follows that by which adventitious dentine is constructed, and that it closely resembles the calcification of the primary dentine.

Inflammation of Pulp Relatively Rare.

Odontalgia is of three types—simple or local, referred or obscure. The first is a localized obvious symptom of physical, vascular and nervous disturbances in the hard and soft tissues, including the alveolo-dental periosteum. Referred odontalgia is a symptom of a distant affection which expresses itself as a non-localized irritation of the peripheral parts of the trigeminal nerve and its connexions; while obscure odontalgia, on the other hand, associated with non-carious lesions, is a symptom of an intrinsic or extrinsic condition in which the vascular and nervous supplies of the pulp are involved, and the causes of which are not evident on mere visual examination.

Obscure odontalgia is believed to be due to: (A) Intrinsic disturbances: (1) Increased or diminished intrapulpal blood-pressure, as in influenza, malaria, and during puberty, menstruation and pregnancy, hysteria, &c.; (2) variations in the chemical constituents of the blood as in a drop of its calcium content; (3) formation and growth of pulp nodules. (B) Extrinsic disturbances: (1) General neurasthenia and debility; (2) intra-oral electrical impulses; (3) reflexes from the glossopharyngeal and possibly the chorda-tympani nerves; (4) osteitis of the alveolar process, e.g., maxillary tuberosity; and (5) lesions of the fifth nerve.

In none of these is acute or chronic inflammation the cause, and it is

probable that mere inflammation is rarer than is generally supposed to be the case.

CONCLUSION.

To sum up: Taking into consideration the structure and specific uses of (a) enamel—solid, adamantine, a mechanical agent supremely adapted for the trituration and comminution of food; of (b) dentine, tubular, hard, serving mainly as a support for the all important enamel, the utility and economic value of the human dental pulp may now be comprehended and estimated probably with much scientific accuracy.

Reviewing its histology, functions, and commonest morbid conditions, its frequent tendency to undergo retrogressive metamorphoses, fibroid or calcific, and to maintain its vitality at a low ebb, it would seem that its removal as a surgical operation is of but little moment so far as the ultimate efficiency of each unit of the dental apparatus is concerned. A pulpless tooth is nearly as useful a member of the dental series as a living tooth, but not quite, because of the physical changes which subsequently must occur in the hard investing tissues when the central organ is destroyed. It would, however, appear to be desirable that the integrity of the pulp should be retained unimpaired and efficient; but if this is impossible, then removal from the containing cavity of a tissue which is seldom capable of adjusting itself successfully to functional and organic disturbances, and of regaining and maintaining its former usefulness and intrinsic worth to the tooth, mouth, and body generally, may be regarded not as a serious but perhaps at times even as an advantageous and successful expedient.

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[The paper was fully illustrated by means of microscope slides, photographs, and a fine series of original coloured lantern slides.]

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1-20

1-21

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Section of Odontology.

President—Mr. W. R. ACKLAND, M.R.C.S., L.D.S.Eng., M.D.S.Brist.

A Case of Unerupted Incisors and Canines in a Male, aged 59.

By GERALD HARBOROW.

THIS patient came to me at the Metropolitan Hospital in July last, with a swelling in the incisor region of the mandible.

Four days previous to his visit he had had great pain followed by swelling, which discharged later ; after this he had little pain and only slight discharge.

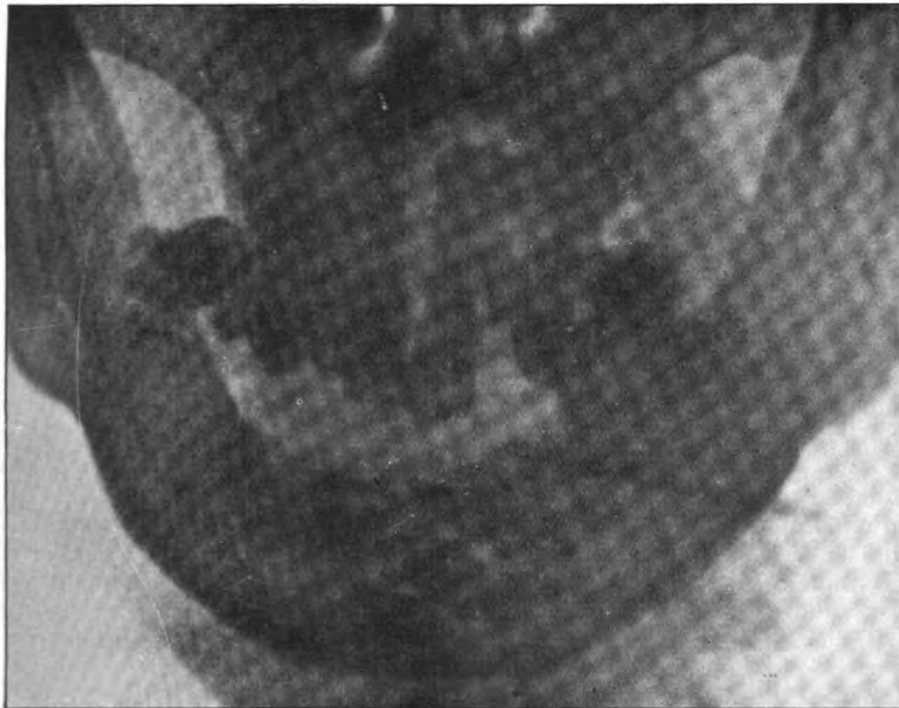


FIG. 1.

On examination there was marked oral sepsis but the only teeth seen in the mandible were $\frac{8}{5}$ and $\frac{7}{5}$. There was not much swelling now, but a good deal of bone thickening. There was a foul sinus in the sulcus to the right of the middle line and this was large enough to simulate a tooth socket. On passing a probe, bare bone, and what felt like a tooth, could be made out. From exploration it could be clearly diagnosed that the condition was more than an

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[April 23, 1923.]

ordinary alveolar abscess. The patient was sent for X-ray examination and a mouth wash was ordered.

The X-ray photograph showed unerupted teeth, four being plainly visible, but which teeth they were was uncertain (fig. 1).

The patient could not supply any definite history; he was a healthy man and had had no illness and no trouble or pain with his teeth. He had been without front teeth for at least twelve years. He remembers having had little black stumps level with the gum, but these fell out, they were not extracted.

His work necessitates his using a stock and bit, which he rests against his chin in working it. This has caused some thickening of the soft tissues.

The operation, which was carried out under chloroform and ether anæsthesia, consisted in making an incision to the full extent of the swelling and cutting away the outer wall. This did not give a very satisfactory field for operation, but the teeth could be felt quite easily. They were loosened by the straight



FIG. 2.

elevator and removed with Reed's forceps. Their removal did not present any great difficulty. They consisted of the two canines and lateral incisors. The patient did well and at the end of three weeks I examined him again and was surprised to find that a tooth could be felt in the cavity. Further X-ray examination showed two teeth, one almost horizontal, the other leaning over to the left side. The patient was again anæsthetized with chloroform and ether and the teeth were removed by the same method used for the others; but these caused a considerable amount of difficulty, due no doubt to their being implanted more deeply in the bone and to their position. On examination of the teeth it will be noticed that the left central incisor has a curved root and is undoubtedly the one which was horizontal, the right leaning towards the left side; each tooth shows some rarefaction at the root; the canines only show the thickened cement, which is usual in cases of delayed eruption (fig. 2).

The Effect of Diet on the Resistance of Teeth to Caries.

By MAY MELLANBY.

(Pharmacological Department—University of Sheffield.)

IN the March number of the *Dental Cosmos* the following statement occurs in the editorial article:—

“Dr. Black's researches led him finally to the belief that caries of the teeth is a factor of the environment of the teeth, and not of the structural peculiarities of the teeth themselves; that structural features may influence

[March 26, 1923.]

the rate of decay but have no bearing upon the liability of the teeth to decay; that any tooth will decay in a caries-producing environment and that no tooth will decay in a non-caries environment. These conclusions are accepted as sound by all who have intelligently weighed the evidence upon which they are founded, and no one thus far has brought forward evidence that successfully rejects them."

I hope to supply you with evidence which refutes at least some of these conclusions, and shows clearly, not only that the structure of the teeth plays an important part in regulating the subsequent resistance of such teeth to caries, but also that those dietetic conditions which determine the perfect formation of teeth, are responsible to some extent for their defence against bacteria and other harmful influences after eruption.

The new facts I wish to bring to your notice concern the question of the resistance of teeth to caries, after eruption. This is usually assumed, as in the paragraph quoted, to be a question of the condition of the mouth and the immediate environment of the tooth, but a study of the secondary or adventitious dentine in human beings and experimental animals indicates that factors acting indirectly through the pulp and other tissues are of dominant importance in regulating this resistance. Before dealing with this evidence and in order to give sequence to my narrative I will briefly remind you of the main facts brought out in my earlier published work [1], which has differed from most other dental investigations mainly in two respects:—

(1) I have attempted to determine the influence of dietetic factors after digestion on the teeth and jaws. This influence of food seems to have attracted comparatively little attention in earlier work although the results obtained prove it to be of prime importance. I have disregarded, for the time being, the chemical and physical action of food in the mouth and its relation to bacterial decomposition.

(2) Most of the work was carried out in the first place on animals. In no other way would it have been possible to determine the part played by each factor of diet and environment in turn, and by processes of direct observation and elimination to narrow down the issue to a few of the more important influences. I feel sure that the animal experimental method will prove to be of great importance in dental investigations.

The experiments on puppies have shown that the structure of the teeth and their arrangement in the jaws are closely controlled by the quality of the diet eaten during their development. The diet must of course contain certain minimal amounts of calcium and phosphorus, but the retention of these elements in the body and their deposition in the growing teeth depends on other factors, the most important of which is a vitamin somewhat similar in distribution and properties to vitamin A. For the purpose of this paper I shall refer to the calcifying vitamin as vitamin A, although, as pointed out in previous papers [1], the two substances have not been definitely shown to be identical. The quantity of this vitamin necessary for the production of perfect teeth and jaws is variable, because its activity is influenced by the amount and type of other substances in the diet, by the rate of growth relative to the food eaten, by the exercise taken, by the earlier nutrition of the animal both prenatal and postnatal, by the sunlight experienced during the experimental period and probably by other factors not yet determined. Thus it is possible by feeding puppies, during the time of development of the permanent teeth, on diets sufficient in amount, but defective in quality—and, more especially, having a deficiency of vitamin A—to produce one or more of the following defects:—

- (1) Thick and poorly calcified jaw bone and alveolar processes.
- (2) Irregularity in the arrangement of the teeth, generally most noticeable in the lower incisors.
- (3) Delay in, and retardation of, the eruption of the permanent teeth.
- (4) Interference with the calcification of the enamel, which is often either deficient in amount or defectively formed.
- (5) Interference with the calcification of the dentine, which may be either small in amount, or poorly calcified containing varying numbers of interglobular spaces.
- (6) The tissues at the gingival margin and the periodontal membrane are often abnormally developed.

It may be added that defectively formed deciduous teeth can also be produced in puppies by feeding bitches during pregnancy and lactation on diets which are abundant from the point of view of energy and also protein, fat, carbohydrate and salt-content, but poor in quality, and especially in vitamin A.

Whereas it is comparatively easy to produce the above-mentioned abnormalities by defective diet it is also easy to produce in other puppies of the same litter perfectly formed teeth and jaws by feeding them on similar diets but containing abundant vitamin A [1]. Another dietetic factor which has been shown to influence the teeth at least of guinea-pigs, is the antiscorbutic vitamin. Zilva and Wells found that the deprivation of this substance was followed by profound changes in the pulp and dentine of guinea-pigs' teeth [2].

There is some hope that in the near future it will be possible to make direct observations not only on the relation of diet to the development of teeth but also to the production of dental caries. American workers have repeated and extended some of my early experiments and claim to have produced caries-like lesions in rats' teeth by feeding these animals on diets deficient in vitamin A, calcium, phosphorus, and other substances. If this hope is justified a great advance will have been made in the study of dental defects, for, although the experiments on the production of artificial caries *in vitro* are of interest, observations of this nature do not appear to touch the real problem. This type of work has resulted in almost universal concentration of dental authorities on carbohydrates, and while I am prepared to admit that these substances probably play a part in the development of dental caries, I have failed to produce this condition in dogs even after feeding them for over two years on a pappy diet containing on the average two ounces of glucose a day; nor has Howe [3] been successful in producing caries in guinea-pigs on diets containing different carbohydrates.

It seems probable that the influence of carbohydrates on caries is not only due to changes in the mouth but also to their action after digestion and absorption. I have found that, under some conditions, foodstuffs, rich in carbohydrates, such as cereals, antagonize the calcification of teeth. Their action on caries may be closely related to this.

In order to get at closer grips with the problem of dental caries I have extended my investigations to children. In a recent publication [1c] I pointed out that, contrary to the usual teaching, a large number of children's deciduous teeth are imperfect in structure. Out of 302 examined microscopically 84.5 per cent. were badly formed. This percentage is to be contrasted with the 3 per cent. described as hypoplastic by the dental surgeons who supplied the cases. Thus it appears that naked-eye examination is, according to standards used at present, of limited value in determining the structure of teeth. In the

following table the result of the histological examination of these teeth as regards both structure and caries are set out:—

TABLE I.

Type	No. examined	NORMAL STRUCTURE			HYPOPLASTIC STRUCTURE		
		No caries	Caries		No caries	Caries	
Incisors ...	47	34	5	...	—	...	8
Canines ...	29	1	—	...	12	...	16
First molars ...	88	1	5	...	1	...	81
Second molars...	138	—	1	...	—	...	137
	302	36	11		13		242

There is obviously a close relationship between structure and caries, since 83·8 per cent. of the teeth referred to were carious and 84·5 were hypoplastic, but more detailed examination revealed that the correlation is not quite so close as these figures imply, for eleven teeth were well formed and yet carious, while thirteen were free from caries although defectively formed. That is to say, of the 302 teeth examined twenty-four (7·8 per cent.) were quite out of harmony with the hypothesis that well formed teeth are more resistant to caries than those badly formed, whereas 278 (92·2 per cent.) agreed with this hypothesis. Most of the exceptions were found in incisors and canines. Thus, out of twenty-nine canines twelve were defectively formed and yet free from caries, and five out of forty-seven incisors showed some caries, in spite of being normal in structure. In the case of the molars the correlation between structure and caries was almost perfect, for only two out of 226 molars were quite free from caries and of these two the first was well formed and the second had nearly normal enamel and dentine.

In trying to find a reason for the above-mentioned twenty-four exceptions I was impressed by the varying types of secondary or adventitious dentine found in many of the sections (see figs.). Throughout life the tooth has the power of reacting to external stimuli whether these be of a physical or of a chemical nature. For instance, even as the result of attrition of the enamel, the pulp can apparently be stimulated to bring about alterations in the primary dentine and also to form secondary dentine (see fig. 1). Caries may cause a similar reaction.

Since the structure of the primary dentine is regulated by the quality of the nutriment supplied during the actual growth and calcification of the tooth, it seemed most probable that the same control might also operate in the production of the secondary dentine. If this were the case, it would afford an opportunity of testing the truth of the suggestion I had previously put forward to explain the exceptions to the simple relationship between dental structure and caries. I have elsewhere [1c] suggested that:—

(1) If the nutrition of the child were "good" in early life but "defective" after the eruption of the teeth, then the teeth would be well formed and therefore, to some extent, resistant to caries, but less resistant than they would be if the diet remained "good" throughout the whole period, for the resisting power of the pulp, surrounding tissues, and saliva would be depressed.

(2) If the nutrition were "defective" in early life but "good" afterwards, then the imperfectly formed teeth would have their resistance to invasion increased by the improved diet after eruption, but would again be more liable to caries than when the diet was "good" throughout.

If the secondary dentine were badly formed under the first of the above

78 Mellanby: *Effect of Diet on Resistance of Teeth to Caries*

conditions and well formed under the second, then substantial support would be given to the hypothesis that, apart from original structure, diet after absorption plays an important part in regulating the resistance of teeth to caries. I shall refer to this as the subsidiary hypothesis, the main hypothesis being that well formed teeth are more resistant to caries and vice versa.

I shall now deal briefly with some evidence obtained in experimental animals which tends to show that this subsidiary hypothesis is valid. Up to the present time I have examined histologically the deciduous teeth of about 100 puppies and found secondary dentine in only thirteen cases. The relatively few cases in which secondary dentine is present in these deciduous teeth can be easily understood, for the two main stimuli, attrition and caries, responsible for its production in the teeth of man, are usually lacking in the puppy experiments. The softness of the diet, no doubt, accounts for the small amount of attrition in the puppies' teeth. I have begun experiments to investigate this point further, and hope by artificial means to induce more frequently the development of secondary dentine.

Microscopic examination showed the structure in these cases to be as follows:—

TABLE II.—*Secondary Dentine in the Teeth of Experimental Animals.*

						Well formed		Badly formed
"Good" diet	7	...	2
"Defective" diet	1	...	8

On the whole it would appear that the structure of the secondary dentine is related to the type of diet eaten during the period of its formation (photomicrographs illustrative of these facts can be seen in figs. 2 and 3). The evidence on this point is meagre, and the problem demands further attention.

Table III gives a classification of the secondary dentine in the human deciduous teeth previously examined for general structure. As explained elsewhere, these teeth were first examined microscopically, and then ground sections were made by Weil's process.

TABLE III.—*Type of Secondary Dentine in the 302 Human Deciduous Teeth.*

<i>Secondary dentine</i>			GOOD PRIMARY DENTINE AND ENAMEL				BADLY-FORMED PRIMARY DENTINE AND ENAMEL			
			Caries (11)		No caries (36)		Caries (242)		No caries (13)	
Badly formed	9	...	8	...	130	...	2	
Well formed	0	...	23	...	15	...	9	
None	2	...	5	...	97	...	2	

The exceptions to the general rule that structure and caries are closely related consist of eleven teeth which are carious and yet are well formed (first column), and thirteen teeth without caries although badly formed (fourth column). In the first group, however, eight out of the eleven have badly formed secondary dentine (fig. 5), in one case the pulp is replaced by cement, while the remaining two show no evidence of reaction. In the other group of exceptions, nine out of the thirteen have well formed secondary dentine (fig. 4). The majority of the 302 teeth examined were carious, and had defectively formed primary and secondary dentine (fig. 7). Thus it would appear that badly formed secondary dentine is an indication of lowered resistance to caries even when the enamel and primary dentine are good, and well formed secondary

dentine is an indication of increased resistance to caries even when the enamel and primary dentine are badly formed. Thus twenty out of the twenty-four exceptions to the general rule fall into line with the subsidiary hypothesis. A definite proof of this subsidiary hypothesis, in support of which I have supplied some evidence, would be of great importance, for it would suggest a means different from any previously suspected of altering the resistance of erupted teeth to harmful influences.

Further evidence which supports the hypothesis is seen in cases of teeth where caries has been arrested. This process is a reaction controlled by the activity of the pulp, and only takes place when this tissue is alive. The extensive translucent zone in the primary dentine and the large amount of secondary dentine often found in arrested caries, are evidences of the potent reaction of the tooth, and are strongly suggestive of the unity of the problem of arrested caries with that described above. It appears, in fact, that the mechanism of dental defence controlled from the pulp and the inside of the tooth is of great importance, and demands consideration in any study of the aetiology of dental caries.

I have attempted to show that the reaction on the part of the tooth, both to caries and attrition, may be strong or weak, as evidenced by the amount and condition of the secondary dentine produced during the reaction. Thus if the secondary dentine is well formed, the damage produced by caries is more likely to be slow or arrested than when it is poorly formed. I have provided statistical evidence which suggests that this holds in the case of the deciduous teeth of children. The few results so far obtained in regard to the relation of diet to the structure of secondary dentine in animals indicate that this relationship is similar to that already shown to exist between diet and the structure of enamel and primary dentine.

If this should ultimately prove to be the case it will be evident that not only is the structure of teeth controlled by the diet during their development, but also that their subsequent resistance to caries and other harmful influences is dependent on the food, and more especially on certain substances supplied to the body in some foods acting on the teeth by way of the blood-stream, through the pulp, and possibly also in other ways. In other words, those dietetic conditions which result in the formation of perfect teeth, regularly arranged in well-grown jaws, will also assist in the defence, even of badly formed teeth, against noxious agencies.

SUMMARY.

There is evidence that those dietetic conditions which, as I have shown in earlier publications [1], control the formation of good enamel and primary dentine, also confer upon teeth after eruption the power to resist bacterial invasion and other destructive influences for :—

(1) The experimental evidence at present existing, although small in amount, suggests that the pulp of erupted teeth reacts to destructive stimuli, so as to produce well formed secondary dentine when the diet is "good," and either does not react at all or only with the production of imperfectly formed dentine when the diet is "defective."

(2) Among the children's deciduous teeth examined, those which had resisted caries, in spite of defective structure, show in general well formed secondary dentine produced as the result of attrition (fig. 4), whereas those which are carious but of normal structure have usually badly formed secondary dentine (fig. 5).

(3) Diets which bring about the development of normal teeth in puppies are just those which enable the animal to resist bacterial infection. This point has been mentioned in earlier publications.

Of course I would be the first to admit that what I have said to-night is only part of the story, and that much more information is needed before dental caries can be really understood. On the other hand, the point of view as illustrated by the experimental results described is, I think, important, and will certainly lead to much further information concerning the physiology and pathology of teeth and the related tissues.

The expenses of this investigation were defrayed by a grant from the Medical Research Council, to whom my thanks are due.

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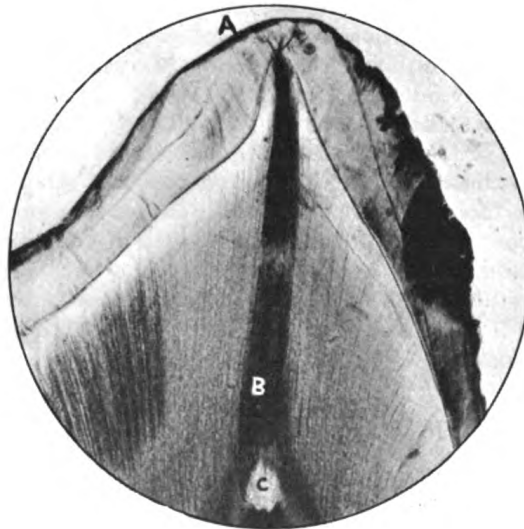


FIG. 1.

FIG. 1.—Human deciduous molar. Slight attrition of enamel at A, resulting in changes in primary dentine B, and the production of secondary dentine at C.

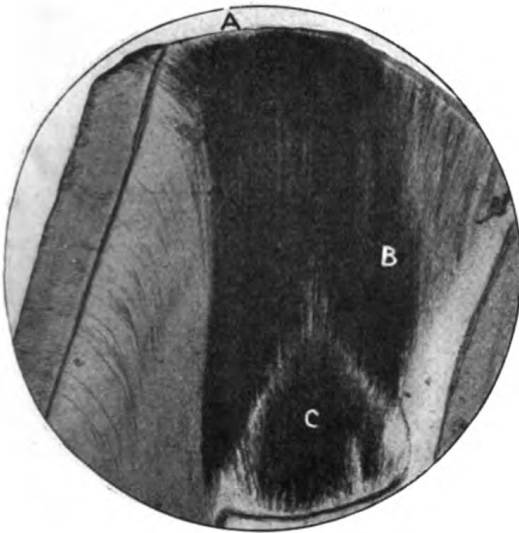


FIG. 2.



FIG. 3.

FIG. 2.—Deciduous tooth of puppy showing a large amount of attrition of enamel and dentine A, resulting in changes in the primary dentine B, and production of secondary dentine C. Secondary dentine well formed as result of a "good" diet after eruption and during time of attrition (cod-liver oil in diet).

FIG. 3.—Deciduous tooth of puppy showing secondary dentine C, associated with attrition (not indicated in photomicrograph). Secondary dentine badly formed and containing many interglobular spaces as result of "defective" diet after eruption (olive oil in diet).

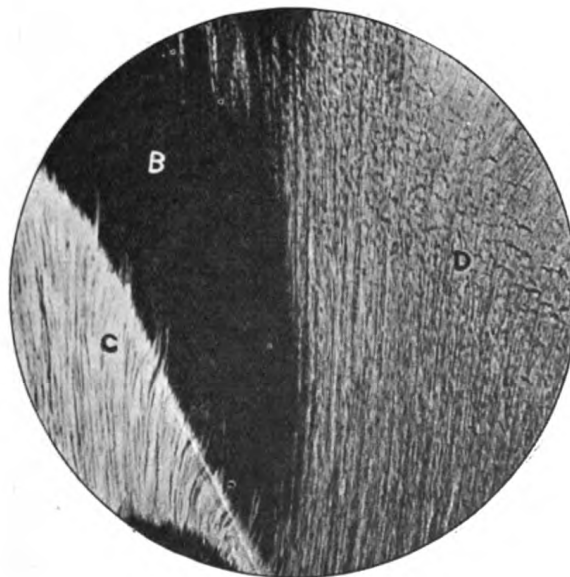


FIG. 4.

FIG. 4.—Human deciduous canine containing well formed secondary dentine C. There is no caries although the enamel and primary dentine D are imperfect.

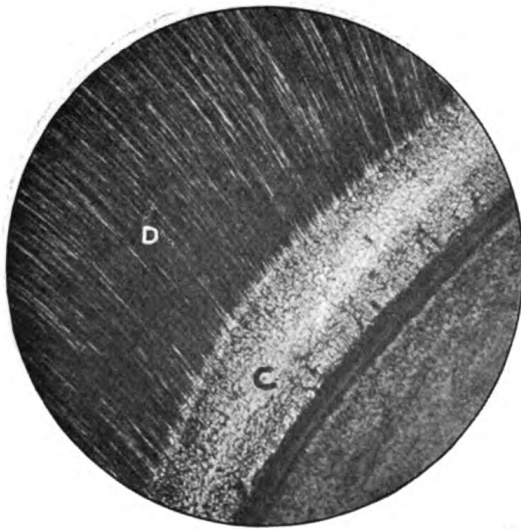


FIG. 5.

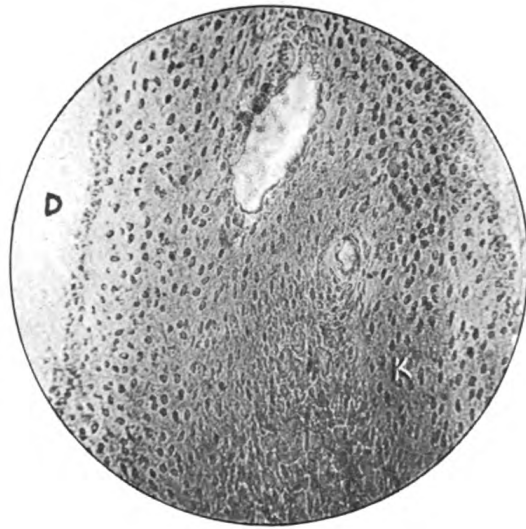


FIG. 6.

FIG. 5.—Human deciduous incisor showing badly formed secondary dentine C, with many interglobular spaces. The tooth is carious although the enamel and primary dentine D are well formed.

FIG. 6.—Human deciduous incisor (Mr. Law's case) showing pulp filled with cement K. The tooth is carious although the enamel and primary dentine are well formed.

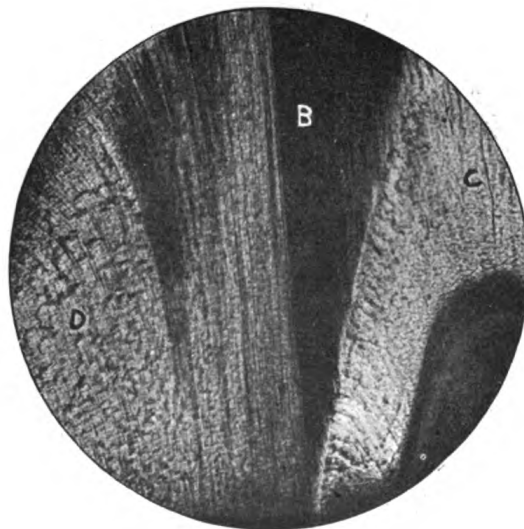


FIG. 7.

FIG. 7.—Human deciduous molar showing badly formed primary dentine D, and badly formed secondary dentine C. Tooth carious.

PROCEEDINGS
OF THE
ROYAL SOCIETY OF MEDICINE

EDITED BY
SIR JOHN Y. W. MACALISTER
UNDER THE DIRECTION OF
THE EDITORIAL COMMITTEE

VOLUME THE SIXTEENTH

SESSION 1922-23

SECTION OF OPHTHALMOLOGY



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1923

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SECTION OF OPHTHALMOLOGY.

CONTENTS.

October 13, 1922.

	PAGE
RAYNER BATTEN, M.D. Calcareous Degeneration of the Eye, with Deposits on the Iris ...	1
F. A. WILLIAMSON-NOBLE, F.R.C.S. A Plane Glass Retinoscope ...	1
A. L. WHITEHEAD, M.B. (President). Some Aspects of Ocular Tuberculosis ...	2
N. BISHOP HARMAN, F.R.C.S. Standards of Vision for Scholars and Teachers in Council Schools (Abstract) ...	7

November 10, 1922.

HUMPHREY NEAME, F.R.C.S. A Case of Retinitis Circinata ...	11
MONTAGUE L. HINE, M.D. Case of Ectopia Lentis (both Eyes)...	12
LESLIE PATON, F.R.C.S. (1) Hæmangioma of Orbit ... (2) Case of Recurrent Detached Retina after Seventeen Years' Reposition	18 14
R. AFFLECK GREEVES, F.R.C.S. Case of Bilateral Proptosis, with Limitation of Movement in One Eye ...	15
J. F. CUNNINGHAM, F.R.C.S. Tumour of Right Upper Lid (Angioma) ...	15
M. W. B. OLIVER, M.B. Plastic Operation for Contracted Sockets ...	15
F. A. JULER, F.R.C.S. Case of Retinal Degeneration, with Mental Deficiency ...	16

January 12, 1923.

J. A. VALENTINE, M.D. Night Blindness: Retinitis Pigmentosa sine Pigmento ...	17
--	----

A. H. LEVY, M.D.	PAGE
Case of Amaurotic Family Idiocy	17
MONTAGUE L. HINE, M.D.	
Two Cases of Early Familial Maculo-cerebral Degeneration	18
PERCY BARDSLEY, M.B.	
Case for Diagnosis (? Polycythæmia Rubra)	19
R. LINDSAY REA, M.D., F.R.C.S.	
Case of Hole in the Hyaloid	20
T. HARRISON BUTLER, M.D.	
Some Unusual Results of Operations for Cataract	21
CHARLES KILLICK, M.D.	
The Treatment of Conical Cornea (Abstract)	24

February 9, 1923.

LESLIE PATON, F.R.C.S.	
Optic Atrophy after Herpes Ophthalmicus	27
ROSA FORD, M.B.	
Intracranial Tumour causing Quadrantic Hemiopia	30
M. S. MAYOU, F.R.C.S.	
Case of Subhyaloid Hæmorrhage in a Girl	31
A. C. HUDSON, F.R.C.S.	
Two Cases of Primary Band-shaped Opacity of both Corneæ	31
F. A. WILLIAMSON-NOBLE, F.R.C.S.	
Atrophic Patches at the Macula Tuberculous; ? Cyst	32

March 9, 1923.

HUMPHREY NEAME, F.R.C.S.	
Tumours of Optic Nerve	34
F. A. WILLIAMSON-NOBLE, F.R.C.S.	
Endothelioma of the Orbit (Abstract)	35

June 8, 1923.

H. M. JOSEPH, M.C., M.B.	
Case of Progressive Macular Changes associated with Tremors	39
MONTAGUE L. HINE, M.D.	
Familial Nodular and Reticular Keratitis	43
Shown by IDA C. MANN, M.B., B.S.	
Some Suggestions on the Embryology of Congenital Crescents	45
PHILIP DOYNE, F.R.C.S.	
The Tournay Reaction (Abstract)	47

SECTIONS OF MEDICINE AND OPHTHALMOLOGY.

(JOINT MEETING.)

November 28, 1922.

DISCUSSION ON "THE SIGNIFICANCE OF THE VASCULAR AND OTHER CHANGES IN THE RETINA IN ARTERIO-SCLEROSIS AND RENAL DISEASE."

Dr. G. NEWTON PITT (Chairman) (p. 1), Dr. H. BATTY SHAW (p. 1), Mr. R. FOSTER MOORE (p. 5), Mr. PERCY BARDSLEY (Salisbury) (p. 15), Mr. PHILIP ADAMS (Oxford) (p. 16), Dr. ARTHUR ELLIS (p. 17), Dr. C. O. HAWTHORNE (p. 20), Dr. J. F. GASKELL (Cambridge) (p. 20).

December 8, 1922.

ADJOURNED DISCUSSION ON "THE SIGNIFICANCE OF THE VASCULAR AND OTHER CHANGES IN THE RETINA IN ARTERIO-SCLEROSIS AND RENAL DISEASE."

Mr. ERNEST CLARKE (p. 22), Dr. W. N. GOLDSCHMIDT (p. 23), Mr. D. LEIGHTON DAVIES (Cardiff) (p. 26), Mr. M. S. MAYOU (p. 27), Dr. G. NEWTON PITT (President of the Section of Medicine) (p. 28), Dr. A. FEILING (p. 29), Mr. J. HERBERT FISHER (p. 30), Dr. C. F. HARFORD (p. 32), Dr. BATTY SHAW (in reply) (read by Dr. IZOD BENNETT) (p. 33), Mr. R. FOSTER MOORE (in reply) (p. 35).

The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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Section of Ophthalmology.

President—Mr. A. L. WHITEHEAD, M.B.

Calcareous Degeneration of the Eye, with Deposits on the Iris.

By RAYNER BATTEN, M.D.

PATIENT, a male, aged 71. History of injury to right eye when aged 2 years. Corneal opacity: dense, white, opaque, and apparently crystalline. Bright white deposits on the pupillary margin of the iris.

THE following cases were also shown:—

- (1) "Cyst of the Retina," By J. A. VALENTINE, M.D.
Discussed by Mr. TREACHER COLLINS.
- (2) "Angeoid Streaks of the Retina." By F. A. WILLIAMSON-NOBLE, F.R.C.S.

A Plane Glass Retinoscope.

By F. A. WILLIAMSON-NOBLE, F.R.C.S.

THIS consists of two metal tubes set at an angle of 30° . At the end of one tube is mounted a 30 c.p. "pointolite" in a light-tight casing, with a lens and focussing arrangement so that a beam of parallel rays is projected down the centre of the tube. Adjustable stops are provided in front of and behind the lens and an orange-red colour filter is fitted. At the end of the second tube is an automatic lens-changing mechanism. Where the tubes are joined there is a piece of plane glass, so arranged that it projects a reflected beam of light towards the lens-changing mechanism.

The surgeon sits about a foot away from the plane glass, the patient at the end of the tube carrying the lens-changing mechanism; he is directed to look at the image of the source of light formed by the plane glass. The surgeon will see the usual yellow-red reflex from the patient's fundus, and on moving his head will note that the "shadow" moves with him in hypermetropia and myopia of less than 1D. and against in myopia exceeding 1D.

By turning the handle of the lens-changing mechanism, neutralization is effected in the two meridians.

The instrument shown was the experimental model, the final form will be slightly different and will incorporate some improvement. It was designed with the following objects in view:—

- (1) To enable one to perform retinoscopy at the macula without the use of a mydriatic.
- (2) To cut out all light from the surgeon's eye except what is reflected from the patient's fundus.
- (3) By means of the stops and colour filter to diminish the luminosity of the image at which the patient gazes to a point where it produces no unpleasant dazzling.
- (4) To enable one to make lens-changes with great speed and in practically total darkness.

Some Aspects of Ocular Tuberculosis:

PRESIDENT'S ADDRESS.

By A. L. WHITEHEAD, M.B. (President).

THE tubercle bacillus is so widely distributed and lesions due to it are so common elsewhere in the body that it is only natural that much should have been written about tuberculous diseases of the eye. I do not propose to occupy your time with a digest of the extensive literature on the subject, but to give you some of my own impressions of ocular tuberculosis, in comparison with the experience of other observers.

Tuberculosis is more frequent in Leeds and less frequent in the West Riding of Yorkshire than in England and Wales as a whole. The official figures supplied to me by the Ministry of Health and the medical officer's annual reports are as follows for 1920: England and Wales, with a population of 37,524,000, had a tuberculosis case-rate of 2'07, the lowest since 1914. The West Riding of Yorkshire, with a population of 1,498,453, had a case-rate of 1'61, also the lowest since 1914. Leeds, with a population of 448,913, had a case rate of 2'61, the lowest since 1914.

A large percentage of our patients in hospital and private practice reside outside Leeds, so that probably on the average the general case-rate will not be very dissimilar to that of England and Wales as a whole.

Every ophthalmologist must have been struck by the comparative rarity of tubercular lesions of the eye in pulmonary tuberculosis, and in serious cases of surgical tuberculosis, but if cases of ocular tuberculosis are followed up, there is good evidence that in after years other manifestations are frequently present.

A valuable paper bearing upon this point was contributed in 1921 by Igersheimer and Prinz [1], dealing with the after-history of ninety-two scrofulous eye cases followed up for ten to thirty years. The authors found that in 13'9 per cent. there were changes in the lungs, and in 13 per cent. there were symptoms of active tuberculosis. In discussing the various forms of ocular tuberculosis, I shall refer again to this question.

There seems to be a very important difference between the severe spreading invasions of the eye, leading to more or less complete destruction of the eyeball, and the more chronic and benign forms of tuberculous infiltration. In the former, typical giant cells are common, and the tubercle bacilli are frequently found; in the latter, careful search and inoculation tests usually fail to demonstrate the bacillus. There is a general consensus of opinion that in the latter group of cases the infection is due to a tuberculotoxin acting on a specially prepared area. This view has been supported by experimental evidence furnished by Guillery [2] and others.

I do not propose to say anything about conglomerate tubercle of the uveal tract, leading to destruction of the eyeball, either by perforation of the outer coats of the eye or by caseation and subsequent shrinking of the globe. The recognized text-books deal very fully with this condition, and my experience coincides with the usual description.

Tubercular Choroiditis.

Miliary tubercles of the choroid in acute tuberculosis of infants are frequently present. In nearly all of the many cases of fatal tubercular menin-

gitis in which I have had the opportunity of examining the fundus, I have found miliary tubercles. Isolated tubercles, usually single, are not infrequent, and, unless at or near the macula, attract little attention. They occur in the same class of individuals in whom these less severe forms of tuberculous infection of the eye are usually found. They are small, one-third to one-half of the size of the optic disc, greyish yellow with a soft indefinite margin. In the later stages the circular clean cut edges of the atrophic patch are very characteristic. Occurring in early infancy, they are not usually recognized until, as the child grows up, the interference with vision attracts attention. I do not think I have seen them in individuals over the age of 30.

Primary Tubercle of the Retina.

I have not seen a case of primary tubercle of the retina, but I have notes of one case in which I regarded a nodule in the papilla as probably tubercular.

Degenerative changes in the blood-vessels, usually the veins, followed by hæmorrhages, either small, confined to the retina, or large, bursting into the vitreous, have been described by Jackson and Finnoff and by de Schweinitz. In my own experience, in two instances, vitreous hæmorrhages have preceded any other manifestations of general tubercular infection. In both these cases, pulmonary tuberculosis became manifest in from twelve to eighteen months after the appearance of the hæmorrhages; in one a typical tubercular iritis with nodules came on after the pulmonary signs were defined, but cleared up under treatment.

Chatterton [3], at a meeting of this Section in 1913, reported a case of tubercular iritis with vitreous hæmorrhages treated by tuberculin. It is possible that some of the unexplained cases of recurrent vitreous hæmorrhage may be due to toxin infection from a tubercular focus elsewhere in the body. Several observers, notably Schieck [4], have reported favourably on the use of tuberculin in recurrent retinal hæmorrhages.

Tubercular Iritis.

I have nothing to add to the text-book descriptions of the various forms of tubercular nodules in the iris. We have all seen many examples of the recognized varieties. Here I would point out that in many of the milder cases the nodules are small, disappear early, and leave little, if any, permanent change in the iris structure. They usually occur in young and apparently robust young adults, frequently females, and with no other evidence of tuberculosis, but often with the family history of susceptibility to infection. In these cases the value of tuberculin treatment by proper dosage has been repeatedly demonstrated.

Hessberg [5] considers that more than 50 per cent. of all cases of iritis are attributable to tuberculosis; he states that the nodules are frequently very small, lasting often only a few hours, and may disappear without leaving any scar. Phthisis and joint tuberculosis are rarely present, but the lymphatic glands of the mediastinum and mesentery are frequently affected. I do not think many ophthalmologists will agree with this extraordinarily high percentage.

Tuberculous Disease of the Lacrymal Sac.

Tuberculous disease of the lacrymal sac is not common in my experience. I have seen it secondary to lupus of the nose, and as a forward extension of tubercular disease of the ethmoid.

4 Whitehead: *Some Aspects of Ocular Tuberculosis*

The possibility of the occurrence of an epiphora and dilatation of the sac secondary to nasal tuberculosis should always be remembered in young adults. In one case under my care, such a chronic dacryocystitis followed a tuberculous ulceration of the inferior turbinate and septum. Tuberculous ulceration of the vulva occurred in this case—that of a girl, aged 12, and it was entirely cured by local and general treatment; but a year later a toe had to be amputated for tubercular "dactylitis"; then the nose and lachrymal sac became affected. Later on the pharynx and larynx were involved. The whole process has continued for five years, and during that time the child has looked the picture of health, and is of the most robust physical development. What her ultimate fate will be I cannot say.

Tubercular Conjunctivitis.

The severe forms of tubercular conjunctivitis with large subepithelial nodules leading to ulceration and formation of large granulations and the so-called coxcomb excrescences are rare in my experience. Lupus of the conjunctiva spreading by direct infection from the face is quite characteristic and calls for no comment.

Many papers have been written in many languages discussing the possible tuberculous origin of the common phlyctenular conjunctivitis of children. Veeder and Hempelmann [6], in the *New York Medical Journal* of 1920, reported 196 cases of phlyctenular disease. There was a positive von Pirquet skin reaction in ninety-three cases. Tuberculous lesions involving other organs were found in half the cases, and in children kept under observation for one year or longer four-fifths gave evidence of other tuberculous processes.

For some time past I have frequently noticed a type of phlyctenular conjunctivitis which I have come to regard as definitely tubercular. It occurs in young adults, rarely in small children, is very chronic in type and is resistant to the ordinary treatment. The phlyctenules are small, closely set, aggregated together near the corneo-scleral junction and there is frequently infiltration of the cornea, with rather large invading vessels; ulceration is very rare. In some instances iritis is also present. I have water-colour drawings of two typical cases of this variety of conjunctivitis with slight corneal invasions and iritis.

In all the cases there are either evidences of tuberculous processes elsewhere or a clear family history of susceptibility to tubercle invasion. The von Pirquet reaction is frequently positive, but not invariably so, although a definite but slight reaction occurring between twenty-four and forty-eight hours after inoculation, and regarded by some observers as positive, is almost always present. Examination of the tissue removed does not show typical giant cells or the presence of tubercle bacilli, and inoculation of guinea-pigs has given negative results.

Although this group of cases is very resistant to ordinary treatment, immediate and striking improvement follows tuberculin injections. It is most important to give in each case the appropriate dose. Tuberculin B.E. is used and the initial dose is 1/5,000 mg. to 1/10,000 mg. according to the age of the patient; four-hourly temperature records are taken for twenty-four hours with the patient in bed; if there is no temperature or general reaction, an increasing dose is given three days later. Our usual scale is 1/5,000, 1/2,500, 1/1,000, 1/750, 1/500, 1/250, 1/100, 1/50 mg. When the reaction is obtained the dose is repeated and if there is again a reaction, the dose just smaller than the earlier one is given, and repeated at weekly or fortnightly intervals according

to the case. I have dealt with this in detail, since I am quite satisfied that only the dose suitable for the individual will give good results. The efficient dose has varied from 1/50 mg. to 1/500 mg., even in cases of about the same age, this showing the uselessness of a uniform dose. A severe general reaction is very unusual, an elevation of temperature of 1° to 2° being the usual indication; in a very few cases local reaction with suppuration at the site of injection is the only indication.

I have no experience of the method suggested by Ellis and Gay [7] of treating tuberculous eyes by the direct local application of various dilutions of tuberculin P.T.O. (bovine), and I do not know whether the authors have found that a more prolonged trial of this treatment justifies its use.

Appropriate constitutional treatment is essential; if possible an entirely open-air life on sanatorium principles should be insisted upon.

Since it is an almost established fact that these minor lesions of ocular tuberculosis are due to toxin infection from a focus elsewhere, it is most important to search for such a focus and when it is found, to employ suitable treatment. Infected tonsils and adenoids should, of course, be dealt with by operation.

The question of treatment of enlarged glands by operation or otherwise is also of great importance. Wolff [8], of Amsterdam, has strongly urged the value of the treatment of the tuberculous lymphatic glands by X-rays in cases of tubercular conjunctivitis and keratitis. I have no experience of this, neither have I tried injections of milk in these cases. After an X-ray examination of the thorax in cases of chronic uveitis, Siegrist [9] came to the conclusion that the intrathoracic bronchial glands were probably the primary focus.

Professor Stewart has supplied me with some very important observations directly bearing upon the question of the possibility of the mediastinal glands being the primary focus in cases of toxin infection of the eye.

In post-mortems in 100 cases of death from rapidly fatal war injuries in men of the best physique, from the front lines, the following striking facts were elicited. In 42 per cent. there were undoubted evidences of tuberculous disease, in 11 per cent. scarring at the apices of the lungs and in 1 per cent. extensive old pleuritic adhesions, making 54 per cent. probably tuberculous lesions. In 32 per cent. of the cases there were definite signs of glandular infection, 15 per cent. being bronchial, 15 per cent. mesenteric, 1 per cent. pre-pancreatic, and 1 per cent. cervical.

In view of these facts indicating the frequency of latent tuberculosis in the glands, it seems very probable that the primary focus in these cases of tuberculo-toxin infection may be in the bronchial or other glands. Kleinschmidt [10] believes that only a small portion of phlyctenular cases are free from tubercle while many others have merely a quiescent lesion.

Ordinary local treatment is usually employed, atropin ointment and boric acid lotion irrigations in the early stages, yellow oxide of mercury ointment and powdered calomel insufflations as the more acute symptoms subside.

Operative treatment in some of the cases is of the greatest value. I have tried excision of the affected conjunctiva in some cases, but without especially satisfactory results. In cases in which there is a vascular invasion of the cornea, a form of peritomy is of very great service.

The most satisfactory results seemed to be obtained if a strip of conjunctiva, 2 or 3 mm. in width is excised all round the corneo-scleral junction and the larger superficial vessels involving the cornea are carefully scarified by the point of a Graefe's knife; six or seven cuts are made across each vessel.

6 Whitehead: *Some Aspects of Ocular Tuberculosis*

The value of peritomy has been emphasized by several ophthalmic surgeons; my revered colleague, Mr. Pridgin Teale, possibly the doyen of ophthalmologists, has advocated the operation repeatedly, and in later years Sir Anderson Crichtett and Sir Richard Cruise have urged its performance in suitable cases. Personally I am firmly convinced that it is one of the most valuable aids which we possess in the treatment of these otherwise very intractable cases.

In ten recent cases of the type of phlyctenular conjunctivitis which I have described, the ages ranged from 10 to 27, the average being 15·3. In four cases there were enlarged cervical glands; in one a patch of lupus on the cheek, not in the neighbourhood of the eye; in one a tubercular synovitis of the knee; in one the mother had suffered from tubercular peritonitis and in another a sister died of acute pulmonary tuberculosis. Many of the cases had been under the usual treatment for long periods without benefit; in one case for nearly three years and in another for over eighteen months. Peritomy was performed in five cases. In one case in which vision had been less than $\frac{6}{80}$ six months ago, it is now $\frac{6}{8}$ in one eye and partial $\frac{6}{8}$ in the other and the corneal opacities have almost disappeared. In all, the improvement was rapid and very marked, and there has been an entire freedom from relapses, which had been frequent before the special treatment was undertaken.

Tubercular Keratitis.

Tubercular keratitis following infection after injury is, I think, rare, and secondary infection from the conjunctiva, the ciliary body or the iris is equally uncommon. A parenchymatous affection, to some extent simulating the keratitis of congenital syphilis, is frequent and is worthy of greater attention. In tubercular keratitis, the surface of the cornea is more irregular; although actual ulceration is rare, the patches of opacity are less regular in distribution and the vascular invasion of the cornea is much more superficial. The onset is not so rapid and the progress is much more erratic. In congenital syphilis it is usual to find the keratitis steadily and progressively advancing, followed by steady and progressive resolution. Peritomy is, in my experience, of the greatest value in the treatment of the pannus of tubercular keratitis and quite valueless in congenital syphilitic keratitis. The possibility of a mixed infection must always be borne in mind.

Tubercular Scleritis.

Tubercular scleritis and episcleritis, often with involvement of the iris and cornea, are possibly in some instances due to direct tuberculous infection. One of the most serious and intractable cases I have ever seen, occurred in a gentleman whose wife was suffering from advanced pulmonary tuberculosis; although he himself had no evidence of infection elsewhere.

The conclusions to which my observations point, but for which I do not claim any originality, are these:—

- (1) Ocular tuberculosis is much more frequent than is usually taught.
- (2) Two very well defined classes of lesions exist: (a) The destructive, caseating or necrosing variety in which the infection is primary, or part of a generalized distribution of tubercle, progressive, and only slightly, or not at all, amenable to treatment. Tubercle bacilli are frequently discovered in these cases. (b) A much milder form of infection, often called attenuated tuberculosis, probably of atoxic nature, since bacilli and giant cells are rarely found. The primary focus is usually not in the eye, and in many cases cannot be

discovered by a careful investigation. The bronchial or mediastinal glands may in many instances be the focus in question.

In the second class recovery is the rule, with more or less interference with vision according to the areas of the eye affected. In the treatment of this group, properly graduated tuberculin injections are of the greatest value. In tubercular vascular keratitis, peritomy and scarification of the invading vessels is a most valuable aid to treatment.

If the primary focus is in a situation where removal can be carried out by surgical means, operation should be carefully considered.

If, as seems frequently the case, the focus is in the bronchial or mesenteric glands, constitutional treatment of the most thorough variety obtainable must be undertaken.

Finally, I wish to express my indebtedness to Professor Stewart and Dr. McLeod, of the Pathological Department of the University of Leeds, for valuable assistance in investigating the pathology and bacteriology of some of my cases; to Mr. S. D. Lodge and the sisters of my wards in the General Infirmary at Leeds, and to various tuberculosis officers, for the careful supervision and execution of the treatment.

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Standards of Vision for Scholars and Teachers in Council Schools.

(ABSTRACT.)

By N. BISHOP HARMAN, F.R.C.S.

[This paper will shortly be published in full in the *British Medical Journal*.]

THE teacher's life is a hard one. Short hours and good holidays do not ensure against risk of strain to normal eyes, and the risk is increased where there is defect. The teacher's work is not limited to the classroom, there is much preparation to be done; also in schools worth the name there is a large social element the organization of which falls to the teacher in State schools and not to prefects and the like.

The State has provided an "educational ladder": scholarships, college, and university training. It is a corollary of this provision that there should be required of the recipient of these bounties sufficient physique to take advantage of them; otherwise there is waste, and better candidates may be kept out by the failures. Tests of vision exist, but it is desirable that these should be standardized; otherwise the anomaly arises that a teacher trained in one area is refused in another, whereas trained teachers should be freely interchangeable, for free movement engenders breadth of view and experience.

Junior scholarships are awarded at the age of 11, and give five years at a secondary school; senior scholarships are awarded at 16 years, and give four

8 Harman: *Standards of Vision for Scholars and Teachers*

years at a university; the training college is entered at 16; and teachers are admitted to their work at 21.

Evidence of the disability of defective vision was recently given by the author so far as myopes are concerned.¹ It was found that myopes of 3 to 5D. engaged in continuous close work showed failure to work to the extent of 33 per cent.; those from 5 to 10D. to the extent of 60 per cent.; and those over 10D. to 77 per cent. The whole of these myopes showed a percentage of failure of 53 per cent., whereas myopes of the same order who did not engage in continuous close work only failed to the extent of 9·4 per cent.

On this basis standards have been worked out and are submitted for consideration. Starting with the allowable margin of error in teachers at the age of 21, the difference is estimated for the children at the age of 11. The table as given embodies the following results:—

TABLE OF STANDARDS.

(a) *Hypermetropia.*

Candidates	Age	Sphere	Cylinder	Sphero-cylinder
All	11-21	5D.	4D.	Average of four meridians = 5; astigmatism not over 3D., e.g., + 8·5D. sph. with + 3D. cyl.

(b) *Myopia, with mixed Astigmatism.*

Teacherships	...	21	5D.	4D.	Ditto	ditto
Senior scholars	...	16	4D.	3D.	Average of four meridians = 4; astigmatism not over 3D., e.g., - 3D. sph. with - 2D. cyl.	
Junior scholars	...	11	3D.	3D.	Average = 3D.; astigmatism 2D., e.g., - 2D. sph. with - 2D. cyl.	

Allowance is made for a number of possible defects due to odd eyes, old inflammations resulting in scarred eyes, and the like, amblyopia from squint, and defects of colour vision.

Difficulty may sometimes arise in the disbaring of young myopes who are alleged by their teachers to be "brilliant." Too much stress should not be laid on these reports. Myopes give an undue amount of time to close work because the state of their sight is a handicap for games. This gives them an unfair advantage over their normal comrades, who may be the better workers in the long run. The 2D. difference between teacher and junior scholar in the allowable degree of myopia is none too much for ten years' hard study from the age of 11 to 21—just the most critical years of development.

It is better in such cases, both in the interests of the candidates and of the teaching service, to divert their work at an early age to safer channels. There are now many "trade scholarships," the work of many of which entails little strain on the eyes, and which provide an entry into valuable spheres of usefulness. For some few whose capacities are clearly scholastic it is possible to provide an avenue through training for teachers in schools for the blind and in myope classes.

DISCUSSION.

Dr. F. C. SHRUBSALL said that the subject of myopia was of great importance. and if tests broke down, the official to bear the brunt was the school medical officer. When the school medical officer rejected a candidate, he was supposed to suggest alternatives. He quoted the case of a girl, aged 13, who wished to commence work

¹ *Trans. Ophth. Soc. U.K.*, 1922, xlii, pp. 20-25.

for the teaching profession. As it was held that the profession was unsuited to her, she was rejected. She then brought a certificate from the ophthalmic department of a large hospital, in the following words: "I see no objection to this girl having a scholarship provided she does not overdo near work, and she should not do any unnecessary reading, such as novels, nor should she read at night." He believed these limitations were sure to be exceeded, whatever warnings were given, and the hours and conditions of work could not be controlled from the school. And in recommending alternative occupations strange difficulties might arise. Cases of defective vision were often recommended for agriculture, but it was pointed out to the school medical officer that a boy who could not see properly would hoe up the potatoes and leave the weeds behind. It was said that school work was the cause of myopia, but there was little doubt that in many cases myopia was inherited in families, and was manifested mostly by the female members of it, not uncommonly being associated with a studious frame of mind, and such a person could not be induced to abandon study for open spaces. Most of the figures relating to myopia in school life had come from Swedish and German sources, and it was shown that abroad myopia was diminishing. The results had been variously assigned to the introduction of physical exercises and to the giving up of intense classical studies and the use of Gothic print. But in this country the conditions of study and home-work had never been so severe. Lighting arrangements had been held accountable in assigning a cause for the prevalence of myopia, but a report from Strasbourg, issued in 1912, stated that there was a greater incidence of myopia in the light schools of the suburbs than in the darker schools in the centre of the whole town. General hygiene must play a part, but most observers placed the greatest stress on heredity. E. Thomson gave the proportion in different school populations in Lancashire as: urban 17 per cent., rural 20.5 per cent., mining 26 per cent. The school medical officer must come to the ophthalmologist for guidance, and it was desirable that this should be authoritative and generally accepted. It was very desirable that a committee of experts should consider the subject and issue some standard.

Mr. ERNEST CLARKE, after suggesting that the considered opinion of the Section should be forwarded to the Council of British Ophthalmologists, said that when one came to arrange the standards, the matter was very difficult if one had to confine oneself to the eyes. He thought the physique of the child should be taken into account as well. Again, the previous history was very important, especially in regard to myopia. In a dozen cases of myopia ten might remain stationary, or would progress very slightly, while in the other two no treatment would seem to stop the progress. He supposed an allowance of 2D. for the progress up to the age of 21 might be taken as the average. With regard to hypermetropia, he thought Mr. Harman was very liberal in allowing 5D. Again, what about the astigmatism? He thought it was the small errors of astigmatism which counted much more than the big ones. He asked whether Mr. Harman was wise in admitting the amblyope. The squint might be cured, but if the eye remained amblyopic he or she remained a one-eyed person. If Mr. Harman allowed that child to be a scholar, and afterwards to become a teacher, would he also admit one who had lost his eye from an accident? He agreed with the remarks concerning special exceptions. It was known that some people were born teachers, and, as far as possible, they should be encouraged to adopt teaching as their profession. It seemed to him that what the standard should be was largely a question of supply and demand. If there were plenty of scholars and plenty of candidates for teacherships, a high standard could be set, but if there were a paucity of supply, the standard would have to be lowered.

Mr. INGLIS POLLOCK said that the standard advocated by Mr. Harman had already been assumed in Glasgow. If there were a bulging of the posterior segment, or any sign of fundus disease, that disqualified the child, so did a visual acuity of below $\frac{3}{6}$ and myopia of over 4D. The children there passed into training at 15 or 16 years of age, when they were subjected to a medical examination, and the eyes were examined independently by an oculist, who had not to do with the candidate's general health. The person responsible for their admission had to consider all the

10 Harman: *Standards of Vision for Scholars and Teachers*

factors. If satisfactory, the child proceeded to four years of training, at the end of which period he had another medical examination. If at 18 years of age he was found to be more myopic, he was disqualified. Inquiry was made as to whether there was myopia in the family, and if so, and if the myopia in the child was on the increase, rejection ensued, unless the candidate agreed to be excluded from the superannuation scheme. It might be that after the four years the examiner was a different one from the one who made the first examination. If a candidate was to be rejected, it was most important that this should be decided upon before the four years of training were entered into, as after 19 years of age it was too late for training for most trades. At 40 to 45 years of age, the myope might have stretching of the eyeball and atrophy of the macula setting in, therefore one had to consider the exclusion of those who might become partially blind.

Mr. T. HARRISON BUTLER said that as teachers were paid by a public body it was legitimate to lay down a standard of eyesight for them, but the matter of awarding scholarships was on a different footing. He could not see that because a child had myopia of 3D., which might or might not be progressive, he should not be allowed to hold a scholarship, and denied a liberal education. Discretion must be allowed to the medical man who was called upon to decide the fitness of the child. It was not possible to settle the question from one examination. If the child was found to be myopic it was necessary to examine it again after a year or more; then and then only could it be said that the disease was progressive. A hard and fast standard would exclude many clever children and condemn them to hand labour for which they were perhaps unfitted. It would also be detrimental to the general standard of education, for the myope was often highly studious and likely to profit from a scholarship. He believed that there was no conclusive evidence that the progress of myopia was dependent upon near work.

Mr. A. L. WHITEHEAD (President) said that it occurred to him that Mr. Harman had practically never considered the question of the visual acuity of the individual; he merely dealt with the error of refraction in figures. He (the President) felt that while the widest discretion should be allowed in individual cases, some sort of standard, such as Mr. Bishop Harman suggested, would be of great value as forming a basis on which to work.

Mr. BISHOP HARMAN (in reply) said that he assumed a reasonable visual acuity for teachers ($\frac{1}{8}$ each eye, or $\frac{1}{4}$ the better eye when one was defective); but he would strongly urge that visual acuity was not the main test for these purposes. Mr. Ernest Clarke's plea for a distinction between the robust and the puny was not the business of the ophthalmologist, who had to deal with the eyes alone. Low degrees of astigmatism were troublesome, but they could and should be corrected. He would not object to a one-eyed teacher. It was possible to recognize Mr. Harrison Butler's suggestion that weight should be attached to the point as to whether or no the myopia was progressive. The belief that myopia had nothing to do with close work he was sure was wrong. He had referred to the histories of 480 myopes¹ classified according to age, work, and rates of breakdown; the breakdowns amongst the habitual close eye workers were altogether out of proportion to those amongst myopes not so engaged. That was evidence of the need of some check. An agreed standard should provide the check. Cases were far too numerous to allow every case to be treated "on its merits" irrespective of a standard. Mr. Harman urged the Section to instigate an inquiry into the matter.

The PRESIDENT suggested that the question of referring the matter under discussion to the Council of British Ophthalmologists for their consideration should be put to the vote.

The meeting unanimously agreed that it should be so referred.

¹ *Trans. Ophth. Soc. U.K.*, 1922, xlii, p. 22.

Section of Ophthalmology.

President—Mr. A. L. WHITEHEAD, M.B.

A Case of Retinitis Circinata.

By HUMPHREY NEAME, F.R.C.S.

PATIENT, a male, aged 39.

April, 1922: Suddenly noticed that he could hardly see with right eye, and that there was a "spot" in front of it.

June, 1922: At Central London Ophthalmic Hospital, right vision $\frac{1}{20}$, right fundus showed a fairly large hæmorrhage in macular region.

August, 1922: Circular grey swelling above macula in addition to large old hæmorrhage below, and some small hæmorrhages.

September, 1922: Very delicate "milky way" incompletely around macular region. Minute white dots—retinitis circinata. Angeioid streaks in left fundus. Mottled appearance well to temporal side of each macular region. Central scotoma, right. Field full.

October, 1922: Retinitis circinata well marked.

The interesting points are the relative youth of the man and the development of the retinitis circinata under observation.

(Coloured drawings shown by epidiascope.)

We thought of various possibilities—an inflammation, a neoplasm, a cyst. A neoplasm is excluded, because the condition has remained fairly constant for three months. The question of tubercle was considered. Tuberculin injections were therefore given (B.E.) starting from 0.00001 mg., but there was no temperature reaction, and no local exacerbation. Against its being inflammatory in origin is the fact that the surface of this swelling is smooth and clear, and that the little vessels are sharply defined. The Wassermann test was negative, and there was neither history nor sign of syphilis. Other possibilities are a cyst of the retina, or retinitis hæmorrhagica externa of Coats.

I think the swelling is either a cyst in the retina or an exudative retinitis with a deposit of fibrous tissue in the seat of a previous hæmorrhage.

The last point of interest in the case is the presence of angeioid streaks; as regards the cause of these we have not got beyond the theoretical stage.

DISCUSSION.

Mr. MALCOLM HEPBURN said that many of these cases of so-called retinitis circinata had origin in the choroid, for it was there that the original inflammatory deposit took place. In old cases one could generally find scars at the macula, with

12 Neame: *Retinitis Circinata*; Hine: *Ectopia Lentis*

much pigmentation associated with organized outlying exudate. The present case was in an early stage, and therefore it was an acute metastatic inflammatory deposit; scar tissue would develop later. These deposits were also found in many other parts of the choroid, but were always more intense at the macula. Some part of the retinitis circinata would clear up, and some would remain as a fibrous or hyaline deposit. At the present time this formed almost a complete ring.

Sir WILLIAM LISTER said he had had the opportunity of seeing a big choroidal hæmorrhage go through the "melting snow" stage which occurred in retinitis circinata. The patient had had a blow on his eye, followed by a large subretinal hæmorrhage in the region of the macula, which caused a plum-coloured spot, unlike the cherry-red spot of embolism. Gradually the hæmorrhage absorbed, becoming a "melting snow" kind of white area, just as in the case under discussion. Then that area disappeared as it became absorbed, and disclosed a large choroidal rupture. It was interesting to see the configuration of white areas, such as occurred in retinitis circinata, as a purely temporary condition following the rupture. He believed that in cases of retinitis circinata there was some form of infection which caused the change to become more permanent; whereas in the case due to a blow there was no infection, and the whole of the hæmorrhage, and the condition following the hæmorrhage, disappeared.

Mr. LESLIE PATON said the case of this condition shown at the last meeting by Mr. Williamson-Noble had a positive complement-fixation test to tubercle. He did not yet know what was the precise value of that test, but those who did the test placed reliance on it as an indication of tubercle. The lesion in the present case might be a tubercular nodule, not a cyst.

Mr. NEAME (in reply) said he agreed with Mr. Hepburn as to the possibility of a choroidal origin in some cases of the disease, but not by any means in all. Sir Jonathan Hutchinson,¹ in his original paper, describing ten cases (the first description published) held that they were all of choroidal origin, and referred to the spots, from the clinical point of view, as in the choroid. Most subsequent writers, however, dissented from this opinion. He thought there must be various causes of retinitis circinata, which he regarded rather as a physical sign.

Case of *Ectopia Lentis* (both Eyes).

By MONTAGUE L. HINE, M.D.

PATIENT, a male, aged 6. Congenital abnormality. No other members of the family affected. No other deformities.

I have brought the patient in order to invite opinions as to what it is best to do in such cases. What is usually the ultimate end of these cases? Has any member seen a case of the kind which has not been operated upon? Has a case been seen which has persisted without operation to adult age? Or does some complication always supervene?

DISCUSSION.

Mr. A. L. WHITEHEAD (President) said he had operated upon two or three cases of this kind by fixing the lens with a needle, and breaking it up with another needle. It was difficult to perform a dissection unless this was done first. The cases in which he had carried this out did well so far as removal of the lens was concerned, but he had always found the acuity of vision afterwards to be subnormal; obviously there was some congenital amblyopia associated with defective development, which had caused the lens to be out of place. He had recently seen a man over 60 years of age with an

¹ *Roy. Lond. Ophth. Hosp. Repts.*, 1876, viii, p. 231.

upward dislocation of his lenses, which were developing senile opacities. As the man could still get about fairly comfortably, he had not yet operated and he was not looking forward to the prospect of removing a congenitally-dislocated lens which was the subject of senile opacity.

Mr. T. HARRISON BUTLER said he had operated upon some adult cases of this kind, by the method of an American who wrote an interesting paper on removing dislocated lenses from the vitreous. The first necessity was to have an extraordinarily good light. Next, to do a preliminary iridectomy. By using an iris hook there was no particular difficulty; then one should wait until the eye settled down. In the last case of the kind he did, a general anæsthetic had been necessary. There was a corneal incision, and he had a Smith spoon ready, and the lens came out easily, without loss of vitreous. Recently the iris had become somewhat drawn up. The operation had been done five years ago, and the vision was now $\frac{1}{8}$. Another dislocated lens, in the case of an infant, he needled with a Ziegler's knife; it was not necessary to fix it; but, up to the present, there had been no sign of absorption. He thought an injection of novocaine, to paralyse the orbicularis, would be better than a general anæsthetic. In some cases the lens slid out easily in a most unexpected way. If left, many of them became dislocated into the anterior chamber and caused glaucoma; others dropped into the vitreous and caused cyclitis.

Mr. R. AFFLECK GREEVES remarked that recently he operated upon two cases, and found that the needling was not particularly difficult. He needled them with a Ziegler knife, putting the knife in at the periphery, close to the attachment of the lens, and drawing it inwards towards the centre of the pupil, and there seemed no difficulty in cutting the capsule. In the last patient he operated on both eyes; one of them he allowed to absorb, the other he evacuated, and in both cases the pupil was now clear. Vision, though much improved, was still subnormal, about $\frac{1}{12}$ or $\frac{1}{13}$, whereas before the operation it was $\frac{1}{60}$ and could not be improved beyond that.

Mr. D. LEIGHTON DAVIES said that, in deciding whether one should operate upon these cases, much depended on whether the edge of the dislocated lens lay inside the pupillary area when the pupil was contracted. If it just came to the pupil, the case was better left alone, because he thought there must be some congenital want of development in the function of the retina also in these cases. If they came within that, there must be more disturbance of vision, and it was always best to needle those cases. He always fixed them with the needle behind the sclera, pushing the lens well forward. The best results he had had were to secure $\frac{1}{12}$ after less than $\frac{1}{60}$; most of them got about $\frac{1}{12}$. On the whole it was better to needle than to do a large operation like an extraction in a young child.

Hæmangioma of Orbit.

By LESLIE PATON, F.R.C.S.

PATIENT, a female, age 76, widow. Right eye, proptosis four years. No pain "except occasionally."

On admission: Eye displaced forwards, downwards and inwards. Lump to be felt below and external to globe by finger pressed deeply into orbit. Enlarged right submaxillary gland. Small shot-like gland in right supra-clavicular region. Upper orbital margin irregular, result of old fracture.

June 24, 1922: Mr. Clayton-Greene examined the glands and did not consider them to be malignant.

July 18, 1922: Operation by Mr. Paton. Outer canthus was split to disclose the tumour, which was removed and the incision sutured.

Tumour was found to be an angioma. The tumour is a simple cavernous angioma. It consists of irregular spaces containing circulating blood and

14 Paton : *Hæmangioma of Orbit ; Recurrent Detached Retina*

lined by endothelium. They are bound together by a loose, fairly cellular connective tissue.

My main reason for showing this case is that practically none of the cases which have been described—and Lagrange collected over eighty of them—have been in patients over 60 years of age. They have mostly occurred in young adults under 40 years of age. One theory is that they are of the nature of congenital nævi, growing as the age increases. In the case of this old lady I am showing, there was practically no trace of anything wrong until she was over 70; she is now aged 76. A complicating point was the presence of enlarged glands in the neck. She insisted on having something done, and I told her I would remove a piece of the growth for examination. I started, but found a growth the size of a walnut, which was encapsuled, and came out very simply and beautifully; its removal was followed by practically no bleeding. The present position of her eye is due to cicatricial contracture of the external rectus. At some date there has been a fracture of the upper margin of her orbit, and the question is whether in that case there is some connexion between the injury at that time and the development of this cavernous angioma at her late age.

Mr. HUMPHREY NEAME said there was a case at the Royal London Ophthalmic Hospital, under Mr. Fisher, last year, in which a larger tumour than in this case was present. The eye was useless, therefore exenteration was performed. Enormous vessels were found, almost as large as the little finger. In that case there was a history of a gunshot wound, and of a small metal fragment being retained in the orbit; the angioma presumably was started by the injury to vessels.

Case of Recurrent Detached Retina after Seventeen Years' Reposition.

By LESLIE PATON, F.R.C.S.

PATIENT, a male, was operated upon originally for detachment of retina in 1904. He had three or four operations performed upon him at Westminster Ophthalmic Hospital, six at Moorfields, and I have done five operations on him. Subsequently to the last operation on his left eye he had obtained reposition of his retina, and has been using that eye from 1904 until last December (1921). During those seventeen years he has been going about London with newspapers on a bicycle, threading cleverly through the traffic as such men can be seen to do, therefore it can be assumed that he has had fairly useful vision. He has had no restoration of vision in his right eye, though he had reposition of that retina. Eleven months ago he had a recurrence of the loss of his sight in the left eye, and I did not see him again until last month, when I saw he had a large balloon-shaped detachment in the upper and lower outer quadrant of his left eye. I operated upon him again, and he illustrates the condition frequently seen in cases of repositioned retina after old detachment. There is very marked pigmentation, and in his left eye there is still a shallow detachment in the quadrant I did not operate upon, reaching to 7D. In the lower detachment there is a fold which runs along what was the inner limit of the detachment in the lower quadrant, but the retina is back in position over the whole lower quadrant except this fold.

I had another case of recurrence of detachment in a man who was operated upon in 1907. His detachment has been in position since 1907 until this summer, when he got a shallow detachment below again, that is, a recurrence after fourteen years.

Case of Bilateral Proptosis, with Limitation of Movement in One Eye.

By R. AFFLECK GREEVES, F.R.C.S.

PATIENT, a male, aged 37. The proptosis began a year ago, first the right eye, then, six months ago, the left. In August he developed double vision. The condition does not appear to have altered much since, and the double vision, both in the primary position and when looking upwards, troubles him a good deal. At first sight the case appears to be one of Graves' disease, because there is retraction of the eyelids, but there is no other symptom of that disease, such as tachycardia or swelling of the thyroid. He occasionally has, however, a slight tremor of his hands. The movements of the left eye are free in all directions. Upward movement of the right eye is absent, and its inward movement is a little limited. The right eye is turned downwards, and slightly displaced in that direction. X-ray examination gives a negative result. No tumour can be felt in either orbit. Wassermann reaction negative.

Mr. A. L. WHITEHEAD (President) said he thought this was probably a case of Graves' disease.

Tumour of Right Upper Lid (Angeioma).

By J. F. CUNNINGHAM, F.R.C.S.

PATIENT, a male, aged 14. The tumour has been present for six months.

Plastic Operation for Contracted Sockets.

By M. W. B. OLIVER, M.B.

PATIENT, a male, admitted to the Queen's Hospital, Sidcup, on August 10, 1921. Very badly contracted socket-loss of lower lid and partial loss of upper lid. New lower lid made from temporal flap lined by inturned flap from malar region. Partial new upper lid.

Many attempts were made to make a socket to hold an artificial eye by epithelial inlays. After each operation the socket contracted again within a few weeks. Five months ago a very radical operation was performed. The whole of the orbital contents were removed down to the periosteum. A very large inlay was inserted. For two months after this a vulcanite mould attached to a dental splint was worn. There has been no tendency to contract, and a large artificial eye can now be worn.

It is a modification of the old method of treating a contracted socket by an epithelial inlay over a wax mould. The objection to this is, that many of the sockets contract again, whatever one does, and there is a discharge which persists for years. In all these cases I remove the whole of the conjunctiva, including that of the lids themselves. I think such an operation could be adopted in cases of exenteration of the orbit; there is no reason to remove the lids in such cases. And when the cavity is skin-grafted, it is very important to skin-graft the under surface of the lids.

Case of Retinal Degeneration, with Mental Deficiency.

By F. A. JULER, F.R.C.S.

PATIENT, a girl, aged 15. I think this case must be of the nature of retinitis pigmentosa. The patient has definite mental deficiency, and there are other unusual features. The disease seemed first to affect the macular regions early. She came with the history that vision had been very defective for three years. She was first seen about nine months ago ; at that date there were fine changes in both maculæ, and some degeneration in the periphery of the retina—a small pigment change there. The optic discs were pale, with the waxy type of atrophy. She has been coming up at intervals since, and there is some progress in the degeneration ; the pigmentary changes are more marked in the periphery of the retina than they were. There is no family history of blindness. In the patient there is no evidence of syphilis, but the mother gives the history that she has had two other confinements ; in one she had a premature child, and the child of the other confinement died in fourteen days. So there is a possibility of syphilitic defect here, though the patient's Wassermann reaction is negative. She was in the lowest standard at school before her sight became bad, when her age was probably 10 or 11. Now she attends a blind school, but she cannot pick up the work as the other children do. Her mother admits the child is mentally dull. There is no consanguinity in the parents.

Section of Ophthalmology.

President—Mr. A. L. WHITEHEAD, M.B.

Night Blindness : Retinitis Pigmentosa sine Pigmento.

By J. A. VALENTINE, M.D.

W. A. K., AGED 26. Sight failing since 1915. No relatives with similar bad sight. Right vision, $\frac{5}{6}$; left vision, $\frac{6}{12}$. Ring scotomata. Pupils normal and media clear. Discs show indistinct margins. Vessels slightly smaller than normal.

Fundi : (1) Outer zone in periphery shows thinned chorioids. Only large vessels of chorioids remain. (2) Intermediate zones : Some thinning of chorioids. Large veins visible with interspaces filled with dust-like pigment deposits. (3) Macular areas appear normal except that the maculæ themselves are not visible as distinct from rest of foveæ. Small bright specks are seen here and there, apparently on anterior of retinae. In the left fundus one or two small collections of pigment of the "bone corpuscle" variety are seen on the nasal side.

Though the patient complains of night blindness, this is due to his tubular vision, and not to a deficiency in light sense, as his light minimum and light difference are normal at the fixation area, as tested by Percival's rotating discs.

Case of Amaurotic Family Idiocy.

By A. H. LEVY, M.D.

E. D., FEMALE, aged 1 year 7 months. Family history : Parents are first cousins. Father (aged 27) born in London of pure British parentage; grandparents British. Has never had any specific disease or serious illness. Mother (aged 24) of London birth; previous history good. Family antecedents for two generations also non-Jewish. One other child, aged 3, healthy. No miscarriages.

Patient's history : Full term child, normal labour and delivery. Weight at birth, 7 lb. Breast-fed up to the age of 10 months, then with cow's milk and water. Brought to hospital on account of having a "weak back" : could not sit up; there was difficulty in weaning. After the seventh month the child lay in any position in which it was placed, and was very quiet during the day but noisy at night. When 4 months old the patient had a fall, causing "black eyes," but she soon recovered. On September 6, 1922, she was admitted to hospital, weight 11 lb. 10 oz. On admission: Fairly well nourished; looked "vacant"; did not perceive light, but was very sensitive to sudden noises. Lay quiet, and appeared only to eat and sleep: uttered a strange plaintive cry when hungry; could not sit up; head rolled from side to side. Anterior

18 Levy: *Amaurotic Idiocy*; Hine: *Maculo-cerebral Degeneration*

fontanelle widely open. No teeth. Knee-jerks brisk; Kernig's sign indefinite; no clonus. Eyes: No perception of light; discs atrophic, white with clear cut edges. Vessels normal in size. Maculae occupied by white area with circular red spot in centre.

Present condition: The child is much wasted; spasticity increased; is unable to sit up unsupported; cannot take any food except liquids. Auditory acuity marked; four teeth now appearing; fontanelle still widely open.

DISCUSSION.

Mr. LESLIE PATON reminded the Section of a case of this condition which Dr. Wilfred Harris showed in 1913, in which no Jewish parentage could be traced on either side; the family lived in Bayswater. About eighteen months ago an excellent paper was published on the subject by Bielschowsky,¹ which dealt with the changes in detail, and it was interesting to learn to how great an extent the cerebellar cells participated in the change. He pointed out that there were marked differences in the extent of macular cerebral and cerebellar disturbance in different cases.

Mr. TREACHER COLLINS said that Sir Frederick Mott had shown that not only the ganglion cells of the cerebellum and cerebrum, but the ganglion cells all over the body were affected in this disease, even those in the sympathetic system. It was of great interest to see, on one evening, cases illustrating two different forms of retinal degenerative change: amaurotic family idiocy, which began in the ganglion cells of the retina, and pigmentary degeneration at the macula, which began in the cones. Often these two conditions were confused, but to-night they had the opportunity of comparing the difference in the ophthalmoscopic appearances which they presented.

Two Cases of Early Familial Maculo-cerebral Degeneration.

By MONTAGUE L. HINE, M.D.

Case I.—C. C., MALE, aged 16. I first saw this, the elder boy, three years ago. He commenced having epileptic fits when he was 10 years of age, and he was treated in the ordinary way with bromides at London Hospital. His vision was defective, and he went to another hospital, where he was supplied with glasses. As he still could not see, even though his glasses were correct, he came to me at the Miller Hospital for examination. The maculae were very finely granular, and I took him to the Royal Westminster Ophthalmic Hospital, as I thought he was suffering from maculo-cerebral degeneration. Dr. Gordon Holmes confirmed that view, and the patient went into Queen Square Hospital for some time. The Wassermann reaction was negative, and there was nothing in the family history to suggest syphilis; there is no parental consanguinity. He has been going downhill, and to-night his cerebration is very sluggish, he is almost paralysed, and there is incontinence of urine, but not yet of faeces.

Case II.—A. C., male, aged 10. Twelve months ago the younger brother of C. C. was brought to see me, not because there was anything the matter with him, but because the parents wanted to know if there might be. His vision was found to be $\frac{1}{12}$ in each eye, and it could not be improved by glasses. He had a very small refractive error, and both his maculae were finely granular, the left rather more so than the right. I then said—though not every one who saw him agreed—

¹ M. Bielschowsky, "Zur Histopathologie und Pathogenese der Amaurotischen Idiotie"; *Jour. f. Psychol. u. Neurol.*, Leipzig, xxvi, 123-190.

that the state of his fundus was very suspicious, in view of the family history and the non-improvement in the vision. I sent him to Dr. Gordon Holmes, who found no changes in the central nervous system, but on October 10 last he had an epileptic fit (so called). He was then the same age as his brother was when his fits had begun. Next day he was brought to hospital, and his fundus was exactly as you see it now, the same in each eye, with very fine granular pigmentation of the right macula, rather more marked in the left macula. Since that time he has had three additional fits, and there can be no doubt that he is going the same way as his brother. There is another brother, now aged 18, who, the mother says, is quite well, and has no trouble with his sight. I have not yet been able to see the other children. There is one brother, between the patients in age, who is not affected; also two sisters and one brother, younger than A. C., who are not affected.

Dr. RAYNER BATTEN agreed that these cases were definitely of the cerebro-macular type; but the pigmentation was not so marked as in the other cases of the condition he had seen. Here it was a fine granular pigment. He thought the present cases were of the juvenile familial type, not the infantile; he considered there was a distinction between the two. It was very remarkable how, in the juvenile type, the disease came on at a particular age, i.e., somewhat bordering on the age of puberty. There was another class also in which pigmentation occurred, but without the cerebral symptoms. That formed the second class, in the juvenile sense.

Case for Diagnosis (? Polycythæmia Rubra).

By PERCY BARDSLEY, M.B.

THIS lad came to me complaining of great fatigue. He is a theological student, and he complained that he could not work more than four or five hours a day. At the time when he was supposed to play games with the other students, he usually went to bed, and he had the greatest difficulty in getting up in the morning. He was conscious of mental as well as of bodily lassitude. I corrected his refraction error, which was very slight, but in a fortnight he returned and said he had double vision. I then examined him more carefully. His medical man had been treating him as a neurotic subject. I was struck by the condition of both his veins and his arteries, and particularly by the brightness of the veins. I wrote and asked his medical man to have a blood examination made. This was done, and it was found he had 7,600,000 red cells, a very slight increase in hæmoglobin, and a slightly diminished colour index. I have noted down the condition as "? polycythæmia rubra." I have read through Dr. Parkes Weber's book,¹ and I cannot see that this agrees with his cases in all particulars; the patient is younger than any mentioned in that book, with the exception of three girls, and in all of them there was a very abnormal menstrual history, connected mostly with a tubercular uterus.

This patient has no enlargement of the spleen or liver, so far as his medical man can detect; he is not aware of having had syphilis, and his Wassermann reaction is negative. I think it possible he has always suffered from a congenital excess of bone-marrow. He says that from his earliest days he has been subject to periods of great lassitude, and that these have always been followed by severe diarrhœa, after the cessation of which he has been better for a time. He was in the Army three years, and during that time his health

¹ "Polycythæmia, Erythrocytosis and Erythræmia (Vaquez-Osler Disease)," 1921.

appears to have been normal; he was passed as A.1. But on returning to Salisbury he again experienced this lassitude and lack of energy. A physician diagnosed that he was suffering from some form of colitis, and he has been treating the patient for this; the patient certainly seems better, though the condition of his blood has not altered. If it is polycythæmia, I am afraid the prognosis is bad, but I shall be glad of information as to the nature of the case.

At the last meeting the treatment recommended to me was frequent venesection, even up to 30 or 40 oz. to be carried out every few months. This seems drastic, but, according to Parkes Weber's book, it is the only treatment which has produced gratifying results.

DISCUSSION.

Mr. A. L. WHITEHEAD (President) said he considered that the veins of this patient were abnormally distended, and that the whole fundus and disc were hyperæmic, though he could not say that the degree was in excess of ordinary physiological limits. He did not detect any hæmorrhages. Had venesection been done since the last meeting?

Mr. TREACHER COLLINS asked whether there had been fundus changes in any other cases of the kind which had been recorded.

Mr. BARDSLEY (in reply) said that no treatment had been adopted since the last meeting, as the patient had been on his holidays, and had only returned in time to show himself at this meeting. In Dr. Parkes Weber's book the ophthalmoscopic appearances in a few cases were described, and the extreme distension of the veins and their bright blue colour were commented upon.

Case of Hole in the Hyaloid.

By R. LINDSAY REA, M.D., F.R.C.S.

THIS case came to my notice three years ago. Vitreous opacities were present so that I could not make out the fundus details. I watched the case closely at three or four weeks' intervals. Sometimes I could see the fundus; next day it would not be visible. The Wassermann reaction of the blood was strongly positive, so I had the patient placed under proper treatment; injections of novarsenobillon were given, followed by mercury and iodides. The vitreous bodies then began to clear, and when I could see the left fundus I found a ring in front of the optic disc. I sent the case to Sir William Lister who agreed it was a true case of "hole in the hyaloid." During the last year I noticed that the ring was shrinking in size and that it was anchored.

In his paper on "Detachment of the Vitreous" which Sir William Lister sent to the Washington Ophthalmological Congress and communicated by Mr. Treacher Collins, the statement is made that the vitreous body is attached most strongly around the ora serrata. Professor Johnson Symington, in Quain's "Anatomy," vol. iii, pt. ii, quotes Iwanhoff and Stuart as supporting this view, but Symington states on page 253 that the vitreous can be readily separated from the retina except behind, at the entrance of the optic nerve, where the connexion is closer, the retinal vessels having here entered it in foetal life.

The only explanation I can think of in the case is, that owing to shrinkage of the diseased vitreous, the attachment to the optic nerve, which, normally

not visible, was made visible and showed as a ring of connective tissue, was dragged away from the optic disc. There is no doubt of its stationary character for during the past year it has not altered its position.

DISCUSSION.

Mr. TREACHER COLLINS said he had always been very sceptical about the occurrence of holes in the hyaloid, until he came across this present case, which was a very definite instance of the condition. There could be very little doubt that in it the vitreous had become torn away from the optic disc, and at the spot where it had been attached a circular hole had been left. Sir William Lister had a very beautiful drawing made of the case, which was shown in connexion with his paper at the Washington Congress, and was published in its *Transactions*.

Mr. GIMBLETT said that whilst he was seeing cases in the examination room of the Ministry of Pensions he saw a case with a similar appearance in the vitreous, but in that case the ring was four times the diameter of that in the case now shown. It could be seen with a + 6 D. lens. Two days later he saw another case of the same kind, but had not been able to trace the second one since. In the light of the explanation given that evening, the ring seemed to have been, in those two cases, exactly the size it would have been if it had come away from the optic disc behind.

Mr. LINDSAY REA also showed a "Case of Papilloedema with Detached Retina in each Eye, in a Young Woman, aged 22."

Some Unusual Results of Operations for Cataract.

By T. HARRISON BUTLER, M.D.

Case I: An Example of Total Aniridia following a Preliminary Iridectomy.—This must be a very rare accident, and I have never seen it before. When I have a reliable assistant I adopt the old-fashioned plan of allowing him to cut the iris. In this case, that of a woman, aged 74, I made an incision with a narrow bent broad needle and withdrew the iris. My assistant attempted to cut the iris, but the de Wecker's scissors failed to cut. While he took up another pair from the tray I held the iris quietly in the forceps, and the section was made cleanly. During the time that the iris was held prolapsed there was no indication of any detachment and no hæmorrhage. The patient did not move. As soon as the iris was cut the whole anterior chamber filled with blood and I saw nothing more. I found that the systolic blood-pressure was 200 mm. of mercury which appeared to account for the hæmorrhage. A month later the anterior chamber was still full of blood and the tension of the eye was somewhat raised. Two months after the operation the blood had become absorbed, and I noted that the iris was totally detached from the periphery and lay rolled up in the vertical diameter of the anterior chamber. The lens was exposed and was seen to be very large. I did not extract the lens but removed the cataract from the second eye with favourable result.

It is difficult to account for this accident. No unusual traction was made upon the iris: I wonder whether the fact that the iris was held prolapsed for several seconds can in any way account for the catastrophe.

22 Butler: *Some Unusual Results of Operations for Cataract*

Case II.—This is one of mistaken diagnosis, but at first it appeared to be an example of clearing of a cataractous lens. It is of interest because it offers an explanation of cases which, if not reported in responsible journals, have been at any rate talked about. It is questionable whether a real cataract can ever clear without absorption. It is said that a diabetic cataract may again become clear, and that a traumatic posterior capsular cataract may disappear.

In this case, the woman, aged 71, came to the Coventry Hospital in April, 1921. She was examined by Dr. Brazil, my assistant, there. He made drawings of the lenses, and noted complete cataract in the right eye and lens striæ in the left. There was a history that the sight had slowly failed during the past year. We have no note of any injection or pain. I made a note shortly afterwards that there was general deep-seated opacity of a brown colour in the right lens. The right fundus was invisible. There were striæ in the left lens and here the fundus was visible and normal. The acuity of the right eye was reduced to hand movements.

On May 25, 1921, preliminary iridectomy was performed upon the right eye. The operation was not followed by any reaction. Two months later there was still some red reflex, but no fundus details could be seen. At this time I still had no idea that I was not faced with an ordinary opacity deep in the lens. On November 7 I made a note: Right fundus dimly seen, but I had no suspicion of the true state of affairs.

I did not see the patient for another year, but she reappeared on October 16, 1922. I found that the fundus was clearly visible and that with a suitable correction the acuity was $\frac{1}{8}$ and J 1! I was now able to examine the fundus with care, and I found floating opacities in each vitreous. In the right eye there was well developed arterio-sclerosis, and one of the sclerosed arteries became quite white as it neared the "north-west" periphery. Here I found several silver wire twigs and marked sclerosis of the choroidal vessels with punctate hæmorrhages.

I think there is no doubt that the original condition was not cataract but a hæmorrhage in the anterior portion of the vitreous which had slowly become absorbed.

Case III.—*One of Total Absorption of a Cataractous Lens in a man aged 46.* A preliminary iridectomy was performed on April 28, 1921, by my house surgeon. I watched very carefully and I am certain that the keratome did not touch the lens, nor do I think that the iris forceps could have done any damage. There was considerable reaction after the operation, and eventually some keratitis punctata was seen. I had a blood count made and this showed an increase in the lymphocyte count (large 20 per cent., small 20 per cent.), but none in the large hyalines. For some time I was anxious with regard to sympathetic ophthalmia, but decided to watch. A fortnight after the operation I noted that there was a notch in the upper aspect of the lens. The eye remained injected for five months; after that time the inflammation subsided. The notch in the lens began to increase rapidly and at the end of six months the whole lens had absorbed, leaving a perfectly clear view of the fundus. With a plus 10 lens an acuity of $\frac{6}{12}$ was obtained.

Case IV.—Some years ago I did a preliminary iridectomy upon a private patient. A small iris encleisis was left. When I came to perform the extraction I made a point of trying to clear away this iris attachment. The wound appeared to heal soundly and the patient returned home with an acuity of $\frac{5}{8}$. She returned in two months showing a large iris prolapse. She said that a dark bleb formed

on the eye which eventually burst and the sight went. I excised this prolapse and covered the site with a conjunctival flap. The wound healed again firmly with no sign of prolapse but the tension remained low and the acuity did not return. She returned home, and I heard no more about her for seven months when I was told that she had suddenly developed acute glaucoma, and had to have an operation to relieve it. This case is interesting for two reasons. It shows what I have noted in other cases, that a very small iris encleisis may begin to bore its way out and grow to a large prolapse, and that an eye which has for some time showed low tension from a filtering scar may, if the scar cease to filter, suddenly develop an acute glaucoma. I had an example in a man whose cataract wound refused to heal for a fortnight. It eventually closed and an acute glaucoma developed which was cured by expectant treatment.

My next two cases deal with *Excessive Post-operative Astigmatism*.

Case V.—I extracted the lens of a man, aged 65, in March, 1920, and fitted him up with a plus 10 sphere and plus 2 cylinder. He now had a severe attack of pneumonia and was nine weeks in hospital. In October with plus 10 and plus 2.5 the acuity was $\frac{6}{32}$. The following January he took a 6 sphere and a 6 cylinder, and in March a 4 sphere and a 10 cylinder, with an acuity of $\frac{6}{12}$. The second lens was removed, and he was given the usual 10 and 2.

This is an example of a gradually increasing astigmatism at the expense of the sphere. I have never noted this before, whereas the reverse change is very common. Placido's disc showed distinctly oval rings, long axis vertical.

Case VI.—Another case, that of a lady, aged only 50, shows that very powerful cylinders can be worn with comfort, and that they give good acuity. I extracted the right lens, and had a good deal of trouble during convalescence from hæmorrhage into the anterior segment of the eye, which came on the third day. At one time I feared that the eye would be lost, but happily all cleared up, and there has been no further trouble. With plus 8 sphere and plus 11 cylinder the acuity is $\frac{6}{9}$. This was reduced eventually to plus 11 and plus 9.

I extracted the second lens under a conjunctival bridge, and the final refraction was plus 8 sphere and plus 14 cylinder. This correction gives $\frac{6}{9}$ acuity. My optician tells me that his firm have never ground a cylinder as high as this. Retinoscopy gave 25 in the vertical and 10 in the horizontal meridian. The Placido figure was at first an oblong with rounded corners, but now it is a perfect ellipse. I make a practice of examining all my extractions with Placido's rings, which often afford valuable information regarding the shape of the cornea.

Case VII.—My final case—the patient a female, aged 50—is one in which a mild sympathetic ophthalmitis took the form of a subacute glaucoma. After the extraction a chronic irido-cyclitis set in, and I wished to remove the eye. This was refused, and one of my colleagues thought that I might wait and watch the case. Suddenly in one night the second eye became very painful and completely blind. I found that the tension was raised to 60 Schiötz units. I at once removed the eye that I had operated upon, and began a course of massive doses of salicylate of soda and injections of novarsenobillon. Fine keratitis punctata developed. I tried atropine and

24 Butler: *Operations for Cataract; Killick: Conical Cornea*

then eserine, but neither seemed to influence the tension, which gradually settled down, but was not normal for several months. The sight gradually returned, and is now $\frac{1}{2}$. There is sufficient lens opacity to account for this lack of full acuity.

I have seen other cases in which sympathetic ophthalmia of a mild type took the form of a semi-acute glaucoma with minimal signs of inflammation.

DISCUSSION.

Mr. A. L. WHITEHEAD (President) said that Mr. Harrison Butler had courageously brought forward accounts of cases such as probably most members had had, at some time or other, and it was very instructive to hear of instances in the experience of others. In regard to the case which was operated upon by the house surgeon, he asked whether there had been damage to the lens or not. It ended with $\frac{1}{2}$ vision; was that after the lens had undergone absorption? The point had been raised as to prolapse of the iris occurring some time after operation. He (the President) had seen that twice: a very minute piece of iris had been caught in the scar, and was represented by a very minute black dot; some months later a definite prolapse appeared. With regard to the extraordinarily high degrees of astigmatism, he asked whether these were all cases in which the conjunctival bridge operation had been done?

Mr. TREACHER COLLINS said that with regard to the cases Mr. Harrison Butler had just related, he agreed that there must have been vitreous hæmorrhage in the case in which a preliminary iridectomy was done. It was said that diabetic cataracts sometimes cleared up; he (Mr. Treacher Collins) had not known them do so; but as so accurate an observer as Mr. Nettleship had reported such a clearance it must be regarded as a possibility. He (the speaker) did not think senile cataract ever cleared up, though the fluid in a Morganian cataract might sometimes diffuse through the capsule. He agreed that a small black point at one angle of the scar, due to the entanglement of a knuckle of iris tissue at the angle of the coloboma, might after some weeks or months become a bulging cicatrix. He had known this happen. Mr. Treacher Collins thought that astigmatism after cataract extraction, which progressed in the way described was usually due to failure in complete union of the posterior lips of the wound from entanglement of lens capsule.

Mr. HARRISON BUTLER (in reply) said that in the case operated upon by the house-surgeon the lens was completely absorbed; he did not think that it was damaged during the operation. In the high astigmatism cases, one eye in each patient was operated on by the bridge method, the other was not.

The Treatment of Conical Cornea.

By CHARLES KILLICK, M.D.

(ABSTRACT.)

THE author remarked that slight and early cases of keratoconus were seldom seen and possibly overlooked. His own experience had been of late cases in which the disease was advanced, and something had to be done by way of operation. After stating that keratoconus might occasionally be unilateral, he quoted a series of six cases recently under his care. Five of these were in women, including two pairs of sisters. One female patient was an

idiot and the solitary male was a young and healthy adult. The disease was probably developmental in the two latter cases, in the others it had shown a tendency to appear in more than one member of the family. After briefly alluding to the pathology and theories of causation the treatment was considered. In two cases a pressure bandage was applied where the apex of the cone had become opaque, with the result of almost completely restoring normal transparency but, as might be expected, without altering the shape of the cone in any way. In one other case six operations upon the right eye were performed, without the slightest benefit. These were in order: (1) Sclerectomy—interval of eight months; (2) first stage of cataract extraction—interval of one month; (3) cauterization with perforation—interval of one month; (4) cauterization with perforation—interval of seven weeks; (5) cauterization only, over rather larger area—interval of seven weeks; (6) cauterization with perforation. The final result was a small buttonhole iridectomy due to the first operation and an insignificant corneal macula. The patient subsequently developed phthisis.

In a second case, to which Mr. Killick particularly wished to draw attention, the patient, aged 49, suffered from double keratoconus, and from a few peripheral opaque striæ in both lenses. Vision was less than $\frac{6}{80}$. The treatment he adopted was: (1) Expectant for a year—no improvement; (2) simple extraction of right lens, by subconjunctival method; (3) small vertical discission of the capsule, based on the old operation of iridodesis, namely, that of making a narrow linear opening in the pupil, resembling a stenopaic slit; (4) correction of refractive error subsequent to operation by -5.00 sph. together with -1.00 cyl. ax. 180° . Vision in the foregoing case was improved to $\frac{6}{24}$ and the correcting lens indicates an antecedent myopia of 32 diopters. The author suggested that the above was the operation of choice in suitable cases and was preferable to any operation involving burning of the cornea. He claimed that it was: (1) easy to perform; (2) involved no disfigurement; (3) could be carried out in a reasonable time; (4) definitely improved vision.

DISCUSSION.

Mr. A. L. WHITEHEAD (President) said he had seen the second case which Mr. Killick had described, at Bradford, and the result was certainly striking and satisfactory; so also was the pupil which was left; it had dilated a little and was oval, instead of being a linear slit. One knew these patients saw well through a small opening, as well as through a slit; and if one was fortunate enough to have a dense homogeneous capsule, it might be sufficient to make a small hole only.

Mr. ELMORE BREWERTON said he had been disappointed with the older methods of dealing with conical cornea. He had had some good results with the cautery, but he did not care to attempt the removal of an elliptical piece with many weeks' delay in healing. He believed that most exponents of this method had given it up. The method he now adopted was to make a crucial incision through the apex of the cone, the first incision being made with a von Graefe knife and the two lateral incisions with scissors. If the apex of the cone was below the centre the first incision should be downwards and in at an angle of 45° to the vertical and about 6 mm. in length; the two lateral incisions on either side of the centre of the first and at right angles to it, and each about 3 mm. in length. The wound healed in three or four days; there resulted a certain amount of flattening and a blurred apex to the cone. The operation was advisable for advanced cases of keratoconus; he would not hesitate to do it on any case in which the cone was increasing. He had operated on five such eyes, and in all had

secured improvement in vision. He thought it was better to deal with the cornea rather than with the lens, in the complicated way described by Mr. Killick.

Mr. KILLICK (in reply) said he agreed that the smaller the opening made in the capsule the better, and in the next case he had he would make it as small as possible. The method of dealing with conical cornea described by Mr. Brewerton had not occurred to him; any method which would supersede the ordinary burning method would yield better results in the long run.

Section of Ophthalmology.¹

President—Mr. A. L. WHITEHEAD, M.B.

Optic Atrophy after Herpes Ophthalmicus.

By LESLIE PATON, F.R.C.S.

MRS. W., aged 67. Sent to St. Mary's Hospital, by Dr. Vincent, of Strood, Kent, in the beginning of December, 1922. The history was that at Easter, 1922, she had had a bad attack of ophthalmic herpes affecting all the branches of the first division of the fifth nerve on the left side. There had been several vesicles on the cornea, which have left faint nebulæ. On recovery from the herpes she found that the vision of her left eye was lost.

When first seen: Right vision: $\frac{+3D. sph.}{+1.5D. cyl. ax. 100} = \frac{6}{8}$. Left vision: Hand movements. Field evidently very limited. Left pupil does not react to direct stimulus, but reacts well on consensual stimulus. Reacts slightly to concentrated light. The ophthalmoscope showed an opaque white disc, with fairly clean-cut edges. Lamina cribrosa not seen. No obvious disturbance of retinal pigment round edges of disc. Vessels reduced in calibre.

The occurrence of true optic neuritis as a complication of ophthalmic herpes is comparatively rare. Personally, I had never seen a case with this complication until last summer when, through the courtesy of Mr. Adams, of Oxford, I saw a private case of his, a lady, aged 35. She had had shingles in the right side commencing on May 15. The whole first division of the fifth nerve was affected. When the eye was opened again after the attack of herpes had subsided, it was found that she had gone completely blind in that eye. The pupil was semidilated and not reacting to light at all. When I saw her, she was still slightly under the influence of atropine so I was not able to confirm the question of pupil reaction. Her tension was quite normal. There was no perception of light. Ophthalmoscopic examination showed an entirely atrophic disc. I understand that there has been absolutely no recovery of vision in this case of Mr. Adams.

The occurrence of these two cases in my experience within a few months of one another led me to look through the literature dealing with this subject. I found that quite a number of such cases have already been recorded. Sir Jonathan Hutchinson, in his classical account of the condition, when he first clearly differentiated it, showed its nature and described its clinical history. He gives one case of blindness resulting from an attack of ophthalmic

¹ Clinical Meeting, held at the Royal London Ophthalmic Hospital (Moorfields), February 9, 1923.

herpes.¹ He lays stress on the fact that the globe is never affected except in cases where the nasociliary branch of the first division of the fifth nerve is affected. This, known as "Hutchinson's law" is, however, like so many laws, subject to exception. The following is Hutchinson's own statement of the law²: "All the cases I have yet seen support the opinion I have expressed that it is only when the side of the nose is affected that any serious inflammation of the eye ensues and that in the worst cases the vesicles will be found on the very tip of the nose, the part supplied by the oculo-nasal nerve." Personally, I have found that, though Hutchinson's law may be subject to some exceptions, it is of very great service in prognosis, and I look first of all to see whether there is evidence of invasion of the side of the nose by vesicles, and if it is free, I feel more confident in my prognosis as regards ocular complications.

Some time in the year 1867, Bowman³ reported another case, left herpes with optic atrophy, and in the same volume, Hulke⁴ records a case in which optic neuritis preceded by a few days the development of herpes, but in these cases the herpes was probably secondary in character and not a primary acute infective herpes.

The next case that I can find is a case of Dagenet, recorded as "*Zona ophtalmique avec névrite optique du côté correspondant.*"⁵ In this case during the acute stages vision was reduced to bare perception of light, but four weeks later, the vision was recorded as being one-sixth. Gould in 1888 recorded a case in an American publication (*The Polyclinic*), but I have not been able to see a copy of it. I cannot find any further record until a case recorded by Wangler in an inaugural dissertation at Zürich, in 1889, but, unfortunately, I have not been able to obtain access to a copy of this.

In 1893, Haltenhoff,⁶ recorded a case in which there was hæmorrhagic retinitis, associated with herpes. From the description of the case it seems almost certain to my mind that there had been a thrombosis of the central vein of the retina, because the hæmorrhages were most profuse, involving the whole retina except the macular region. The veins were very dilated and tortuous, and obscured in numerous places by hæmorrhages, and the arteries were reduced in calibre. There was, however, very little evidence of any swelling of the disc.

Sulzer, in 1898,⁷ recorded a case of double optic neuritis, more severe in the right than in the left eye, with right herpes. The fact that there were three recurrences points rather to the conclusion that this was not an ordinary case of primary herpes but rather a herpes associated with some other intracranial condition. As Sir Jonathan Hutchinson points out, in true herpes there is no liability to a recurrence of the disease. In cases where recurrences are frequent, it is more likely that these are what I should term "secondary herpes."

Antonelli in 1902 recorded another case and at a meeting of the American Ophthalmological Society in June, 1919, Dr. Clarence Veasey recorded a case

¹ *Ophthal. Hosp. Repts.*, 1866, v, p. 191.

² *Ibid.*, 1869, vi, p. 48.

³ *Ibid.*, 1869, vi, p. 1.

⁴ *Ibid.*, 1869, vi, p. 105.

⁵ *Recueil Opht.*, April, 1877.

⁶ *Annales d'Oculistique*, 1893, cix, p. 201.

⁷ *Ibid.*, 1898, cxix, p. 401.

in which on the seventh day of an attack of right ophthalmic herpes vision was lost in the right eye except for a small portion in the upper field, the loss beginning as a central scotoma and gradually spreading without evidence of ophthalmic changes or of corneal involvement. A month later some vision was restored in the peripheral field and the patient could count fingers at 2 ft. The first nerve was quite pale and atrophic and subsequently all vision was lost. In this case, then, the neuritis was evidently retrobulbar.

Evidence of other cranial nerve involvements is much more frequently found. I have had myself three or four cases of diplopia during the epidemic this last spring, and in the summer I saw one case in which the sixth nerve was involved, though undoubtedly, from the statistics, the involvement of the third nerve is very much more frequent. Some involvement of the nerve supply of the iris and ciliary muscle is also not uncommon, though it is more usual to find a very small pupil than a semidilated pupil. I have at present under my care a patient with a most severe attack of ophthalmic herpes with no portion of healthy skin on which you could put down even a pin head. Her cornea has so far remained quite clear. There does not seem to be any more ptosis than could be explained by the drooping of a swollen lid but she has an extreme degree of miosis in that eye.

I shall not refer to the other complications, though I have at present a very interesting case in which there was no superficial vesicle on the cornea but a well-marked deep keratitis with irido-cyclitis. I have seen definite hypotony in connexion with this last year's epidemic and this is especially interesting as numerous speakers at the American ophthalmic meeting in 1919 referred to an association of raised tension with herpes. Undoubtedly the majority of the cases in which the tension is raised are cases in which the iris and the ciliary body are involved in the congestion. In the majority of cases it is the supra-orbital and the supratrochlear branches of the first division of the fifth nerve that are affected. The nasociliary and the lacrymal division are not so frequently affected, but I have seen within the last six weeks a case in which the whole of the first division and the whole of the second division of the fifth nerve were affected, and Letulle, in his article on paralysis of the facial in ophthalmic herpes,¹ gives a case of ophthalmic herpes associated with paralysis of the seventh.

So far as the evidence goes, I think the indication is that the changes in the optic nerve are secondary to the peripheral changes in the circulation produced as the result of the herpes.

I have once or twice spoken of herpes as being secondary or primary. I think it is most important that we should be very careful to differentiate those cases in which the herpes occurs as the result of the implication of the Gasserian ganglion, or of the branches of the fifth nerve, by any other associated intracranial condition, from the true cases of herpes ophthalmicus, due probably to an infection of the Gasserian ganglion.

As regards the relationship of this infection to chicken-pox, we are very much in need of further information. There is an increasing body of evidence in favour of the association of epidemics of herpes with epidemics of chicken-pox. It may be within the recollection of some of you that some years ago I showed a case of optic atrophy resulting from retro-bulbar neuritis during chicken-pox,² and I have been able to trace in several cases a definite relation-

¹ *Archives de Physiol.*, 1882, ix, p. 162.

² *Proceedings*, 1917-18, xi (Sect. Ophth.), p. 12.

30 Ford: *Intracranial Tumour causing Quadrantic Hemiopia*

ship between cases of ophthalmic herpes developing in adults with cases of chicken-pox in children.

DISCUSSION.

Mr. RANSOM PICKARD (Exeter) said that some years ago he was called to see the most extreme case of herpes ophthalmicus he had ever observed. In that case the whole left side of the forehead was sloughing, and the patient was completely blind in that eye. The condition of the cornea did not allow him to inspect the state of the nerve, and he did not see the patient any more; the doctor, however, told him the blindness was complete and persistent. With regard to the relationship between chicken-pox and herpes, he had a most interesting case under his care for some time, that of a child, who had had chicken-pox, and with it a persistent paralysis of the pupil. The paralysis persisted for at least a year. In the South-west of England it was common knowledge among the people that there was an association between small-pox and herpes.

Mr. W. H. McMULLEN said that he had at present under care a case of optic atrophy following herpes. He did not see the patient while he was suffering from the herpes. The history was supplied by the doctor who attended him. In July last the patient had very acute right herpes ophthalmicus, and the eye was very congested and painful. There was also severe constitutional disturbance, for he was delirious during two or three nights. By September, when he (Mr. McMullen) first saw the patient, the affected eye had only perception of light. There were a few spots of keratitis punctata, which were probably fairly old, as many of them were brown, and the disc was somewhat pale. Since then the disc had become paler, and now was white, though not of such an opaque white as in the case Mr. Paton had just shown. The appearance was more that of a primary optic atrophy, and the difference in the appearance could probably be explained by assuming that the retro-bulbar neuritis affected the nerve further back in the eye, and the optic disc itself was less affected.

Mr. G. M. KENDALL, speaking of the connexion between herpes and chicken-pox, said that when he was a child he had chicken-pox, and a few weeks previously to that his mother had what he now knew to have been herpes ophthalmicus.

Mr. FRANK JULER, F.R.C.S., showed a Case of Cicatrization of the Retina.

Intracranial Tumour causing Quadrantic Hemiopia.

By ROSA FORD, M.B.

THE patient, a female, aged 35, was first seen on October 17, 1922, on account of "muddled" vision for six weeks and headache. There was one hæmorrhage on the disc in the right eye, and the edge of the left disc was blurred. Papillœdema of both discs shortly developed. There was only some general contraction of the fields for white, but the colour fields showed right quadrantic hemiopia. Later, the fields for white also showed this. The macula was spared (for white and red). Vision $\frac{6}{8}$ both eyes. There were slight coarse nystagmoid movements on looking to the extreme right. The Wassermann was negative both for the blood and the cerebro-spinal fluid. There was no history of any injury to the head. In February, 1923, the papillœdema became more marked, especially in the right eye. Vision: Right, $\frac{6}{12}$; left, $\frac{6}{8}$;

Section of Ophthalmology

and attacks of "feeling faint" were frequent. She was therefore transferred to the care of Mr. Percy Sargent, who admitted her into the National Hospital, Queen Square, with a view to operation. There has been no vomiting throughout, and headache has not lately been particularly marked, except at times. No other definite localizing signs developed, and there were no evidences of disturbance in the pituitary region or of word or mind blindness.

Later Note.—Mr. Sargent operated on March 2, 1923, and removed an endothelioma, about the size of a hen's egg, which was attached to the falx cerebri and pressing into the left occipital lobe. The patient has made an excellent recovery.

Mr. J. B. LAWFOORD, F.R.C.S., and Mr. H. NEAME, F.R.C.S., showed a specimen of Bilateral Tuberculosis of Choroid with Detachment of Retina, in a Kitten.

[This communication will be published in the *British Journal of Ophthalmology*.]

Case of Subhyaloid Hæmorrhage in a Girl.

By M. S. MAYOU, F.R.C.S.

(Demonstrated by his house surgeon.)

Discussed by Mr. A. L. WHITEHEAD (President) and by Mr. RANSOM PICKARD.

Two Cases of Primary Band-shaped Opacity of both Corneæ.

By A. C. HUDSON, F.R.C.S.

Case I.—E. R., aged 78, coach painter, until twenty years ago; had lead colic thirty years ago. August 22, 1922: Right vision, $\frac{6}{36}$; left vision, less than $\frac{6}{36}$. Vision defective in the left eye three and a half, in the right eye one and a half years. August 30, 1922: Opacity scraped from left cornea. December 6, 1922: Opacity scraped from right cornea. At present: Right vision, $\frac{6}{36}$; left vision, $\frac{6}{18}$ partly.

Case II.—H. Z., aged 64. No illnesses except rheumatic fever twenty-five years ago. Sight of each eye failing two years. Band-shaped opacity in cornea of each eye. Thin horizontal brown line in opacity in left eye. Right vision, $\frac{6}{18}$; left vision, $\frac{6}{18}$ partly.

These cases are of special interest on account of the rarity of the condition. As far as I know, no satisfactory explanation of its causation has been given; and in neither of these cases is there anything pointing to the cause.

Points of practical interest arise in regard to operation and its results. In one of the patients I operated by scraping away the film, and the result was very satisfactory. The sight in the left eye, which before operation was less than $\frac{6}{36}$, is now $\frac{6}{18}$ in part. The eyes stood the operation very well; there was practically no reaction. The instrument I found most satisfactory for removing the film was the ordinary sharp corneal spud. It was difficult to make a start, as at first the instrument skated over the rigid surface of the opacity; but when the film had been perforated it could be removed fairly easily, without much damage to the underlying parts.

The main constituent of the opacity was examined in the clinical laboratory of St. Thomas's Hospital, and no definite decision as to its nature was reached except that it was not definitely calcareous. I received the impression that the opacity was *in* Bowman's membrane.

DISCUSSION.

Mr. LESLIE PATON said that Mr. Hudson's remarks largely bore out his own experience in a similar case, namely, the very tolerant way in which these patients bore scraping. An old gentleman, of whose case he had experience, was remarkably intolerant of atropine, and he had band-shaped opacities. Fortunately they did not obstruct the pupils, and he had $\frac{5}{8}$ vision in each eye. The only reason he (Mr. Paton) had for scraping him was, that periodically a calcareous nodule formed in the opacity, in one eye only. This acted like a grit, and the patient came with his eye streaming with water. After each scraping the cornea healed in twenty-four to forty-eight hours, and there was left a clean bright piece of cornea. At the end of six scrapings, there were areas of perfectly clear cornea scattered over the band of opacity.

Mr. A. L. WHITEHEAD (President) said that in the second case the opacity seemed to him to be of a much deeper and more infiltrating character than the other, and he wondered whether that case would lend itself so well to scraping as the other. It seemed almost certain that Bowman's membrane was involved. It was well worth while to try scraping.

Mr. AFFLECK GREEVES said he had a similar case to that just related by Mr. Paton, that of an old lady aged 80, who had a corneal opacity in both eyes, below the centre. Periodically the opacity in one eye automatically broke up into separate spicules. She then suffered most intense pain, which was completely relieved by scraping the surface. But the opaque surface layer gradually formed again over the area which was left clear by the scraping. Vision was good in both eyes, because the opacities were situated below the centres of the corneæ.

Atrophic Patches at the Macula ; ? Tuberculous ; ? Cyst.

By F. A. WILLIAMSON-NOBLE, F.R.C.S.

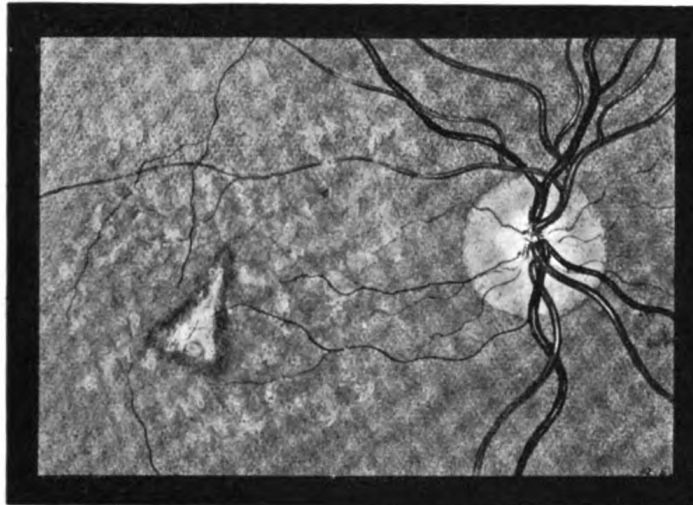
PATIENT, a married woman, aged 36, came to St. Mary's Hospital with a long history of various operations for tuberculous disease. She noticed that vision in the left eye had been getting bad since 1920, and it is now less than $\frac{5}{20}$; in the other eye also the vision is now as bad. In the right eye (*see* figure, p. 33) one can see the internal limiting membrane pushed forward 3D. Evidence of that is, that vessels can be traced coming from the disc; one can follow the vessel with the ophthalmoscope and get the parallax on it, showing it is in front of the rest of the fundus. There are also fine lines which suggest that there are a few wrinkles in the internal limiting membrane. In the lowest part of the patch there is an impairment suggesting that there is a hole in the membrane, but we were not certain of the existence of such a hole.

DISCUSSION.

Mr. LESLIE PATON said he could not offer an explanation of the appearance presented in this case. As had been suggested, it might be a thin-walled cyst in that area. He (Mr. Paton) thought the fibres of Müller had been ruptured and the membrana limitans interna lifted forward. He had specimens showing such an action in the course of papilloedema. In effect, of course, that would be a cyst in the substance of the retina.

Mr. R. AFFLECK GREEVES said he made the suggestion referred to by Mr. Paton because he had seen appearances in sections suggesting this condition, viz., large cysts at the macula, which had apparently begun to form in the outer molecular layer, and which seemed to have become distended, and had caused thinning of the elements of the retina. The walls of the cysts consisted of neuroglia.

Mr. FRANK JULER said the curious membrane on the front of the retina might be a new membrane; it was a fairly common event to see a new membrane in degenerate eyes, microscopically, probably of endothelial origin, in the neighbourhood of the disc.



Mr. COLLEY showed the following Series of Cases illustrating various Congenital Defects: (1) Coloboma of Lens, (2) Coloboma of Iris, (3) Coloboma of Choroid, (4) Persistent Pupillary Membrane, (5) Coloboma of Disc, with Maculo-choroidal Changes, (6) Coloboma of Iris, (7) Persistent Pupillary Membrane, (8) Hyaloid Bodies on Optic Disc, (9) Microphthalmos.

Mr. BARANOFF showed cases illustrating Eye Injuries.

Section of Ophthalmology.

President—Mr. A. L. WHITEHEAD, M.B.

MR. CYRIL WALKER, F.R.C.S., showed a Case of Spring Catarrh.

Tumours of Optic Nerve.

By HUMPHREY NEAME, F.R.C.S.

TWO cases of optic nerve tumour (the first in a boy aged 14, the second in a woman aged 79) were described and illustrations shown.¹

DISCUSSION.

Mr. E. TREACHER COLLINS said a number of points arose in connexion with this subject which were of great interest, in diagnosis, treatment, and pathology. Many of them had been dealt with by the author. With regard to diagnosis, he (Mr. Treacher Collins) had found a helpful sign in the progressive character of the hypermetropia. As the growth increased in size, more pressure was exerted on the back of the globe, and the hypermetropia increased correspondingly. With regard to treatment, he had removed such a growth by Krönlein's method, and had thus saved the eyeball. He considered it necessary to stitch the lids together at the time of the operation, as anæsthesia of the globe persisted for some time after, and ulceration might take place. The tumour, in the case he operated on, was cut across at the back of the eyeball at the optic foramen; at the latter spot the optic nerve was much enlarged, and pathological examination showed that the growth extended back through the optic foramen into the skull. No cerebral symptoms ensued, however, and the child lived many years afterwards, this showing that these tumours were not very malignant. He did not think he would do the Krönlein operation in future cases, but would operate as Mr. Neame had done in this case, by cutting the outer canthus. This allowed a sufficiently wide field for exploration; a finger could easily be inserted and any tumour felt at the back of the orbit. After such an operation, there was much less disfigurement than after a Krönlein's operation. Knowledge as to the pathology of these tumours had much advanced. Formerly, the names used for the different forms of growth were very confusing: they were spoken of as myxomata, myxo-sarcomata, or myxogliomata. Yet the fluid from the cysts did not contain mucin, as Mr. Neame had remarked.

¹ The subject matter of this paper is incorporated in an article with illustrations and references published in the *British Journal of Ophthalmology*, May, 1923, p. 209.

[March 9, 1923.]

Mr. Neame had shown that evening slides of two varieties of growth: gliomatosis and endothelioma. There was also a third variety, which was mentioned in Mr. Hudson's paper—neuro-fibroma.¹ This last he (Mr. Treacher Collins) would have thought was more common than Mr. Hudson's paper seemed to show. Three sorts of tumour arose in the optic nerve because there were three different tissues from which growths might arise: endothelium in the arachnoid membrane, fibrous tissue in the sheaths and trabeculæ, and glial tissue supporting the nerve fibres. The glial tissue being a portion of the primary optic vesicles, gliomatous growths were neuro-epiblastic growths. Therefore one would expect that the clinical history of such growths would be of a different character from that of growths which began in the fibrous tissue, or in the endothelium, which were mesoblastic.

Sir JOHN PARSONS asked how these cases compared with glioma of the brain, supposing them to be cases of true gliomatosis. They should be parallel with them, i.e., decidedly more frequent in the young, as borne out in glioma of the brain. What Mr. Collins had just suggested was, that a certain proportion of those called glioma were fibromatosis. He (Sir John Parsons) believed that was the view which Byers put forward in his paper on "Intradural Tumours of the Optic Nerve."² He (Sir John Parsons) objected to the term "neurofibromatosis" as applied to these cases, because it implied an association with peripheral nerves. Fibromatosis was a better term.

Dr. JAMES TAYLOR said he thought glioma of the brain was common in adults, rare in children.

Mr. M. S. MAYOU said he had had a case of what used to be called neurofibromatosis of the nerve, and he thought the microscopical appearance of these tumours was largely determined by the respective predominance of fibrous structure or glial structure; he doubted whether the two classes could be separated. He regarded them as hypertrophy of all kinds of tissue comprising the nerve sheath.

Mr. HUMPHREY NEAME (in reply) said he did sew the boy's lids together, but the swelling in the orbit, presumably from hæmorrhage, was so great that the stitches cut through on the fourth day. Fresh sutures held the lids together a week, and that probably did much good in saving the eye, because there was some corneal ulceration, as evidenced by the nebulae now to be seen. With regard to gliomatosis and these tumours of the central nervous system, the age differed, but the histological appearance showed a striking similarity in the arrangement of the transverse glial fibres, just where the nerve was swelling out and becoming widened. The transverse arrangement of the glial fibres was similar to that in glioma of the cord, close to the strands of fibrous tissue, and near blood-vessels.

Endothelioma of the Orbit.

By F. A. WILLIAMSON-NOBLE, F.R.C.S.

(ABSTRACT.)

[This paper is printed in full in the *British Journal of Ophthalmology*, May, 1923, p. 222.]

Case I.—Sections of growth showed a very marked tendency to the formation of whorls. The larger ones consisted entirely of fibrous tissue,

¹ A. C. Hudson, "Primary Tumours of the Optic Nerve," *Roy. Lond. Ophth. Repts.*, 1912, xviii, p. 339.

² "Studies from the Royal Victoria Hospital, Montreal," 1901,

in the smaller the constituent cells could be seen. The growth was of a scirrhus character and closely resembled the type of endothelioma shown by Mr. Mayou to arise from the endothelial lining of blood-vessels rather than from that of lymphatics. It occupied the orbit and caused proptosis and limitation of movement of the eye. The optic nerve showed vacuolation and œdema of the nerve head as a result of the pressure. At operation the growth was thought to be continuous through the sphenoidal fissure with the contents of the cranium. A dermoid was present in the ocular conjunctiva on the nasal side at the site of an old tenotomy wound.

Case II.—A tumour occupying the orbit, causing proptosis, limitation of movement and papillœdema. Sections showed that it contained cartilage, bone and fibrous tissue, and that it was permeated by a large number of endothelial cells which showed a marked tendency to the formation of blood spaces. The patient, a boy, aged 8½, died a few months after the operation, with signs suggesting the presence of an intracranial growth. Permission for an autopsy could not be obtained.

Both these cases were under Mr. Levy and the author is indebted to him for his permission to refer to them.

DISCUSSION.

Mr. A. LEVY said Mr. Williamson-Noble had stated the most important facts in the history of the case. The patient in the second case referred to lived six months after the operation, and then the death was due to intracranial extension of the original growth. As no post-mortem examination had been allowed, nothing could be said about the site of origin of the growth. From the appearance, he thought it seemed more likely that it had extended backward from the orbit, and its nature, he thought, was endothelioma.

Mr. M. S. MAYOU said endotheliomata of the orbit were very interesting growths. He had had two such cases.¹ The first was in a child, who was admitted to Paddington Green Hospital with proptosis on one side, and a swelling in the temporal region of the same side. The surgeon, thinking the temporal swelling was an abscess, opened it, and a considerable quantity of soft grumous material came away. He (Mr. Mayou) was called in to see the case, and he said he thought there was a tumour behind the eye. The eye became so badly proptosed that it had to be removed. The tumour consisted of very large cells, packed together, with practically no fibrous tissue at all. The child died within a month or two, while in the hospital. The tumour was a large endothelioma, which had probably started in the orbit. It filled the whole middle fossa of the skull, perforating the skull through the temporal bone, and bulging outside in the temporal region. It was of extraordinarily rapid growth. The next case of the kind he had had was in a girl aged about 21. She had proptosis, and an intra-orbital tumour was diagnosed. He first removed the tumour, which was attached to the periosteum, because he could not get permission to remove the eye. Subsequently the orbit was cleared out. The section was very similar to that of one of Mr. Williamson-Noble's cases; superficially it resembled scirrhus of the breast: large cells, with much fibrous tissue between. The patient lived eighteen months. No recurrence took place in the orbit, but there was an intracranial recurrence, which the physician who saw her said was situated in the frontal region; therefore it probably occurred on the other side of the periosteum of the bone. Endotheliomata presented a great variety of appearances, largely on account of the varying amount of interstitial tissue.

¹ *Trans. Ophth. Soc. U.K.*, 1919, xxxix, p. 135.

because of the degeneration so liable to take place in them. Clinically, they all seemed to be very malignant.

Mr. R. AFFLECK GREEVES said he would regard the tumour in the second case as a mixed tumour, rather than an endothelioma. Mixed tumours contained cartilage, bone, and not infrequently epithelial structures. He had seen a mixed tumour which contained not only cartilage and bone, but also epithelial tubules and unmistakeable prickly cells and cell-nests. In the orbit these tumours mostly arose from the neighbourhood of the lacrymal gland, but not from the gland itself, though sometimes the lacrymal gland was stretched over the tumour.

Mr. TREACHER COLLINS agreed with Mr. Greeves that these tumours arose in the neighbourhood of the lacrymal gland. They were analogous to those met with in connexion with the parotid gland—tumours containing bone, cartilage, or endothelial cells. It was better to call them mixed growths of mesoblastic tissue which had reached different degrees of development.

Mr. BERNARD CRIDLAND referred to the case of a patient who came to him some years ago, a young man who had shown very marked proptosis. As he did not feel equal himself to proceeding very far with the operation which might be needed, he handed the case to one of the general surgeons of the hospital who had the same feeling of hesitation with respect to operating. The case was then sent to the late Sir Victor Horsley, who performed an extensive operation, and found an intracranial endothelioma which had entered the orbit through the sphenoidal fissure. A not unimportant point in this connexion was as to how far an ophthalmic surgeon should be prepared to go in operating on these cases. He (Mr. Cridland) considered that unless an ophthalmic surgeon was prepared, if need be, to carry out an extensive operation, sometimes intracranial, it was far better to hand such a case over to a general surgeon. This patient lived for about six months after the operation, as a result of which the sight of the eye was destroyed. The vision of the other eye which was previously equal only to hand movements owing to amblyopia from squint, improved to $\frac{1}{12}$ and J. 4, enabling him to carry on his work as a clerk for some weeks. The patient's age was about 22. He mentioned this point, although it was only a side issue, as showing the visual improvement that might occur in an eye amblyopic from squint even at the age given.

Mr. LESLIE PATON said that there was a point in the clinical history of these cases which had not been brought out in this discussion. It occurred in a case in which he was interested, which he had shown at the Ophthalmological Society eighteen years ago, that of a woman with endothelioma of the orbit starting on the inner side, close to the lacrymal bone.¹ The nature of the tumour was a subject of prolonged discussion among pathologists of London, and the consensus of opinion was that it was endothelioma. It was a very large growth, and getting it away involved removal of the eye. Before the eye was removed, there was involvement of glands, first the submaxillary gland, and that was removed at the same time as a portion of the orbital growth was taken for examination. The nature was the same in both cases. Then the glands down the anterior border of the sternomastoid became involved; then a growth appeared on the top of the clavicle, and later, one in the axilla. These were removed in turn, and proved to be of a similar nature to the primary growth. Five years after the original growth in the orbit the woman died with a huge mass of mediastinal glands, and the growth there was of the same nature. It was a case of transference of growth along the lymphatics, not by the blood stream.

Mr. A. L. WHITEHEAD (President) said that last week he operated upon a case—the specimen of which he now showed—which, from the character of the proptosis and the general appearance of the case, he diagnosed as tumour of the optic nerve. The eye was blind, the growth was considerable, and there was some corneal ulceration, therefore he removed the eye in order to get the growth away. Behind the eye he found a

¹ *Trans. Opth. Soc. Lond.*, 1906, xxv, p. 240.

series of three olive-shaped bodies, and on examining the complete mass after removal it was found that the optic nerve itself was not involved, but was pushed on one side. Neither was its sheath implicated. He considered it to be a neuroma, probably of the ciliary nerve. With regard to the nature of the specimen, Mr. Mayou thought it was possibly endothelioma of the kind which had been described.

Section of Ophthalmology.

President—Mr. A. L. WHITEHEAD, M.B.

Case of Progressive Macular Changes associated with Tremors.

By H. M. JOSEPH, M.C., M.B.

PATIENT, a female, aged 34, by occupation a clerk. Duration of disease, two and a half years. History: Two and a half years ago, tremor of right hand; no defect of vision noticed by the patient. Two years ago eyes examined for first time. Several light spots about both maculæ. Corrected vision, with sph. — 1.5 in each eye, $\frac{6}{12}$. Since then the number of spots has greatly increased, and they are lighter than when first seen. Meanwhile the vision has fallen to $\frac{6}{18}$, and the tremor has become more marked and extensive. Wassermann reaction negative (fig. 1).

DISCUSSION.

Mr. RAYNER BATTEN said that Mr. Joseph's case was of considerable importance as it might be the adult form of cerebro-macular disease. There was a symmetrical change of an unusual character at the macula, associated with evidences of disease of the central nervous system. The difficulty was to classify it. He had long been trying to establish some classification of the various (protean) forms of central choroiditis or choroido-retinitis, and he endeavoured to classify them into (1) cardio-vascular: (a) toxic; (b) arterio-sclerotic; (2) syphilitic; (3) septic; (4) degenerative. Mr. Joseph's case did not appear to fit into any of these. There remained therefore the cerebro-macular class. In this class the changes would appear to vary in the different ages at which they occurred, probably varying with the different stages of development of the central nervous system and also with the development of the macula. The early stages of macular disease were extremely liable to be overlooked even where there was defect of vision to draw attention to them.

Edema would appear to be the early stage of nearly all forms of primary macular disease. A diffuse α -edema was extremely difficult to recognize. The only thing that could be said was that the macular area could not be seen or focussed. Then as the α -edema subsided the various forms of definite macular change developed. He (Mr. Rayner Batten) found it extremely difficult to measure low degrees of swelling at the macula; there was nothing definite to focus. Also the changes might be at different levels, and this might account for the way in which changes would gradually emerge and come into view while one looked at a macula. It was as if a fine veil had been drawn away, and changes which had been invisible at first sight became clear to the investigator upon prolonged and closer inspection. He thought there was still some swelling or α -edema in Mr. Joseph's case and that this had not reached its final stage. While the connecting link between cerebral and macular disease—whatever that might be—remained the same in all classes, it was conceivable that the same toxin might be at

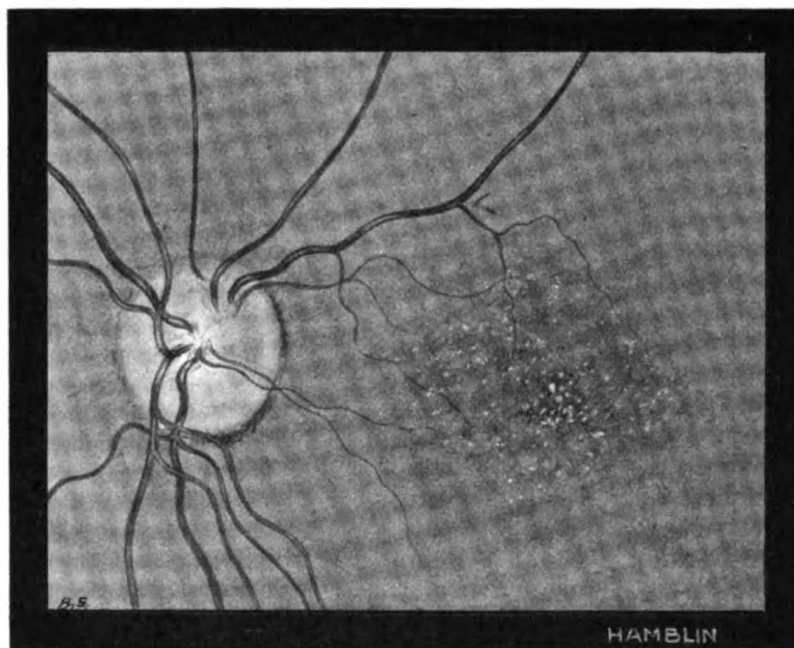


FIG. 1.—Drawing of left macula of Mr. Joseph's case: right eye showed a similar condition.



FIG. 2.—Drawing of left macula, shown by Mr. Batten, as a possible adult cerebro-macular degeneration, for comparison with Mr. Joseph's case.

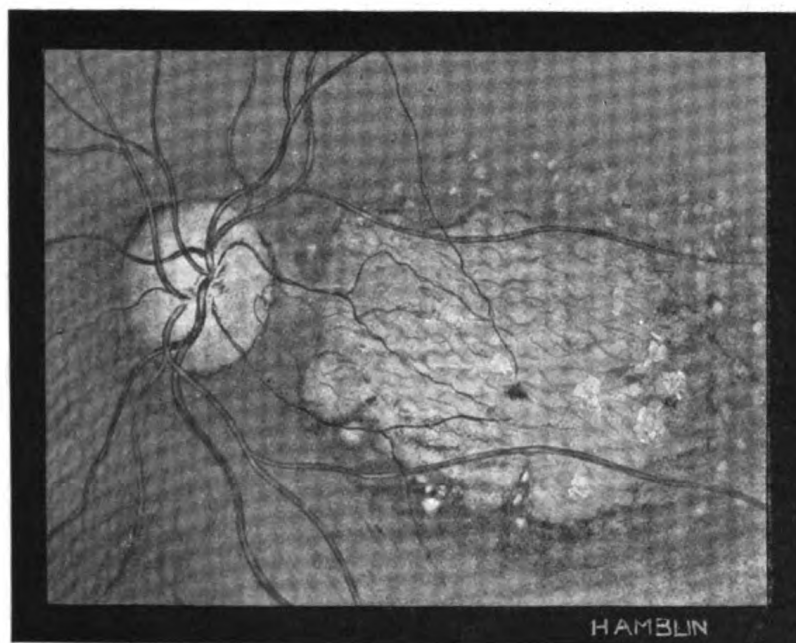


FIG. 3.—Drawing of left macula, shown by Mr. Batten, as a case of senile cerebro-macular degeneration (?).

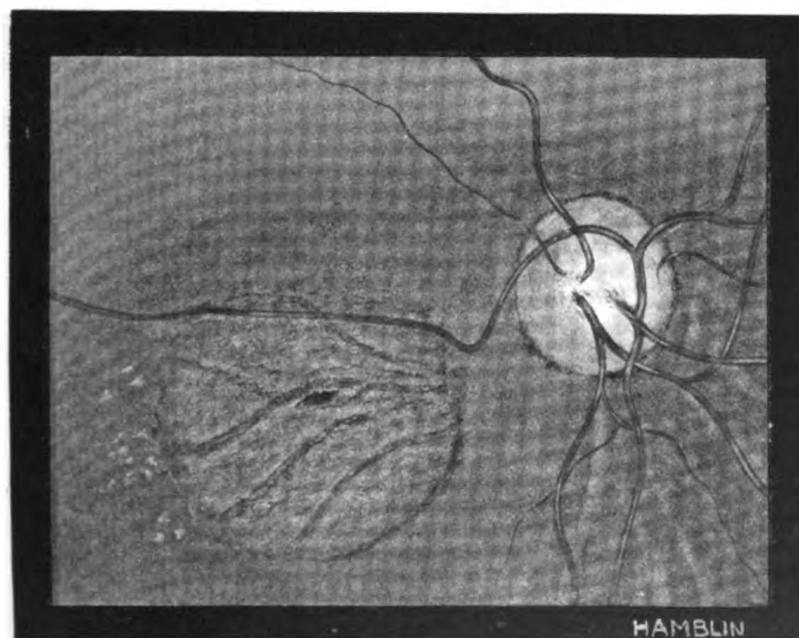


FIG. 4.—Drawing of right macula: senile cerebro-macular degeneration (?).¹

¹ The drawing of this case is not distinct, but it illustrates the main features.—R. B.

work, producing different symptoms at different ages. At present they had the well-recognized infantile and juvenile types of cerebro-macular diseases, and he had suggested a senile type (figs. 3, 4); but as far as he knew there was no description of an adult type, and he suggested that Mr. Joseph's case might be the missing link, i.e., adult cerebro-macular disease.

The points in favour of Mr. Joseph's case being a cerebro-macular disease were: (1) The extraordinary symmetry of the disease in the two eyes; (2) its onset at the time of the tremors; (3) the amount of fundus change being in excess of the defect of vision; and (4) the peculiar brown pigment.

He had found amongst his cases two others which had points of resemblance in the condition of the macula. The first was that of a man, aged 85, with peculiar oscillating pupils, a fine degree of diplopia, especially with near work, and progressive macular changes (fig. 2); the second, that of a woman, aged 45, with facial paralysis, in whom failure of vision had come on simultaneously with the attack of facial paralysis. It remained to be seen whether the course of these two cases would support or disprove his theory.¹

Mr. F. A. JULER said he would like, in the absence of Mr. Leslie Paton, and in connexion with Mr. Batten's cases, to relate a case which Mr. Paton kindly allowed him to see at the National Hospital last month. A boy, aged 18, was brought to Mr. Paton, who diagnosed cerebro-macular degeneration. He heard that there were other children, and asked that they might come to see him. One girl, aged 12, was brought up and was taken into hospital. She had definite cerebral changes. She was mentally deficient, and also showed interesting signs at the macula; these consisted of an area of greyness round the macula, with a definite spot at the fovea, not so marked as in amaurotic family idiocy, but suggesting that type. Mr. Paton thought it was really a connecting link between amaurotic family idiocy and the later cases of cerebro-macular degeneration.

Dr. GORDON HOLMES (in answer to the President's invitation) said he could throw very little light on the condition. He was asked by Mr. Hine to see this case a year ago. At that time the patient had a tremor, very similar to that seen on the present occasion. It was then, as now, more marked on the right side than in the left arm. They had become familiar with this type of tremor since the epidemics of lethargic encephalitis. He did not mean to say the condition presented by this patient was a sequel to lethargic encephalitis, but that the localization of the lesions of the brain must coincide more or less with those responsible for post-encephalitic tremor. Its chief characteristics were that it came on when any part of the limbs was not fully supported, persisted during movement, but could for a moment be controlled by volition. In cases with much tremor after lethargic encephalitis the most important pathological lesions were found in the substantia nigra and the neighbouring mesencephalic nuclei. The slow progress of the symptoms in this case suggested that there was a primary degeneration of certain portions of the grey matter of the brain, involving certain mesencephalic centres. He did not remember having seen a case quite like this before; it was unlike the other instances of so-called cerebro-macular degeneration, at least as far as the other nervous symptoms were concerned. On the other hand, it could be safely assumed that, as in that disease, there was in this case a widespread degeneration of nerve cells, involving the retinal elements as well as those of the brain.

Mr. A. L. WHITEHEAD (President) said it was interesting, in association with Dr. Gordon Holmes' remarks, that he (the speaker) was not conscious, in the various cases of encephalitis lethargica he had seen, of having noted any fundus changes, an experience which he believed to be a common one. If the basal ganglia were affected in lethargic encephalitis without fundus changes, it would be a point against the basal ganglia being affected in these cases of Mr. Joseph.

¹ Mr. Rayner Batten exhibited a number of drawings of the macula illustrating cerebro-macular degeneration at different ages, and amongst them the drawings (figs. 3, 4) illustrating senile cerebro-macular degeneration. See also *Trans. Ophth. Soc. U.K.*, 1922, xiii, p. 109.

Familial Nodular and Reticular Keratitis.

By MONTAGUE L. HINE, M.D.

THE cases of nodular and reticular keratitis reported in this country are not very numerous, and the family I am able to show this evening may help to demonstrate the close connexion between the two conditions, which, at first sight, dealing with individual cases, does not appear so obvious.

In 1904 Holmes Spicer showed a case before the Ophthalmological Society, of nodular opacity in a woman, aged 23, in whom there was an increasing defect of vision, whose father showed very similar changes, while there was a history of similar defect in her paternal aunt and uncle. The next year Hancock showed a case in which mother and child were affected, and noted that the child showed many more small ring-like opacities in the corneæ than the mother, in whom nodules and larger masses formed by the coalescence of nodules were a more marked feature. The same year the late Mr. R. W. Doyne showed a child, in whom, close under the corneal epithelium, there were a number of tiny milky spots, in places tending to coalesce, regarding it as a case of this affection, and in 1914 Moxon showed two children, sisters, with an exactly similar condition, before this Section.

In 1905 Doyne and Stephenson, in reporting five cases of familial degeneration of the cornea, definitely progressive, and more severe than any of the above, summarized all the literature of the condition up to that date, grouping many apparently dissimilar cases under the same head, while in his famous Bowman Lecture in 1908, Mr. Nettleship gave the pedigrees of all the reported familial cases in full.

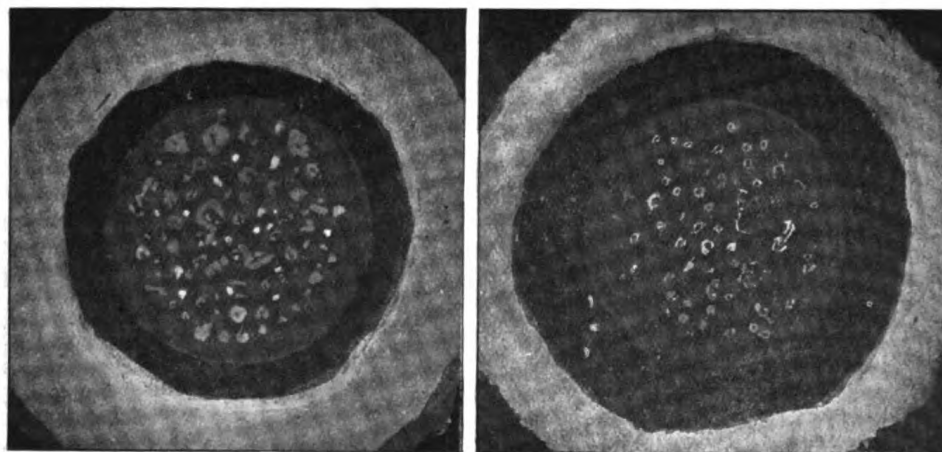
Case I.—Mrs. B., aged 36, gives a history that her eyes have worried her since infancy. She had not had them examined before she came to the Miller General Hospital in February last, to be "tested for glasses." Right vision $\frac{6}{18}$, with $+\frac{1}{0.5}$ 90° = $\frac{6}{18}$. Left vision $\frac{6}{12}$, with $+\frac{1}{0.5}$ = $\frac{6}{12}$. On examination with a corneal loupe the central area of the cornea is seen to be studded with numerous, rather close-set, greyish-white opacities, some rather denser than others, some in the form of rings, others nodular, while in places the nodules become confluent (fig. 1). There is not the same general diffuse haze which was noted in Hancock's case. The periphery of the cornea is clear in each eye. There is no evidence of previous inflammation of the corneæ, or of the eye, and both corneæ are very similar.

Case II.—G. B., boy, aged 14, gives a history of severe headaches and intolerance of light since infancy. He had not had his eyes examined before I requested his mother to let me see the whole family. Right vision $\frac{6}{6}$, with + 2.5D. sph. = 5n. Left vision $\frac{6}{12}$, with + 2.5D. sph. = $\frac{5}{6}$. His corneæ show very similar changes to those seen in his mother, but the rings are more numerous and the nodules fewer (fig. 1). The apparently clear cornea between the nodules shows some almost invisible greyish specks under the epithelium, which extend beyond the area of the true opacity, but do not reach the limbus. Mr. Basil Graves, who kindly examined these patients for me with a corneal microscope, aptly describes the condition as "a fine sheeny speckling," and suggests that the tissue may be impregnated with a fine translucent granular deposit, which may be a crystalline material having a refractive index differing from that of the cornea. He also suggests

44 Hine: *Familial Nodular and Reticular Keratitis*

that perhaps the definite opacities may be due to a condensation of this deposit, as the same appearance is not seen in the mother. Both eyes show the same changes.

Case III.—Lily B., aged 10, gives a history of slight headache, and of no special intolerance of light. Like her brother she had not had her eyes previously examined. Right vision $\frac{6}{12}$, with



Mrs. B.

FIG. 1.

G. B.

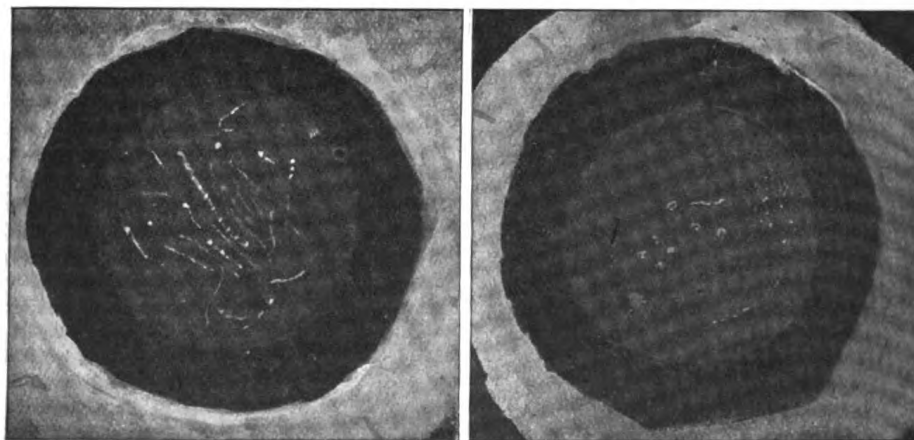


FIG. 2.—L. B.

$\frac{6}{12} + 2.5D. sph.$
 $+ 0.5D. cyl. ax. 90^\circ = \frac{5}{6}$. Left vision $\frac{6}{9}$. The right cornea shows quite a different appearance from that seen in her mother and brother, the opacity being composed of a number of fine lines, many of which are seen to be composed of a series of tiny dots, while some have larger dots interspersed in their length (fig. 2). The lines in this case do not form a meshwork, as they do in most of the reported cases of reticular keratitis, but remain separate and uncrossed. One very fine line, rather fainter than the rest, and not obviously

composed of dots, can be seen running upwards from the limbus below, and is the only one that might suggest that it is an obliterated vessel, resulting from previous inflammation.

The left cornea shows very few changes, but is especially interesting in that it combines two of the linear opacities with several of the small rings, similar to, though fainter than, those seen in her brother. Her corneæ show the same sheen as those of her brother.

In all these cases Mr. Basil Graves finds that, under high stereoscopic magnification with the corneal microscope, the anterior faces of the opacities lie in a plane which he judges to be that of Bowman's membrane, which is invisible. Each nodule has a slight thickness. In the mother, but not in the children, some of the denser opacities were seen to be at deeper levels in the corneal substance, even down to Descemet's membrane.

There is one other boy in this family, aged 12, whom I have examined, and whose corneæ are perfectly normal. There is a history of syphilis in the maternal grandfather. The Wassermann reaction is negative in all the cases.

Mr. HUMPHREY NEAME said Mr. Fisher had a case at Moorfields, that of a man, aged 50, who had been attending at fairly regular intervals, and who had a similar condition to that now described in each eye. But the particularly interesting point about him was that in the right eye it was in the form of lines of opacity, and in the left eye almost entirely in the form of nodules. He (Mr. Neame) examined that man with the corneal microscope, and the lines in that case appeared to him to be mainly about the middle depth of the substantia propria.

Mr. R. LINDSAY REA, M.D., F.R.C.S., showed a case of "Tuberculous Eyelids, together with Disseminated Tubercle of the Body and Limbs."

Some Suggestions on the Embryology of Congenital Crescents.

Shown by IDA C. MANN, M.B., B.S.

(Henry George Plimmer Fellow in Pathology.)

(ABSTRACT.)

WE have for consideration a group of cases characterized by the fact that they are congenital, stationary, not necessarily associated with any one error of refraction, and most frequently situated below the disc. There is apparently some factor which determines the greater proportion of inferior crescents and this factor must be looked for in the normal development of the disc, since an abnormality is more likely to be associated with a normally existing embryonic structure than to arise *de novo* as a pure aberrance. The embryonic structure in this case is the choroidal fissure. The presence of congenital crescents in other situations does not detract from the fact that the choroidal fissure is the determining factor in the inferior type. The presence of the fissure will merely account for the preponderance of this type, since the essential condition for the formation of a crescent is that the pigmented outer layer of the optic cup should not reach quite up to the insertion of the optic stalk. The presence of the fissure merely allows of this occurring more easily below the disc than elsewhere.

The slides exhibited show the structure of the inferior crescent in the human eye and also illustrate the development of the fissural region in the

human subject, the bird and the reptile. In the inferior crescent it can be seen that the essential difference from the normal consists of the failure of the pigment epithelium and nuclear layers of the retina to reach the edge of the optic disc in its lower part. There is therefore a small area below the disc in which all the elements of the retina, except the nerve fibre layer, are absent, so that this layer is separated from the sclerotic only by a very thin extension of the choroid. Developmentally, it has been shown that eversion of the unpigmented layer of the optic cup normally takes place in the upper end of the choroidal fissure. This is seen in a large number of vertebrates. In some animals, including man, the small everted portion of the inner layer loses its connexion with the rest of the inner layer and disappears, but in some other types (birds) the connexion is retained and nerve fibres grow into the everted projection. If in man the everted portion developed aberrantly and retained its connexion with the inner layer, a condition resembling that normally present in birds might result. Such a condition is present in a congenital inferior crescent, which bears a great resemblance to the structure of the lower part of the disc of the normal bird.

The occurrence of crescents in situations other than inferior—i.e., the failure of the pigment epithelium to reach the edge of the disc—is comprehensible in the light of the work of the late George Coats, who showed that any one part of the optic cup may differentiate into tissue resembling that normally derived from another part. The failure of the choroid and occasional thinning of the sclera in the crescent has its parallel in normal development. The mesoderm in contact with the outer layer of the optic cup differentiates into choroid, while that in contact with the inner layer—whether inside the cup or everted along the fissure—does not. The scleral condensation in its turn follows the developing choroid.

To sum up, therefore, we may say that congenital crescents in any situation are due to the failure of the pigment epithelium to reach the site of implantation of the optic stalk. This failure may occur anywhere, since aberrant differentiation of the various parts of the secondary optic vesicle is known to occur. It is, however, much more likely to occur below the disc than elsewhere, since it is normal here at one stage of development in man. Hence the greater frequency of inferior crescents, which can be looked on as developmentally homologous with the *cauda* of birds, the architectural basis of the *cauda* being present, though small, in the normal human embryo. The failure of the choroid is directly correlated with the absence of the pigmented epithelium, and the occasional failure of the sclera has its parallel in the retardation of condensation along the fissure during development.

DISCUSSION.

Sir WILLIAM LISTER said that the subject of the paper had been treated in a very interesting way, but he pointed out that the author did not offer any explanation why inferior crescents were always associated with an inferior staphyloma. He thought that it was a generally accepted view that the inferior staphyloma was due to weakness and giving way of the sclerotic in the region of the choroidal cleft. Inferior staphyloma was characterized by an increased myopia in this region, with thinning of the retinal pigment, which led to a greater visibility of the choroidal vessels over this area. With these changes was associated the inferior crescent together with a downward tilting of the disc. He thought that the tilting of the disc and the inferior crescent were both due to the stretching of the sclerotic below the disc, which caused a dragging downwards of the retina, choroid and optic disc in this direction, and

therefore that the inferior crescent should be looked upon as a true "retraction crescent," viz., a crescentic exposure of the sclerotic or nerve sheath by a retraction of the retina and choroid from the disc margin. He believed that in the same manner the ordinary external myopic crescent was due to stretching at the posterior pole of the eye, the internal crescent to a nasal staphyloma and the so-called "ring crescent" to stretching taking place in the sclerotic round the disc. Retraction of the retina and choroid from the margin of the disc seemed to him the only reasonable explanation for the perfectly crescentic margin of the defect, a regularity of outline which would not be expected if the crescent were due to a local failure in development of pigment in the outer layer of the secondary optic vesicle.

Sir JOHN PARSONS said that his view on the matter differed from that just expressed by Sir William Lister; he thought the mode of production was that suggested by Miss Mann. But in many cases there was associated with it some failure of the choroidal fissure to close, and that accounted for the ectasia. With regard to the myopic so-called retraction crescent he had long thought it was probably an atypical coloboma—a crescent in an unusual situation, and that it had no relation to a stretching of the walls of the globe associated with myopia. Of course it was necessary to explain why it was so frequently present in myopia, and that, he confessed, was a difficulty. But, having a myopic crescent as, probably, an atypical type of inferior crescent, then with the stretching of excessive myopia, there might be an increase in the crescent due to the stretching process. But the fact that one met with high degrees of myopia with no crescent, and low degrees of myopia with a considerable crescent, and the further fact that there was no relationship between the size of the crescent and the degree of myopia, were strongly against the view that the primary cause was a stretching directly due to myopia. Miss Mann's suggestion with regard to the excessive pigmentation in the disc was very interesting; a very good example of that was found in a specimen which he and the late Mr. Coats examined, a unique case in which there were several congenital malformations in the eye, associated with congenital malformation of the brain and meningo-encephalocele. That case was described in *Brain*.

Miss MANN (in reply) said that the presence of a staphyloma below the disc associated with inferior crescent could also be explained as dependent on the retardation of the sclerotic condensation along the line of the fissure. The sclerotic there might not be so strong as in the rest of the eye, hence the eye would tend to bulge at any spot which was weak in association with delayed or abnormal closure in the upper part of the fissure. She did not think that the presence of staphyloma below, in the line of fissure, militated against the argument that the crescent was an anomaly of development. She agreed with Sir John Parsons that myopic crescents also had a possible congenital basis, though in them there was some pathological process superimposed. But the progression of the sclerotic condensation from the equator backwards might possibly account for a weakness of the whole posterior pole of the eye in some of these cases.

The Tournay Reaction.

By PHILIP DOYNE, F.R.C.S.

[This paper will be published in full in the *British Journal of Ophthalmology*.]

(ABSTRACT.)

THE data for this paper were obtained by examining the eyes of forty cases of general paralysis of the insane for this reaction. These cases were seen at the Springfield Mental Hospital, and I must express my indebtedness to Dr. Worth, the Superintendent of the hospital, for affording every opportunity for my so doing. In addition to these cases of general paralysis of the insane, in

the course of routine examination of all patients at that hospital I looked for this reaction. Tournay¹ describes the reaction as follows :—

“ When a man, whose ocular apparatus is normal, whose pupils are equal, reacting normally to light and accommodation, looks strangely to his right and maintains this position, the right pupil becomes larger than the left. Thus, isocoria being the rule in anterior fixation, anisocoria becomes the rule in lateral fixation.”

My conclusions were that this reaction in normal eyes can always be obtained, though the ease with which it can be obtained varies from case to case. With reference to the reaction in the cases of general paralysis of the insane, it appeared that the reaction was not to be obtained in those eyes in which the pupil reactions to light and accommodation were not present.

DISCUSSION.

Mr. J. HERBERT FISHER repeated a suggestion which he had previously made in private with regard to this pupillary phenomenon. The dilatation of the pupil on the abducted side did not take place immediately the eyes were lateralized, but only after a certain interval had elapsed. It occurred on the abducted side, possibly, he thought, by reason of pressure exerted by the external rectus muscle, in strong contraction, upon the ciliary ganglion. Mr. Doyme, relating his own experience, said he had had a sense of strain during the maintenance of the extreme abduction which was necessary. The pressure on the ciliary ganglion probably paralysed the control of that ganglion over the pupil; therefore on that side there was a dilatation. He thought one need not go further back than the orbit for the explanation.

Mr. RAYNER BATTEN called attention to the unequal reaction of the pupil according as light fell on the nasal or temporal half of the retina. Light falling on the nasal half of the retina produced a prompt reaction, while light thrown on the temporal half gave little or no reaction. The difference in reaction of the two halves of the retina was extremely common but he had been unable to find any pathological significance for it. He had no explanation to offer for this symptom, but all examinations of pupil reaction with light from one side must take this very common but variable symptom into consideration, to be of any value.

¹ *Bull. de l'Acad. de Méd.*, 3 sér., 1917, lxxvii, p. 680.

Sections of Medicine and Ophthalmology.

JOINT MEETING.

Dr. G. NEWTON PITT, President of the Section of Medicine,
in the Chair.

DISCUSSION ON "THE SIGNIFICANCE OF THE VASCULAR AND OTHER CHANGES IN THE RETINA IN ARTERIO-SCLEROSIS AND RENAL DISEASE."

Dr. G. NEWTON PITT (Chairman)

Said that this was a subject which interested both the ophthalmologist and the physician, and one on which the profession had yet much to learn. He hoped some new facts would emerge from this discussion.

There were one or two in particular upon which he would like information. One was, what was the relation between the amount of blood urea and the development of these retinal changes? If any observations on a great number of cases had been made—which he thought was doubtful—he hoped the results would be placed before the meeting.

The other was as to the relationship of retinal changes to arterial pressure and to arterio-sclerosis. In very many cases one could demonstrate arterio-sclerotic changes in the retina when retinitis was not necessarily present; and there were well marked cases of albuminuric retinitis in which the arterio-sclerotic change was not necessarily present. There was also the retinitis due to toxæmia which occurred during pregnancy, yet the retinitis would clear up and get well as soon as the child was born.

These were some of the points on which, particularly, information was desired. Also to what extent was retinitis dependent upon toxæmia, upon arterio-sclerosis, and upon high arterial pressure respectively?

Dr. H. BATTY SHAW.

I need hardly say that the task of opening this discussion on the significance of the vascular and other changes in the retina in arterio-sclerosis and renal disease is an exceedingly interesting one, because the ensuing debate will give two branches of our profession an opportunity to consider together the difficulties both meet with in the consideration of the above subject.

My presence here is largely due to the fact that owing to the generosity of my medical colleagues at University College Hospital I have had an opportunity of studying the clinical aspects and the post-mortem findings in a number of cases in whom the heart was found to be hypertrophied at the necropsy.¹ I may say at once that when I had gathered together the facts connected with this inquiry, and tried to fit them in with the various theories which had been advanced in explanation of them, I became quite bewildered. The only way in which I could explain the facts was to jettison former explanations, and look about for others. To show you how necessary this step was, I have merely to tell you of the following occurrences: (a) Some of the cases observed died of uræmic symptoms, and yet the kidneys did not reveal the changes usually described in these organs in such cases; (b) albuminuria was present in varying degrees of intensity, or was absent, and yet the kidneys gave no uniform appearances which could lead one to say why they were responsible for the former, or latter condition of the urine; (c) changes were met with in the retinæ which in no way appeared to represent regularly what form or what detail of structure the kidneys would reveal in those cases; (d) some of the patients who during life presented signs of grave disorganization of the brain, showed at post-mortem examination that the brain was, to the naked eye, normal in appearance, and that the blood-vessels were free from changes in the middle coat or in the intima.

One particular phenomenon present in the cases studied seemed to be directly associated with the cardiac hypertrophy which they all presented; this particular phenomenon—hyperpiesis—was remarkable in one feature, viz., that it was so variable; in the course of a few days or so it would fall from a maximum to a minimum, not far removed from the normal; moreover the converse would occur, and neither clinical study nor post-mortem inquiry was able to reveal why these curious fluctuations occurred. In common with yourselves I had been taught to believe that this particular phenomenon was caused by a condition of the arteries known as arterio-sclerosis, an expression which has been used to cover an extensive field of change, but for the purposes of the discussion to-day I propose to limit it to that change met with in the middle coat of the arteries—the origin of which caused so much interesting discussion years ago as to whether it was due to hypertrophy or sclerosis of the middle coat—and to the other change due to the proliferation of the cells of the intima. The obvious question arose, how could this physical sign—hyperpiesis—vary so extraordinarily, and so quickly, between minimal and maximal heights, and yet be due to such a stable condition as that which we have called arterio-sclerosis? It was obvious that there was no such dependence. I could only explain the variability of the hyperpiesis by invoking the presence of a variable amount of a poison in the blood which, besides producing many hitherto inadequately explained clinical conditions, could also explain the changes in the middle coat of the arteries, as well as those in the intima.

I found that the invocation of toxic agents present in the blood also enabled me to explain away the difficulty of correlating many hitherto accepted signs and symptoms of "renal" disease, when little or no actual renal disease was present. I know for a fact that many other observers, both physicians and ophthalmic surgeons, would also wish to turn their eyes away from the view that arterial change is responsible for so much of what interests us in this

¹ "Hyperpiesia and Hyperpiesis (Hypertension)," Oxford Medical Publications, 1922.

discussion, and would rather look upon the change in the arteries as being a mere result of a toxæmia, working behind the scenes, and they have suspected that part of this toxæmia is bacterial in origin, and thus have explained some of those terminal episodes which present themselves to us in the form of hæmorrhages, pleurisy, pericarditis, &c.

With regard to the nomenclature of kidney disease, we know also that a break-away has occurred from older doctrines; too much of the change in the kidneys seemed to be allocated to a local dyscrasia of these organs, and an effort has been made to show that the changes in the kidneys are themselves direct products of the toxæmia originating from bacterial action at a remote site, or actually in the kidneys, or to a toxæmia the origin of which is at present unknown to us, but is certainly present as judged by its effects. This tendency towards a study of the blood has already led to a greater certitude of diagnosis as to the condition of the kidneys than any clinical examination of the body, or chemical study of the urine—at any rate so far as protein extrusions are concerned. Moreover, claims have been put forward to the discovery that changes in the arteries are due to a toxæmia of bacterial origin. We have known for years that intimal change can be produced experimentally by the injection of bacterial toxins, and now it has been claimed that the changes in the middle coat are simply of the nature of a chronic inflammation. As a physician, I feel that the time has arrived when we may safely discard the older doctrine which makes arterio-sclerosis responsible for the signs and symptoms we are to discuss to-day, and by concentrating our efforts upon the toxic view and supporting it as probable, we may thereby stimulate research which shall give this newer, or rather, revived conception, the support of demonstration.

Turning to the studies carried out by ophthalmic surgeons, it is well known that despite the strong views held by them that it is possible to differentiate arterio-sclerotic retinitis from albuminuric retinitis, they find difficulties. They admit that there is something wrong in their deductions, and we are familiar with the fact that they would prefer to speak of renal retinitis or the retinitis of renal disease, rather than of albuminuric retinitis, for reasons approved by physicians; indeed, as a physician, I feel that they have relied upon my branch of our profession too much, and have been satisfied too readily sometimes to accept our clinical diagnosis of some vascular catastrophe which has ended in paralysis or death. Further, they have gone so far as to admit that an arterio-sclerotic retinitis may be succeeded or accompanied by the signs of renal retinitis, and would argue that this is due to the fact that the harmful effects of arterio-sclerotic changes have progressed and, by involving the kidney, have led to the superadded changes formerly considered attributable to kidney disorganization. Such explanation has been widely held in the past, but as already intimated, evidence has been found which points to the possibly minor part played by the kidneys in the production of so-called "renal" signs and symptoms.

Some ophthalmological experts are already prepared to concede that the changes in the retina in renal retinitis are toxic, and that the seat of origin of these blood-borne toxic agents is pre-renal: so that, for them, even renal retinitis is a misnomer, just as is its predecessor, albuminuric retinitis. There are other weak points in the deduction made by some ophthalmic observers, namely, some of the changes met with in arterio-sclerotic retinitis are due in part to thickening of the middle coat, hence the silver or copper-wire appearance of the retinal arteries; other objective signs are due to thickening of the intima,

hence the irregular lumina of the arteries as seen by the ophthalmoscope. Now both of these changes indicate additions to, not subtractions from, the strength of these vessels.

How is it that hæmorrhages occur in the retina in arterio-sclerotic retinitis? When the hæmorrhage passes off no change of the nature of a rupture of retinal artery or vein has been demonstrated. As a physician, I am inclined to wonder whether the hæmorrhages are not more likely to be capillary in origin, and to have a genesis similar to that met with in the petechiæ of malignant endocarditis and other infective disorders. It is true that we have not yet arrived at a clear demonstration that these petechial manifestations are due to ruptured capillaries; but there is a large amount of analogical evidence that they are capillary in site of origin, and that they are of infective origin. Why should their genesis be different from that of the petechiæ developed elsewhere? And how are the white patches met with in arterio-sclerotic retinitis explained? Where do they come from? Or are they formed from local elements? If so, what provokes their formation? Surely not arterio-sclerotic changes! Are they not likely to be provoked by some similarly noxious agent, also brought by the blood stream?

We cannot, however, disregard the fact that most painstaking studies over very long periods have been made by ophthalmic surgeons, and a steady progress has been observed from changes met with in the retinal arteries to the development of hæmorrhages, of white patches, and even of papilloedema, all due to arterio-sclerosis! There can be no reason whatever to carp at these findings, except that they need not be charged primarily to the account of the vessels. Is it not possible that changes even in the arterial walls leading to the thickening of the middle coat, with or without proliferative changes in the intima, are all due to a poisonous condition of the blood? Indeed, as already stated, one recent study has led to the deduction that the changes in the middle coat are of a chronic inflammatory nature, implying that an infective agency is behind them all. We also know from experimental studies already referred to, that intimal changes can be produced by the injection of bacterial toxins, and the study of syphilis shows incontestably what havoc can be engendered in the arteries by spirochætes.

It is said that renal retinitis is always bilateral, and yet works by ophthalmic surgeons reveal the observation that "renal" changes may be shown in one retina by unilateral papilloedema, which conforms with the well-known observation that blood-poisons need not necessarily produce symmetrical changes—unilateral Argyll-Robertson pupil and single wrist-drop in lead-poisoning being other examples of this anomaly. Further, I have seen figured in one work of reference "renal" retinitis which had lasted six years, though it is stated that cases of "renal" retinitis seldom survive two years—in contradistinction to cases of arterio-sclerotic retinitis in which the patients may live a great number of years: the duration of life after the development of both forms of retinitis seems somewhat variable.

If we need further disquieting evidence as to the dependence upon vascular disease, of hæmorrhages into the retina and the development of white patches, we may reasonably put forward the observation of ophthalmic surgeons that retinal hæmorrhages and white patches have been noted in pernicious anæmia, in the "fast disappearing disease" chlorosis, and even in the secondary anæmias due to peptic ulcer or to malignant ulcer of the stomach—diseases in which arterio-sclerosis is not charged with being the immediate cause. We may well ask, also, what is the source in secondary

anæmias of the development of soft-edged white patches in the retina which are quite indistinguishable from the "cotton-wool patches" of renal retinitis? Assuredly the changes in the "renal" retinitis and in the secondary anæmias are due to noxious agencies brought by the blood. It seems to me that it is likely that the changes met with in arterio-sclerotic retinitis are not due to the vascular change, but that the hæmorrhages and white patches and the slight change in the disc differ only from those met with in "renal" retinitis in their being called into existence by the slower operation of blood poisons, or by a decreased local tissue-sensitiveness in the former case as compared with the latter, or by the smaller amount of such poisons arriving at the scene of action in the former case, as compared with the latter. Some such explanation would also account for the longer survival of the former cases compared with the latter. It is taught that œdema of the limbs and body is a late arrival in the cases under consideration: possibly that is the reason why the white patches of arterio-sclerosis are so sharply defined and somewhat less extensive and are so "woolly" and so voluminous in so-called renal retinitis.¹

If further evidence is needed that changes in the retina can be, and are, produced by a disordered blood-state rather than by changes in the kidney or by changes in the retinal vessels, we have it beautifully illustrated in eclampsia, where changes may be met with in the eye and in no other organ, such changes being remedied by abortion or by other evacuation of the womb.

Arterio-sclerosis has had a long innings as a cause of retinal change, but I submit that it is more reasonable to look upon arterio-sclerosis as a first effect, and arterio-sclerotic retinitis as a later one, of a toxæmia which acts slowly and in minimal quantity: when the toxæmia acts quickly or in accumulated large bulk the other type of retinitis results, and this other type needs another qualification than "renal," for the changes in the kidneys may be minimal, or the changes may be extremely variable, or extreme changes may be found in the kidneys with little or no change in the arteries or in the retina. I would suggest that for the terms "arterio-sclerotic" retinitis and "albuminuric" or "renal" retinitis, or the retinitis of renal disease, we should substitute the terms "chronic" and "acute," "late" and "early" or "minimal" and "maximal toxic" retinitis, leaving for the future the investigation of the nature of the toxin concerned, the laws by which its potency is regulated and the reason why the kidneys should be so variably involved in different cases.

Mr. R. FOSTER MOORE.

I propose to bring forward this evening what evidence I can in support of the view that in some cases of arterio-sclerosis a distinctive form of retinitis is developed, which is due, I believe, to the local vascular disease in the retina. I need do no more than say that the term retinitis is applied in the sense in which it is used in nephritis or diabetes.

It may be stated at the outset that these cases have usually been confused with renal retinitis, and I believe the statement that has been made and repeated, that renal retinitis in the old conveys a prognosis which is less serious than in the young, is in part due to the inclusion of the cases under consideration.

¹ Sir J. Herbert Parsons, F.R.S., "Diseases of the Eye," 1918; R. Foster Moore, "Medical Ophthalmology," 1922.

6 Foster Moore : *Arterio-sclerosis and Renal Disease*

An endeavour will be made to establish the three following propositions :—

(1) That the ophthalmoscopic appearances of the condition are in large measure distinctive as compared with renal retinitis, the chief condition from which they have to be identified.

(2) That the retinal exudates are developed as the result of the local vascular disease in the retina.

(3) That as regards length of life and manner of death, this ophthalmoscopic condition implies a prospect which is in sharp contrast with that conveyed by renal retinitis.

I have collected in the table recorded (pp. 11-14) forty-seven cases of retinitis which I have seen on several or many occasions, and which I have had under observation in most cases for a number of years:

PROPOSITION 1.

It will, I believe, be agreed, that in the majority of cases of general arterio-sclerosis, the retinal arteries share in the disease to such a degree, that the condition in them is recognizable on ophthalmoscopic examination. Thus, of forty-four consecutive cases admitted to the wards of St. Bartholomew's Hospital on account of a gross vascular cerebral lesion, thirty-one, i.e., 70 per cent., provided undoubted evidence of retinal vascular disease; and we may go further and say, that an estimate as to the degree to which the general arterial disease has attained can be arrived at, with considerable accuracy, by the evidence which the retinal vessels supply.

It does not come within the scope of the present communication to describe these appearances, but as the general disease progresses, the disease in the retinal vessels becomes more marked, until, in a certain proportion of cases, exudates are developed in the retinal tissues, which I believe are due to the thickening of the coats of the arteries and to the reduction in their lumina, leading to impaired circulation in the tissues. I have elsewhere given reason for thinking¹ that the local pressure in the retinal arteries in these cases is less than the normal, though the pressure in the large arteries is greatly raised.

The Ophthalmoscopic Appearances.

It is not suggested that the individual spots or small areas of exudate, the presence of which is taken to justify the term retinitis, are pathognomonic, or that spots which at any rate, ophthalmoscopically, are similar do not occur under other conditions, but it is suggested that the spots themselves are in some measure characteristic in appearance, arrangement, and in the changes they undergo, and that when they are present in combination with marked vascular disease an ophthalmoscopic picture is presented which is in large measure distinctive.

The exudate takes the form of small whitish dots, or spots, or small areas; they have hard edges, and there is no pigmentary disturbance nor evidence of œdema around them; a spot of the diameter of one of the main retinal veins would be a rather large one. They occur chiefly in the central regions and are seldom copious; they may take the form of a partial or complete star figure around the yellow spot; at times they seem evidently arranged in relation to the radicles of the veins. Occasionally larger areas or small plaques are seen, formed apparently by the coalescence of spots which previously were discrete.

¹ *Trans. Ophth. Soc. U.K.*, 1916, xxxvi, p. 319.

In some cases the exudate is so scanty and the evidence of vascular disease is so conspicuous, that the exudate is overlooked or ignored amongst the much grosser and more obvious changes. In most cases flame-shaped retinal hæmorrhages are present; they are incidental to the vascular disease and have but little distinctive value.

A very striking feature of this form of retinitis is the frequency with which it is unilateral; thus out of forty-five of the present cases it affected one eye only in twenty-eight instances, i.e., it was unilateral in 60·7 per cent.; evidence of disease of the vessels was always present in the other eye. In renal disease retinitis may occur in one eye before it is evident in the other, but it very seldom remains unilateral for any long period.

The exudate is slow to develop and slow to undergo change, but if an accurate plan is made, it is easy to satisfy oneself that individual spots disappear and leave behind no trace of their former presence, but fresh ones are usually simultaneously appearing, so that the general aspect of the ophthalmoscopic picture may be maintained over long periods. The longest periods of which I have notes are seen in Cases XXXII and XLIII, in which retinitis persisted for eight years and seven months, and seven years and nine months, respectively. In other cases the exudate may entirely disappear; this happened in eight of the present cases, viz., Cases XII, XXVIII, XXX, XXXI, XXXV, XXXVIII, XLI, and XLVII.

In two of the cases disappearance of the exudate followed thrombosis of the retinal artery and seemed to be dependent upon the thrombosis for its occurrence, for in Case XXVIII retinitis was at first present in each eye, thrombosis then occurred in the left retinal artery, and disappearance of the exudate followed in this eye whilst it persisted in the other. There is good evidence that an eye which has been the subject of serious fundus disease, such for instance as thrombosis of the central vein or artery, or previous retinitis, or even high myopia, is as a consequence protected against the occurrence of retinitis in renal disease, and it seems that in the above mentioned cases another aspect of this phenomenon is exemplified, for retinitis was at first present, and the occurrence of thrombosis of the retinal artery seemed to determine its disappearance.

The chief points in which the ophthalmoscopic appearances differ from those of renal retinitis consist in the character and distribution of the exudate and the changes it undergoes, the frequency with which it is unilateral, its association with severe retinal vascular disease, the absence of œdema of the retina so that retinal detachments seldom if ever occur, and the absence of cotton-wool patches.

Finally, in this connexion, I should like to quote the following from the late Marcus Gunn's original paper on the ophthalmoscopic evidences of arterio-sclerosis,¹ in which he says:—

"In the most advanced cases the lines of the folds which radiate from the fovea centralis, due to the œdema, are sometimes eventually marked out by the deposit of white spots of degenerated effusion, so that we get the ophthalmoscopic appearances diagnostic of so-called albuminuric or renal retinitis, though in the variety now under consideration the condition may exist only in one eye and may not be accompanied by albuminuria."

This appears to be a good account of the condition we are considering; it is an account to which I had paid no attention previous to preparing this

¹ *Trans. Ophth. Soc. U. K.*, 1898, xviii, p. 361.

address. Amongst Gunn's fourteen cases there are six to which the above description would apply, and one may anticipate by saying that each of these six suffered from a cerebral hæmorrhage.

Histology.

These cases were followed as "out-patients," and consequently I have once only obtained a specimen for examination: I hesitate therefore to say much with respect to the histological characters of the exudate. In this specimen the spots were composed of small spherical areas of structureless hyaline material in the external molecular layer; they were deeply stained by orcein and took on a mauve colour in eosin and hæmatoxylin sections. So far as their structure goes they seem to be similar to the exudate which forms the "star figure" in renal retinitis, they are however smaller, there is no histological evidence of œdema, and no fat-containing phagocytic cells were present, such as may be seen in renal cases.

PROPOSITION 2.

That the retinal exudates are developed as the result of the local vascular disease in the retina and indicate a further advanced stage of it. This point can be considered in individual cases, and by a comparison of groups of cases. It is sometimes possible to watch, in the individual, exudate becoming super-added in the course of some years, where at first vascular disease alone was present. Cases XXVI, XXXII, XXXIV, XXXVIII, XL, XLI, and XLIV are examples of this.

Case XXXII is worth quoting in a little detail. The patient was first under the care of Marcus Gunn in 1908, at which time she had extensive arterio-sclerosis but no retinitis, her urine was free of albumin and sugar, and this was also true in 1909 and 1910. In 1911 she came under the care of the late George Coats, and then for the first time was found to have what he described as "white glistening spots disposed radially round the macula," and albumin in the urine. I saw her first in 1913 when she had a blood-pressure of 250 mm., a cloud of albumin in the urine, and retinitis in each eye; she was under my constant observation from this time till September 1, 1919, the date on which I last saw her. She had had a stroke in May, 1918, her blood-pressure was 260 mm., and she still had retinitis in each eye: thus for the first three years during which she was under observation she had retinal vascular disease only; she then developed retinitis, and this persisted more or less unchanged for eight years and seven months.

Again one may make a comparison between a group of patients in whom vascular disease alone is present in the retina, and another group in whom retinitis is present in addition. If, as I believe, the presence of exudates in the retina implies a stage of arterio-sclerosis in advance of a case in which vascular disease alone is evident, and if, as I am sure is the case, the disease of the retinal vessels increases *pari passu* with the general arterial disease, then a comparison of two groups of patients—in one of which vascular disease alone is present, and in the other of which exudates are present in addition to the vascular disease—should provide evidence that the latter group is composed of patients suffering from a further advanced stage of the disease. For this comparison I have available thirty-five patients belonging to the former group, and thirty-one belonging to the latter; the average age of each group is 59.

The average systolic blood-pressure of those without retinitis was 211 mm., and of those with retinitis 222 mm.

In March, 1919, seventeen (i.e., 48 per cent.) of the former group were known to have died, and eight were known to be alive, whilst on the same date twenty-one (i.e., 67 per cent.) of the latter group were known to have died, and six were known to be alive; further, on the same date, eleven of the first group were known to have suffered from a gross vascular cerebral lesion, and in eleven there was evidence that such had not occurred, whilst of the second group thirteen were known to have suffered from such a lesion, and nine had not done so.

It will be seen that, as regards the systolic blood-pressure, the incidence of death, and the frequency of gross vascular cerebral lesions, the second group manifests a higher grade of disease than the first. One may safely assert that these findings are at any rate compatible with the view that it is in the more advanced cases of arterio-sclerosis that retinitis is developed.

It is interesting to speculate, were it possible to examine the other tissues of the body during life under a magnification of fifteen diameters, as is done in the eye, whether changes in them would not be also found to occur, corresponding with these changes which are visible in the retina alone of the whole body, and especially would one expect such changes to be found in the brain tissues, for the retina is but a specialized part of the brain which is rendered subject to our examination.

PROPOSITION 3.

Lastly, we come to the consideration of the length of life and the manner of death of patients who are the subject of this form of retinitis. It is undoubtedly true that few patients live so long as two years after the discovery of renal retinitis; thus Belt¹ found that of 419 patients, 94 per cent. died within two years, and Miles Miley² found in forty-five cases that the average duration of life after the discovery of retinitis was four months, and many other figures of a similar nature are available. We shall see that the group of patients we are now considering contrasts sharply with the foregoing.

As regards the prospect of life, the prognosis is of course somewhat grave; it is, however, less grave and more uncertain than in renal cases; such a patient may at any time develop a cerebral apoplexy, but, on the other hand, he may live for several or many years; thus, of twenty-eight patients who were known to have died, the average length of life after the discovery of the retinitis was two years and eight months,³ whereas, as stated above, the average length of survival of the renal cases was four months only. Again, fifteen patients out of the twenty-eight, i.e., 53 per cent., lived for more than two years, whereas 6 per cent. only of Belt's renal cases lived for a similar period.

Whilst the prospect of life in these cases is very uncertain it will be seen how much better it is on the whole than in renal cases.

¹ *Journ. Amer. Med. Assoc.*, 1895, xxv, p. 735.

² *Trans. Ophth. Soc. U.K.*, 1888, viii, p. 134.

³ Or, if one takes living and fatal cases together, the average length of survival after the discovery of retinitis is *greater than three years and 6.6 months*; I say "greater than," for it is clear, that as the "at present surviving" cases die, the average length of survival of the group will be increased.

The second point under this head refers to the manner of death of these patients. I have no precise figures, nor have I been able to find any, as to the proportion of patients with renal retinitis who die in uræmia, but it is certainly a considerable number. Of the present cases there are three only in which either nephritis or uræmia is given as the cause of death. I have received a report as to the cause of death in twenty-eight out of the thirty patients who are known to have died. Of these twenty-eight, fourteen, i.e., exactly 50 per cent., are known to have died of a gross vascular cerebral lesion, and in addition, four of the seventeen who were alive when last heard of, were known to have developed such a lesion which had not culminated fatally. I do not think it can be doubted that the vascular lesion was the dominant lesion in these patients, and that whilst no doubt the kidneys shared in the general disease, their function was not sufficiently impaired by it to threaten life seriously.

Condition of the Urine.

Having regard to the frequency with which albuminuria is intermittent in this class of patient, and seeing that most of them were watched as out-patients, the most that can be said is, that in seventeen cases the urine was albumin-free on one or more occasions. In some cases six or eight examinations were made, but in others the urine was examined once only, so that, no doubt, had the examinations been more frequent, the number of cases in which albumin was present intermittently would have been increased.

In conclusion I suggest that:—

- (1) In a proportion of cases of general arterio-sclerosis, as the disease of the retinal vessels increases, exudates form in the retinal tissues which are probably dependent upon the local vascular disease.
- (2) That the ophthalmoscopic appearances resulting are in considerable measure distinctive.
- (3) That the prognosis implied by this form of retinitis as to length of life, is quite uncertain, but may extend to several or even many years, and that it differs greatly from renal retinitis in this respect.
- (4) That a large number of these patients die of a gross vascular cerebral lesion, according to the present investigation 50 per cent., and
- (5) That the condition calls for separate recognition, and that the term "arterio-sclerotic retinitis" seems appropriate.

In explanation of the table of cases now following, in the second column is given the age of the patient when he or she first came under observation. The third column gives the systolic blood-pressure and is in most cases the average of several readings taken at different visits. The fourth column includes some details with regard to the retinitis. The fifth column gives the ultimate history so far as it is known, the evidence with regard to the occurrence of cerebral vascular lesions, and the cause of death where this is known. Cases I to XXX are those of patients who are known to have died, and Cases XXXI to XLVII are those of patients who were alive when the last information with regard to them was obtained.

TABLE OF CASES.

	Age	B.-P.	Details of retinitis	Ultimate history
I J. B.	45	215	June, 1913, retinitis right eye only; left arteries thin threads, no exudate	Died November 11, 1913, five months after the discovery of retinitis, of "cerebral hæmorrhage"
II K. G.	49	215	January 30, 1914, star figure in right eye only; March 25, 1914, star still present	Died November 6, 1914, ten months after the discovery of retinitis, of a "cerebellar cyst"; post mortem
III A. C.	56	243	February 13, 1913, retinitis both eyes; November 27, 1913, retinitis increased, star figure left eye; January 15, 1914, retinitis still present	April 4, 1914, "three slight attacks of temporary loss of power of right arm and leg;" died November, 1915, two years and nine months after the discovery of retinitis; "a day or two before her death, which occurred suddenly, she had a hemiplegia; cerebral hæmorrhage"
IV M. A. W.	68	255	November 4, 1913, retinitis right eye	"One year ago awoke and found three fingers of the right hand were funny and numbed, she has not recovered full use of them"; died November 14, 1913, ten days after the discovery of the retinitis, of a "paralytic stroke"
V A. C. W.	71	230	July 14, 1913, retinitis right eye	"Loses the use of her legs"; died May 29, 1916, two years and ten months after the discovery of the retinitis, of "interstitial nephritis and uræmia"
VI A. C.	74	173	December 31, 1913, retinitis right eye	Died May 7, 1914, four months after the discovery of retinitis, "paralysed in speech and unconscious"
VII H. C.	54	220	December 14, 1912, retinitis both eyes; November 8, 1914, retinitis almost gone from both eyes with partial atrophy following œdema of the nerve	Died December 6, 1915, three years after the discovery of retinitis, of "cerebral congestion with symptoms of effusion"
VIII G. H.	60	265	October 20, 1913, retinitis both eyes	Died August, 1914, ten months after the discovery of retinitis, of "cardiac failure"
IX I. J.	50	290	January, 1913, retinitis left eye, with a star figure	June, 1913, "in Soho Square Hospital with a paralytic seizure"; died January 2, 1915, one year and eleven months after the discovery of retinitis, of "cerebral thrombosis"
X J. L.	53	210	February 3, 1914, retinitis both eyes, exudate along the radicles of the veins	Died February, 1915, one year after the discovery of retinitis, of "cardiac failure; a complication of diseases"
XI J. P.	53	205	April 30, 1913, retinitis with a star figure, left eye	Died October 4, 1913, five months after the discovery of the retinitis; "had a stroke, lost all power of his left side, died within twenty-four hours"
XII J. G.	63	235	March 6, 1913, retinitis, with a star figure beginning both eyes; December 18, 1913, thrombosis of artery has occurred in right eye with disappearance of exudate; left eye as before	Died April 14, 1914, one year and one month after the discovery of the retinitis, "in a fit"
XIII A. S.	63	—	March 4, 1915, partial star figure in right eye	Died September, 1915, six months after the discovery of the retinitis, of a "malignant growth of the liver"
XIV W. H. T.	66	—	October 25, 1913, retinitis right eye; February 22, 1914, still present	Died November 21, 1915, two years and one month after the discovery of the retinitis, of "chronic interstitial nephritis and uræmia"
XV G. H. Q.	67	240	February 26, 1913, retinitis right eye; September 20, 1913, still present; March 7, 1914, still present	Died February, 1915, two years after the discovery of the retinitis, "in his sleep; cardiac failure"

12 Foster Moore: *Arterio-sclerosis and Renal Disease*

TABLE OF CASES—(Continued).

	Age	B.-P.	Details of retinitis	Ultimate history
XVI O. R.	71	225	February 1, 1911, retinitis both eyes; January 3, 1914, still present; February 7, 1914, still present	Died January 16, 1916, five years after the discovery of the retinitis, of symptoms of "cancer of the liver"
XVII F. B.	65	240	October 21, 1913, retinitis right eye	Died November 23, 1913, one week after the discovery of the retinitis, of "cerebral hæmorrhage"; post mortem
XVIII E. C.	53	242	April 14, 1913, retinitis both eyes; March 12, 1914, retinitis reduced, both eyes	"Three years ago sudden attack of numbness running up both legs, went off after three minutes"; died February, 1917, three years and nine months after the discovery of the retinitis, of "symptoms of consumption"
XIX E. E.	55	—	March 13, 1914, retinitis both eyes, with a partial star figure; February 24, 1915, as before	Died October 14, 1915, one year and seven months after the discovery of the retinitis, of symptoms of "chronic renal disease and apoplexy"
XX A. A.	56	235	September 16, 1913, retinitis both eyes; March 3, 1914, still retinitis, both eyes	"Numbness in legs and thighs as if half dead"; died August 29, 1916, two years and eleven months after the discovery of the retinitis, of symptoms of "chronic nephritis"
XXI J. B.	65	300	June, 1913, retinitis left eye; November 4, 1914, still present	February, 1914, "had a stroke from which she has a good deal recovered"; died September 30, 1917, four years and three months after the discovery of the retinitis, "in a stroke, totally blind"
XXII J. G.	60	185	November 11, 1913, retinitis affected eye; March 7, 1914, still present	Thrombosis of central retinal vein; died March 4, 1918, four years after the onset of retinitis, of "consumption"
XXIII L. F.	52	237	June 12, 1912, retinitis right eye	Thrombosis of a tributary of the retinal vein; died April, 1920, seven years and ten months after the discovery of the retinitis, "of a stroke of which she had three"
XXIV A. H.	50	285	March, 1913, retinitis right eye; April 23, 1914, still present, a partial star figure	Died January 15, 1921, seven years and nine months after the discovery of the retinitis, of "diabetes mellitus"
XXV E. N.	55	—	—	Died nine years and six months after the discovery of retinitis, of "angina pectoris, no stroke"
XXVI C. S.	50	260	October 28, 1913, no retinitis; February 24, 1914, retinitis, right eye	Died April 5, 1914, ? cause; no evidence of a stroke
XXVII E. F.	68	225	Retinitis in eye without thrombosis only	Thrombosis of a tributary of the right retinal vein; died, date unknown, "suffered from cerebral hæmorrhage"
XXVIII W. T.	63	220	December 30, 1912, retinitis both eyes; April 2, 1914 (when last examined) thrombosis of left central artery occurred and all exudate disappeared, still present in right eye, in which thrombosis did not occur	April 2, 1914, "no stroke, can walk three or four miles at a good pace"; died January, 1916, three years and one month after the discovery of retinitis, cause not known
XXIX A. S.	47	240	August 7, 1913, retinitis both eyes	1911, "stroke, right arm and leg useless, speech unintelligible"; died December 24, 1915, one year and four months after the discovery of the retinitis, of "Bright's disease, end came quite suddenly"
XXX W. C.	48	194	March 13, 1914, retinitis both eyes; March 12, 1916; retinitis disappeared	March 12, 1916, "slight paralytic stroke all down right side one year ago, speech affected, laid up four months, right side of face drawn"; died January 7, 1918 (? 1917), three years and ten months after the discovery of the retinitis, "after another stroke within twenty-four hours"

CASES WHICH WERE ALIVE WHEN THE LAST INFORMATION WAS ACQUIRED WITH REGARD TO THEM.

	Age	B.-P.	Details of retinitis	Ultimate history
XXXI A. D.	47	180	May 24, 1913, retinitis right eye only, well-marked star figure; November 15, 1913, star figure much less conspicuous; February 14, 1914, star figure gone except for one or two minute dots; November 17, 1914, star gone completely; February 13, 1919, no star figure and no exudate; thrombosis has occurred in the left central retinal artery	October 18, 1913, "suddenly taken with shaking all down the right side, right foot seemed to drag and have no use in it, all right leg numbed"; November 17, 1915, "twitchings all down right side"; July 18, 1918, "acute right hemiplegia and aphasia, blood-pressure 193 mm., urine no albumin"; February 13, 1919, "has been in bed ever since 1918"
XXXII A. H.	48	215	November 28, 1908; "extensive arterio-sclerosis; no albumin" (Marcus Gunn); January 28, 1911, "gross vascular changes, white glistening spots disposed radially round the macula" (George Coats); September 1, 1919, hæmorrhages and exudate still present in each eye (see text for fuller reference, p. 8)	November 28, 1908, first seen by Marcus Gunn who noted extensive arterio-sclerosis, no albumin; January 28, 1911, first seen by George Coats who found retinitis and albuminuria; since this time she has been under my constant observation till September 1, 1919; May, 1918, "stroke left side of face, left hand, and left leg;" September 1, 1919, "still gets about, blood-pressure now 260 mm., hæmorrhages and exudate still present in each eye"; retinitis is known to have been present for eight years and seven months. (See text for a fuller account, p. 8)
XXXIII L. S.	65	197	May 30, 1913, retinitis right eye	Under observation for five years and six months, gives a good account of a mild stroke in 1910, and of a second in 1911; June 4, 1916, "no fresh stroke"; February 12, 1919, "too feeble to walk, eyes about the same"
XXXIV M. M.	60	160	June, 1913, no retinitis; August 29, 1913, star figure just beginning in the left eye; September 16, 1913, star figure increasing	March 15, 1916, "my health is better, my eye does not trouble me"; retinitis known to have been present for two years and six months
XXXV E. P.	60	223	February 14, 1913, scanty exudate which completely disappeared	February 28, 1916, "thoroughly fit in every way"; retinitis present three years before last seen
XXXVI E. B.	67	210	October 6, 1913, retinitis right eye; April 18, 1914, still present	March 5, 1916, "am very well in health and my eye improves"; January 15, 1919, "general health very good, never any stroke"; retinitis known to have been present five years and seven months before last report
XXXVII A. B.	66	175	May 20, 1913, retinitis right eye; September 2, 1913, retinitis increased	March 5, 1916, "still active and able to take walking exercise and can read"; January 29, 1919, "father in very good health and leads quite an active life, remarkably active considering his age, 71"; "no stroke"; retinitis present five years and nine months before last report
XXXVIII C. L.	55	175	February 4, 1911, no retinitis; April 5, 1913, retinitis both eyes; October 25, 1913, retinitis still in right eye; disappeared from left	April 4, 1914, "no stroke, is in very good health"; retinitis known to be present one year before last report
XXXIX S. R.	65	205	Retinitis, both eyes	Not traced; June 30, 1916, "death certificate has not been issued" (Registrar-General)

14 Foster Moore: *Arterio-sclerosis and Renal Disease*

TABLE—(Continued).

	Age	B.-P.	Details of retinitis	Ultimate history
XL M. R.	62	137	August, 1908, no retinitis; August 31, 1921, retinitis both eyes	Thrombosis of venous tributary, right eye; under observation thirteen years, developed retinitis whilst under observation; August 31, 1921, "no stroke, walks to hospital"
XLI A. B.	49	195	May 16, 1913, no retinitis; March 25, 1914, retinitis in affected eye forming a star figure; September 10, 1921; no retinitis	Thrombosis of central retinal vein; September 21, 1921, "perfectly fit, nerves a bit shaky at times, no stroke": under observation eight years and four months; no retinitis present seven years and six months after its first appearance
XLII E. G.	—	—	April 13, 1909, retinitis in affected eye only	Thrombosis of central retinal vein: January 13, 1914, "quite lost her sight and been ill many months, unable to leave the house since last January"; retinitis present four years and nine months before last report
XLIII R. W.	54	155	December 16, 1913, retinitis affected eye only; August 22, 1921, very little retinitis still present	August 22, 1921, perfectly fit, no stroke, looks very well, blood-pressure 155 mm.; retinitis known to have been present for seven years and nine months
XLIV R. H.	66	245	March 23, 1913, no retinitis; February 2, 1919, retinitis, right eye	February 2, 1921, no stroke at any time, walks to hospital, blood-pressure 220 mm.; under observation five years and ten months; retinitis seen on last visit only
XLV G. H.	66	165	April 4, 1911, retinitis both eyes	January 15, 1919, "general health very good, never any stroke"; retinitis present seven years and nine months before last seen
XLVI J. B.	50	220	June 12, 1913, retinitis in affected eye only	Thrombosis of tributary of retinal vein; nothing further known
XLVII C. P.	56	222	March 7, 1914, retinitis right eye; March 24, 1916; retinitis disappeared; January 18, 1919, no retinitis	January 18, 1919, a year ago awoke suddenly with a giddy feeling which has never gone off, blood-pressure 210 mm.; retinitis present four years and ten months before last seen

ANALYSIS OF CASES IN THE FOREGOING TABLE.

Forty-seven cases in all. Thirty of these are known to have ended fatally, and the cause of death is known in twenty-eight of them. Seventeen were alive when the latest information with regard to them was obtained, and of these there was satisfactory evidence that four had suffered from a gross vascular cerebral lesion.

Prognosis as Regards Life.—The average duration of life after the discovery of retinitis in the twenty-eight fatal cases was two years and eight months, and fifteen of them (i.e., 53 per cent.) lived for more than two years. The average duration of life of thirteen cases who were alive when the last information with regard to them was obtained was five years and five months. If one takes the forty-one cases, fatal and otherwise, in which the length of survival after the discovery of the retinitis is known, it works out at *three years and 6.6 months*.

Cause of Death.—Of twenty-eight fatal cases where the cause of death is known, fourteen, i.e., 50 per cent., are known to have died of a gross vascular cerebral lesion. In three cases only is nephritis or uræmia given as the cause of death. Of seventeen cases who were alive when the last information with regard to them was obtained, four were known to have suffered from a gross vascular cerebral lesion.

Retinitis.—Of forty-five cases the retinitis was unilateral in twenty-eight (i.e., 60.7 per cent.). In Cases XII, XXVIII, XXX, XXXI, XXXV,

XXXVIII, XLI, and XLVII, the retinitis disappeared whilst under observation.

Urine.—In seventeen cases at least the urine was intermittently free of albumin.

Mr. PERCY BARDSLEY (Salisbury) :

I have long held the views of Dr. Batty Shaw on the toxæmic origin of retinitis and sclerosis.

Mr. Foster Moore kindly sent me a copy of his paper to study in advance. I agree with all that he has written in that paper, but, in the short time allowed me this evening, I must bring forward a somewhat different view.

While admitting that the picture of arterio-sclerotic retinitis, which he has so carefully and ably drawn, is correct in every detail, I think that picture only applies to cases of great chronicity. In other words, the retinitis depends on the acuteness of the disease producing the sclerosis. If the toxin is of a more drastic nature, or if exacerbation of the disease takes place, then the picture produced is indistinguishable from so-called renal retinitis.

Now the classical changes in so-called renal retinitis are four: (1) The retinal oedema, resulting in radiating lines or in a macular star; (2) the fatty spots; (3) the hæmorrhages; (4) the high pressure signs in the vessels.

These four cardinal signs may all be present in three groups of cases without albuminuria, viz.: (A) In intracranial pressure; (B) in advanced arterial sclerosis, perhaps I should say acute arterial sclerosis; (C) in many toxæmias. For instance, I myself have seen: (a) Several cases of syphilitic retinitis; (b) two cases of post-influenzal retinitis; (c) one case of unknown, but supposed cerebro-spinal origin; (d) also two monocular cases of pyorrhæic origin. In all these cases the four classical signs were present, and without albuminuria; yet they were indistinguishable from renal retinitis.

Since, then, these retinal signs occur so frequently without albuminuria, and since albuminuria can *only* be diagnosed by urinalysis, and not by the ophthalmoscope, I ask: "Is it not time that this misleading term 'renal retinitis' should be abolished?"

How, then, does the ophthalmoscope help us in these cases? It informs us that there is a toxæmia causing high blood-pressure and vascular inflammation with its accompanying sequelæ. The toxin may or may not at the same time be causing albuminuria; this is shown by urinalysis. The ophthalmoscope shows us also what is of the utmost importance, viz., whether this inflammation is accompanied or unaccompanied by sclerotic changes in the vessels walls.

This is of the *utmost importance*, for the prognosis of life or early death largely depends upon it.

If the ophthalmoscope shows advanced sclerosis, together with gross retinitis and albuminuria, then I believe the termination of life may be forecast in months, or even in weeks. If, however, the retinitis shows only high blood-pressure with *little* sclerosis, then the chance of recovery and fair length of life is good; the poison may be evacuated, the blood-pressure reduced, and no gross sclerotic changes left in the vessel walls. Of course, if the toxin is not acute enough to produce a retinitis, or only a mild form of retinitis, the prognosis is not so grave, even with advanced sclerosis. The sclerosis is the index of the chronicity of the poison, not of its acuteness. If you reject for the army the man with the hypertrophied heart because he is less able to

endure, you should also increase the premium of the life assurance policy of the man with hypertrophied arteries. That man will not withstand the toxin that produces retinitis as a man with normal arteries will.

Here I must challenge one statement made by Dr. Batty Shaw. He says: "Thickening of the middle coat produces the silver or copper-wire appearance of the retinal arteries." Now silver and copper-wire have very different sheens, and the respective sheen of each bears a very different interpretation. To-day I may see a patient whose arteries show neither. Next week, after a severe influenza, I see that patient with broad copper-bright light streaks and indented veins. The brachial blood-pressure reads 160 to 170. Have those arteries become sclerosed in two or three days? No! In a few weeks they may be back to normal.

Is it possible, then, to distinguish between the signs of simple high blood-pressure and the signs of arterio-sclerotic changes? I have stated in the past that this is possible, and, after five years' interval, I confidently reiterate that statement.

I would also emphasize a point that I think is not clearly grasped, namely, that one can detect sclerosis in the vessels when high blood-pressure is not present, and can thus forewarn the physician and the patient. Of this the following is rather a striking illustration:—

In 1911 a medical man sent his wife to me for refraction. Her age was 31. They had one child, aged 11.

After careful observation I wrote him that in my opinion it would be unwise to risk a further pregnancy, as her vessels showed considerable sclerosis. He was naturally very upset, and he took her to a well known physician, who found her blood-pressure a little under 130, and while he thought this perhaps a little high for her age, attached no significance to it.

A little later the lady became pregnant. At seven months albuminuria and eclampsia set in: the child was delivered, and died shortly after birth; the wife took more than a year to recover. She is still alive, and enjoying moderate health.

MR. PHILIP ADAMS (Oxford).

With reference to the so-called "renal" retinitis, this condition appears to be rare in the district in which I work. I hardly ever see a case nowadays, either at the Radcliffe Infirmary or the Eye Hospital, and yet I remember seeing it fairly often some years back. Only last week I was asked to examine the eyes of a man in the infirmary with chronic interstitial nephritis, but the condition present was one of arterio-sclerotic retinitis in one eye, thickened arteries and a hæmorrhage in the other. In contrast to this, towards the end of the war I was asked to see a man with a wound in the hip-joint, who was complaining of blurred vision; he had absolutely typical "albuminuric" or "renal" retinitis, slight papillædema, soft cotton-wool patches and hæmorrhages, with a stellate figure at each macula; but in spite of repeated examination of his urine nothing abnormal was found, except a very slight trace of albumin on the first occasion. His wound was very septic and draining badly, and he was extremely ill, but after amputation and free drainage he quickly recovered his health and sight, and he is alive and well at the present time. This case, shows, I think, that toxæmia can of itself cause the condition known as "renal" retinitis, without involvement of the kidney.

With regard to Mr. Foster Moore's propositions, I quite agree that there is

a distinctive condition of the fundus oculi, which he has named arterio-sclerotic retinitis, but I am inclined to think that the explanation of the condition given by Dr. Batty Shaw is likely to prove the correct one.

Again with regard to the length of time these patients live, my experience is similar to that of Mr. Foster Moore. Some years ago¹ I collected 159 cases of retinal vascular disease, exclusive of true "renal" retinitis, but including arterio-sclerotic retinitis and other retinal lesions associated with arterio-sclerosis, and I found that the patients in many of these cases lived to an advanced age provided their urine was free from albumin, whereas if albumin was present, eight or nine years was the maximum and this was quite the exception. Again, speaking generally, the older the patient at the time of onset of the eye symptoms, the less serious was the prognosis.

The more I ponder over my cases of vascular disease of the retina in arterio-sclerosis, the more convinced am I that one cannot make any prognosis on the eye condition alone; this must depend on the associated condition of the heart and kidneys. What is wanted, it seems to me, is, a comparison of the length of life between arterio-sclerotic patients who show no retinal change and those that do, because not all cases of arterio-sclerosis show distinguishing retinal changes, though the majority do so; and then one could form some idea of the true significance of these changes. This research could only be carried out conjointly by a physician and an ophthalmic surgeon working together.

Dr. ARTHUR ELLIS.

At the London Hospital, Dr. Marrack and I are attempting to determine the relationship between disturbance of renal function and the occurrence of retinitis. With this end in view we are making a careful study of renal function in all patients in whom retinitis is determined. The tests of renal function employed consist in observations as to the presence or absence of albumin and casts, the determination of blood urea, the estimation of the power of excretion of phenol-sulphone-phthalein, the urea concentration test and observations on the occurrence or non-occurrence of fixation of the specific gravity of the urine. Up to date, twenty cases have been examined and the results obtained are shown in the following tables:—

Examination of these tables reveals two facts of major importance, first the constancy of high blood-pressure in these patients and second the possibility of separating them into two groups, one with gross disturbance of kidney function, the other without evidence of such gross disturbance.

(1) *High Blood-pressure*.—With one exception all the patients with retinitis examined showed high blood-pressure. In only three of the twenty was the systolic pressure less than 200: in one it was 180, in one 160, while in one patient the relatively low pressure of 148 mm. of mercury was found.

(2) *Differentiation of Cases according to Renal Function*.—In Table I are given those cases of retinitis in which gross disturbance of renal function was determined. It will be seen that in all nine there was marked urea retention, the figures for blood urea being in all these patients over 100 mg. per 100 c.c. There was also gross disturbance of phenol-sulphone-phthalein (P.S.P.) excretion, in six of the nine patients the excretion being either *nil* or only a trace. The urea-concentration test also, in every case in which it was carried out showed marked impairment of renal function, the ability to concentrate urea in the

¹ See *Brit. Journ. Ophth.*, 1917, i. p. 161.

TABLE I.—RETINITIS WITH GROSS DISTURBANCE OF RENAL FUNCTION.

Sex	Age	Duration of visual symptoms	Edema	Blood-pressure	Albumin	Casts	Blood-urea	P.S.P.	Urea concentration test	Fixation of specific gravity	Remarks
F.	42	9 months	Slight	240/120	2/3	None seen	0.496	0	18 c.c. = 0.84 per cent.	No	Died, uremia
F.	25	None	Slight	160/100	Cloud	None seen	0.400	0	" = 0.9 "	No	Dying, uremia
M.	38	2 months	None	210/160	1.5	Granular and hyaline	0.480	Trace	124 c.c. = 1.14 "	Yes	Died, uremia
M.	46	1 year	Moderate	225/150	1.5	Granular	0.141	Trace	104 c.c. = 1.1 "	Yes	Died, broncho-pneumonia
M.	29	3 months	None	200/140	2.5	Granular and hyaline	0.107	Trace	" = 1.1 "	Yes	Died, uremia
M.	30	3 months	None	230/135	2.5	Granular and hyaline	0.141	Trace	" = 1.1 "	Yes	Died, uremia
F.	43	2 months	Slight	280/	1.6	Granular	0.109	6 per cent.	80 c.c. = 0.87 "	No	
M.	23	1 week	Slight	240/170	2.3	Granular and hyaline	0.171	7 per cent.	62 c.c. = 0.96 "	Yes	
F.	25	3 months	Slight	210/160	1.5	Granular and hyaline	0.120	7 per cent.	58 c.c. = 1.25 "	Yes	

Total number = 9. Average age = 33. Average B.P. = 220.

TABLE II.—RETINITIS WITHOUT GROSS DISTURBANCE OF RENAL FUNCTION.

Sex	Age	Duration of visual symptoms	Edema	Blood-pressure	Albumin	Casts	Blood-urea	P.S.P. ¹	Urea concentration test	Fixation of specific gravity	Remarks
F.	54	2 years	Slight, both legs	290/170	1.3	Granular	0.080	15 per cent.	93 c.c. = 1.86 per cent.	No	Early cardiac failure
F.	36	1 month	None	180/120	Cloud	Hyaline	0.043	26	" = 1.44 "	No	Onset in pregnancy
M.	56	3 months	None	250/160	Cloud	Granular and hyaline	0.045	23	" = 2.1 "	No	Hemiplegia
F.	51	6 months	None	230/170	Trace	None seen	0.040	32	" = 2.2 "	No	Died, "cardiac failure"
M.	54	Moderate (heart failure)		210/160	1/8	Many, hyaline	0.035	30	" = 2.2 "	No	Died, cardiac failure
M.	37	3 months	None	250/160	1/10	Hyaline	0.030	30	95 c.c. = 2.3 "	No	Died, cerebral hemorrhage
F.	43	2 years	None	280/180	Cloud	Hyaline	0.020	48	60 c.c. = 2.28 "	No	
M.	57	2 months	None	280/	Trace	Granular and hyaline	0.048	49	128 c.c. = 2.25 "	No	
F.	35	None	None	225/160	None	None seen	0.036	52	" = 2.3 "	No	Hemiplegia, now marked loss of vision
F.	30	1 month	None	220/150	1/10	Granular and hyaline	0.026	55	76 c.c. = 1.98 "	No	Onset in pregnancy
M.	49		None	145/85	Trace	Hyaline	0.040	68	73 c.c. = 2.3 "	No	

Total number = 11. Average age = 46. Average B.P. = 235.

¹ The phenol-sulphone-phthalein employed in these tests gives a low figure for the normal, 40.70 per cent., instead of the 60.90 per cent. given by that supplied by Hynson, Westcott and Dunning, on which most normal figures are based.

urine being in the neighbourhood of or under a concentration of 1 per cent., instead of the normal of over 2 per cent. In six of the nine patients there was fixation of the specific gravity of the urine. Four of these nine patients are known to have died of uræmia, one is dead of broncho-pneumonia and two others are dying of obvious renal inefficiency.

In Table II are given the findings in those cases of retinitis in which evidence of gross disturbance of renal function was lacking. There are eleven patients in this group. In only one was there any evidence of nitrogen retention: this patient had on admission 80 mgm. and two months later 60 mgm. of urea per 100 c.c. of blood. It will be noticed that in this patient the phenol-sulphone-phthalein figure was also the lowest for any patient in the group and the urea concentration also below normal. The patient was suffering from cardiac failure and it is probable that the poor figures for renal excretion were in part dependent on the circulatory failure. In the other ten patients the figure for blood urea was within normal limits. The phenol-sulphone-phthalein excretion was less than the normal in more than half the cases in the group, but with the exception of the patient showing urea retention it did not approach the condition found in cases of advanced renal disease.

The results of the urea-concentration test in this group of patients was particularly interesting. In only three was the figure below the normal 2 per cent. One of these three was the patient already mentioned with urea retention and low phenol-sulphone-phthalein, the other two were both cases of retinitis occurring in pregnancy—a retinitis recognized by the ophthalmologists as different on account of its much more favourable prognosis. None of the patients in this group showed fixation of the specific gravity of the urine. Of this second group of patients three only are known to be dead, one of cerebral hæmorrhage and two of cardiac failure. It is to be noted that two other patients in this group are hemiplegic.

We see then that patients with retinitis may be divided into two groups, one showing gross impairment of renal function, the other not. In the former death usually occurs relatively soon after the patient seeks admission to a hospital, the common termination being uræmia. In the latter, life is more prolonged but vascular accidents are frequent and are probably the common eventual cause of death.

With what are we dealing in these two groups? This opens up an interesting question. Are they the same disease, one being an advanced stage of the other, or are they two separate and distinct diseases? Is one primarily a renal disease with a secondary rise of blood-pressure, and the other primarily a high-pressure disease with renal involvement merely as a secondary result?

If the renal cases do represent the end stage of a disease the earlier manifestations of which are seen in the "vascular" group, then we should expect the patients listed in Table I to be older and to have higher blood-pressures than those in Table II. The reverse proves to be the case, the average age in the "renal" group being thirteen years younger than in the "vascular," and the average blood-pressure in the "vascular" group 15 mm. higher than in the "renal." This is in keeping with experience and in agreement with the generally accepted differentiation of two diseases, (a) chronic nephritis with high blood-pressure, and (b) essential vascular hypertension with secondary renal damage. Is there a different ophthalmoscopic picture in these two conditions? Will these ophthalmoscopic appearances coincide with Mr. Foster Moore's renal retinitis on the one hand and arterio-sclerotic retinitis on the other? These are questions which require answering, and to-day's discussion should help to hasten their solution.

Dr. C. O. HAWTHORNE.

I propose to limit my remarks to the various descriptive or diagnostic terms that have been proposed in the debate: these terms have been suggested as appropriate when retinitis is associated with renal disease on the one hand, and, on the other, when retinitis is free from that association but exists in the presence of more or less conclusive evidence of arterial degeneration. In Dr. Batty Shaw's view the retinitis existing in these two sets of circumstances is one and the same: it is due to a toxic condition of the blood, and is produced by a direct action of the toxin on the retinal tissues. Hence Dr. Batty Shaw wishes to call the condition toxic retinitis. On the other hand, Mr. Foster Moore recognizes two forms of retinitis, though he admits he is not always able to distinguish the one from the other. One form Mr. Foster Moore regards as a result of advanced degenerative change in the retinal vessels, and he therefore calls it arterio-sclerotic retinitis; and the other he takes to be an expression of renal disease, and hence he applies the term renal retinitis.

I challenge both sets of proposals, not on the ground that the underlying propositions are not true, but on the ground that we do not know them to be true. Hence it follows that terms implying their truth ought not be admitted to a scientific vocabulary.

Each of these terms—toxic retinitis, renal retinitis, arterio-sclerotic retinitis—involves an undemonstrated speculation or hypothesis, and this consideration alone is sufficient to condemn such terms as bad and as inadmissible to a scientific nomenclature; for scientific terms, if pretending to be descriptive, should be descriptive of facts and not of opinions about facts. Already in medicine there are more than sufficient areas within which confusion is perpetuated and counsel darkened by words without knowledge; and any extension of this method should be resolutely resisted.

Dr. Batty Shaw knows nothing of his hypothetical toxin, except from what he judges to be its effects; he cannot tell what the toxin is, nor where it comes from, nor how it acts. It is one thing to teach or to argue in favour of a certain theoretical explanation, and quite another thing to impose this explanation as settled doctrine by incorporating it in a descriptive title.

Similarly, Mr. Foster Moore's terms are obviously speculative or hypothetical. In time they may turn out to be justified, but at present they are far in advance of the facts. There is the more reason for circumspection here, seeing that Mr. Foster Moore allows that the two pictures he draws are not in all cases confidently distinguishable the one from the other.

In a word, it may be urged that into clinical names and phrases we ought not to introduce terms implying a confident knowledge of causation, when, as a matter of fact, such knowledge is not in our possession.

Dr. J. F. GASKELL (Cambridge).

I will endeavour to give my views as shortly as possible on those forms of disease which bear upon the present discussion.

The point in this discussion I want to emphasize above all others is that two conditions of entirely different pathology are concerned: one being primarily a disease of the kidneys, the other of the vascular system as a whole. It is inevitable, owing to the close interdependence of these two systems, that lasting disease of one should affect the other, so that a composite picture is ultimately formed in which both systems are affected.

Dealing first with the condition which is primarily renal, I hold that the initial occurrence is an acute inflammation of the whole organ, in which every structure of which it is built up is involved—glomeruli, tubules, their supporting or interstitial tissues, and very probably the intrinsic blood-vessels of the organ also.

There are various stages of this diffuse nephritis with which are usually associated those forms of retinitis called albuminuric, or better, renal retinitis.

The great damage which the kidney thus undergoes calls for an increased efficiency in the circulatory system in order that the excretory function should be adequately carried on, and cardiac hypertrophy and increased blood-pressure result. The essential pathology is still however renal, and the one danger to be apprehended in the later stage, where such an hypertrophy has been successfully brought about, is renal failure in the form of uræmia.

The second condition is a cardio-vascular one in which three factors invariably take part—a raised blood-pressure, cardiac hypertrophy and disease of the small arterioles of certain organs of the body. Organs which are always affected are the brain and the kidney, around which the discussion turns to-night. It is to this class that I consider Mr. Foster Moore's series of cases belongs, and I am fully in agreement with him that they should be separated from any connexion with renal conditions. Another point that I want particularly to emphasize is that in the purest examples of this condition the vascular changes, which are degenerative in nature, are entirely confined to the small peripheral arterioles. The largest arteries are wholly free from any such change; on the contrary they frequently manifest a true muscular hypertrophy enabling them to cope with the increased pressure of the blood-stream.

In these cases the cause of death is almost always to be traced to the changes which occur in the brain, cerebral hæmorrhage being by far the commonest occurrence. There is, however, another group of events depending upon relative failure in the cerebral circulation, which also leads to death. The disease of the cerebral arterioles, especially in the medulla, becomes so great that a local cerebral anæmia is brought about if the high level of the blood-pressure is not maintained. Death often follows without gross cerebral lesion, but with signs of respiratory failure, such as Cheyne-Stokes breathing, &c. This condition must not be confused with uræmia.

With regard to the cause of this condition and the sequence of the three chief events, it is very difficult to form an opinion as to whether the arterio-sclerotic changes of the arterioles are secondary to the high blood-pressure, or whether the high blood-pressure is a response to obstruction to the peripheral circulation through essential organs. I do not myself consider that the invoking of a "toxæmia" without any evidence of its nature adds to the clearness of any view of the pathology of this condition. I am, however, in hearty agreement with the opener of the discussion that the changes are due to some cause which acts upon the circulation as a whole, and that the sooner terms such as chronic interstitial nephritis, which imply that the kidney is the primary cause, are given up, the better. The term I incline to adopt for the condition is one which we owe to Sir Clifford Allbutt, namely, "hyperpiesis," which both draws attention to the circulatory system as the primary system involved, and emphasizes one, and perhaps the primary, of the factors which constitute the pathological entity.

(The Discussion was adjourned till December 8, 1922.)

Sections of Medicine and Ophthalmology.

JOINT MEETING.

Chairman—Mr. A. L. WHITEHEAD, President of the Section of Ophthalmology.

ADJOURNED DISCUSSION ON "THE SIGNIFICANCE OF VASCULAR AND OTHER CHANGES IN THE RETINA IN ARTERIO-SCLEROSIS AND RENAL DISEASE."

Mr. ERNEST CLARKE.

I think we shall find that when their statements have been thoroughly confirmed the excellent papers of Dr. Batty Shaw and Mr. Foster Moore will be really "epoch-making." What many of us have been long suspecting has at length materialized. Those who can go back as many years as I can will remember that we were taught (and believed all this time until recently) that high blood-pressure was the *cause*, or one of the causes, of so-called "hæmorrhagic retinitis" in the majority of cases. We sent our patients to a physician for him to lower the blood-pressure, and fortunately for the patient, even bleeding did not do this. Now we are beginning to see that high blood-pressure is *not* the cause, but *one* of the *symptoms* (we may even believe that it is a *protective measure*), and we must go further back for the cause which in most cases is some form of toxæmia. This being so, is not the term "hæmorrhagic retinitis" (which most of us disliked because of its unscientific style) probably the best term we can use at present as indicating the trouble and not binding us to any theory (and thus it would, I suppose, meet with the approval of Dr. Hawthorne).

Mr. Foster Moore, under "Proposition 2," makes this remark:—

"If, as I believe, the presence of exudates in the retina implies a stage of arterio-sclerosis in advance of a case in which vascular disease alone is evident, and if, as I am sure is the case, the disease of the retinal vessels increases *pari passu* with the general arterial disease, then a comparison of two groups of patients—in one of which vascular disease alone is present, and in the other of which exudates are present in addition to the vascular disease—should provide evidence that the latter group is composed of patients suffering from a further advanced stage of the disease."

This of course tallies with the statement made by Dr. Batty Shaw, who suggested that the different stages of disease are due to the amount of poison, and the time during which it is acting.

With reference to the so-called "exudates," why should they be present in one eye and not in the other? Is it due to the same cause which we think

[December 8, 1922.]

operates in unilateral hæmorrhage, namely, that the tension of the affected eye is lower than that of the other? It would be a useful investigation if, in the future when we find hæmorrhages or exudates, or both, in one eye only, we were to take the tension of both eyes with a tonometer.

We ophthalmic surgeons are quite familiar with retinal hæmorrhages occurring when the tension of the eye is lowered; it is an unfortunate accident which may occur in doing an iridectomy for glaucoma, or extracting a cataract, but we have always assumed in these cases that the vessels were diseased.

Dr. Batty Shaw has very rightly hinted that the thickening of the middle coat and of the intima strengthens the vessels, and we have sufficient evidence to show that this thickening or sclerosis is inflammatory in origin and is only followed by degenerative changes if the blood supply is deficient. Both the openers of this discussion have suggested that the hæmorrhages do not come from ruptured vessels. Dr. Batty Shaw suggests that a hæmorrhage may be of the nature of a leakage from the capillaries; this would seem to be confirmed certainly in those cases in which small hæmorrhages are unaccompanied by any retinitis, and which disappear without leaving any trace, and appear to be unassociated with any constitutional disease. Such cases, I take it, constitute an example of the first stage which, if allowed to continue, may develop into so-called "arterio-sclerotic retinitis," which itself, if allowed to continue, or if the toxins are increasing, may lead to renal disease, and so eventually to the final stage of "renal retinitis."

Dr. W. N. GOLDSCHMIDT.

The success of any attempt to divide up cases of retinitis into those due to renal "mischief" and those due to other disorders depends partly on the decision as to what symptoms and signs, apart from retinitis, justify the labelling of a case as "renal." The following record illustrates the difficulty of this problem.

It is of interest from the point of view of the relationship between the symptoms and signs of "parenchymatous nephritis" and the condition actually found in the kidneys, both during the disease and at the post-mortem examination. The effect of decapsulation and other remedies is also discussed.

On January 28, 1922, a man aged 42 was admitted to hospital complaining of swelling of his legs for seven weeks and of his "stomach" and back for six weeks. Additional symptoms were very troublesome, viz., flatulence, shortness of breath on exertion and occasional headaches. The only serious illness from which he had suffered was a febrile attack ("influenza") in Egypt in 1918. There was no venereal history.

Condition on examination: There were great pallor and marked œdema of legs, abdomen and back, and also ascites. The cardio-vascular system showed no abnormalities, though the heart sounds were very faintly heard. The systolic blood-pressure was 132 mm. In the respiratory system there was evidence of some pleural effusion at both bases, especially the right. The tongue was furred, the tonsils rather injected and the gums showed pyorrhœa. The abdomen was distended, not only by fluid but by meteorism as well, especially in its upper part. He complained of a dull pain all over his abdomen and great flatulence after meals. Urine: About 200 c.c. were passed in twenty-four hours, specific gravity 1030, brown in colour, acid, and containing about 2 per cent. of albumin: microscopic examination showed red blood cells and numerous granular, hyaline and epithelial casts: the urea-concentration by MacLean's test was nearly normal, averaging 1.7 per cent. The blood-urea was within normal limits (33 mg. per 100 c.c.). The Wassermann reaction was negative. The fundi, examined ten days after admission, were normal.

Treatment at first consisted of rest in bed, salt-free diet and a diuretic mixture containing potass. acetat., tr. scillæ, sp. aether. nit. and succ. scoparii. In four days the quantity of urine passed in twenty-four hours rose to 660 c.c. On the sixth day hot-air baths once daily were used, but no satisfactory perspiration was produced. The bowels were also very obstinate, and large doses of jalap, given with hot water, were required to achieve any result. On the tenth day, urea (1 dr.) was given four times a day for a few days, without any effect on diuresis, and then diuretin (20 gr. t.d.s.), but with no result: the urine continued to average about 500 c.c. daily. He was drinking fairly freely, but little fluid was excreted, either by the skin or by the bowels. The flatulence was very troublesome, and had to be relieved by hot applications and carminatives.

Since no progress was being made by the aid of medicinal and dietetic measures, and since the history was comparatively short, it was decided that the effect should be tried of decapsulating a kidney. Accordingly three weeks after admission his right kidney was dealt with in this way by Mr. Gwynne Williams. One kidney only (the right) was operated upon to begin with, in order to watch the result. The kidney was found to be of normal size and colour: a slit was made in the capsule (which was not tense) and a small piece of kidney substance was removed for examination: the capsule was then separated from the kidney all round. The peritoneal cavity was opened and 2 to 3 pints of ascitic fluid allowed to escape: this was milky and opalescent from the presence of lecithin-globulin (Dr. Mackenzie Wallis). The intestines were pale but there was no visible peritonitis.

Four days later the urine had risen to 900 c.c., and the œdema was rather less: ten days after the operation it rose suddenly to 1,200 c.c. (albumin, 0·36 per cent.), but within four days it dropped as quickly to 600 c.c. Urea was now again tried and pushed in doses of 15 grm. t.d.s. except when the patient was sick; the urine now increased within eighteen days to 1,600 c.c. in twenty-four hours and then averaged 1,200 c.c. for about twelve days. The general condition of the patient was, however, but little altered and the œdema not materially diminished. The abdomen and scrotum were again tense.

With the idea that the increased urinary output might have been due partly to the operation, it was decided to decapsulate the left kidney. Accordingly, on April 5, 1922, seven weeks after the first operation, a second operation was performed by Mr. Gwynne Williams. The kidney and its capsule were normal in appearance and size: the capsule, which was not tense, was slit and retracted: the peritoneal cavity was opened and several pints of milky fluid like that at the first operation were removed. The intestines were pale but otherwise normal. For the next two days the quantity of urine dropped to 700 c.c., albumin still being present. On the third day the urine rose again to 1,350 c.c., but after that it steadily decreased in amount and the œdema reaccumulated. Three weeks after the operation an attempt was made to reduce the œdema by giving him less to drink (only about 1½ pints daily), salt-free diet, purgatives and theocin sod. acet. 4 gr., tr. digitalis 7½ minims, and sod. sulph. 15 gr. t.d.s.: the increasing oliguria could not be stayed.

On April 27 the blood-urea had risen to 48 mg. in 100 c.c. The abdomen was now very distended again and vomiting started. On May 9 everything else having failed to relieve the œdema it was considered justifiable to insert Southey's tubes into his feet, which was done with scrupulous aseptic precautions. A very large quantity of fluid was drained off (4 pints on the first day). The abdominal distension, which was partly gaseous, was not relieved. The chlorides in the urine were distinctly diminished.

After a few days the feet became red and painful: the temperature dropped to 95° F.; the blood-pressure, which had been 132 mm. Hg on admission, soon dropping to 120 mm. where it remained, now fell to 90 mm., and he became drowsy and sometimes semi-comatose. On May 30 his blood-urea had risen to 63 mg. in 100 c.c. The albumin in his urine on June 1 was 0·5 per cent., on June 2, 1·25 per cent., and on June 3, 2·5 per cent. He was now somnolent, often sick, and there was evidence of cellulitis in the legs. He died on June 5.

Autopsy.

Externally a few petechiæ were seen on the arms. Incisions into the dorsa of the feet showed the cellular tissues to be distended with pus. Heart was small, only 5½ oz. in weight, and atrophic. The heart muscle was dark brown. A few patches of atheroma were seen in the aorta and coronary arteries. Lungs, &c.: Each pleural sac contained about 6 oz. of clear fluid: scattered fibrous adhesions on both sides. The lungs were congested and œdematous. Peritoneal cavity contained a large quantity of turbid ascitic fluid; considerable flakes of fibrinous exudate on the peritoneal surface. Intestines: Sigmoid colon contracted, apparently by thickening of bowel walls; mucous membrane showed slaty-grey patches, and was œdematous and congested; no ulceration or evident scarring; the affected area was sharply marked off from the contiguous normal gut.

Kidneys: The left kidney, normal in size, was removed and injected with preservative fluid soon after death. The right kidney, of normal size and weighing 5 oz., was not injected with preservative fluid; the surface was smooth and rather pale; the cortex and medulla were normal, and so were the vessels.

Microscopic Sections of the Kidneys.—(Described by Mr. T. W. P. Lawrence.)

"(1) *Section of right kidney, piece being removed at the first decapsulation operation:* (a) There is localized cloudy swelling and necrosis of the tubules, probably of an acute nature; adjacent areas are quite normal. (b) There are some slight chronic changes shown by thickening of the capsules. (c) There is proliferation of the epithelium of the glomerular tufts. (d) There is slight increase of the interstitial tissue between the tubules in places. (e) There are small localized effusions of red corpuscles.

(2) *Section of right kidney, piece being removed at the post-mortem examination, the organ not having been injected with preservative fluid:* (a) All acute changes above described are absent. (b) Chronic changes are shown by thickening of the capsule, only slight, as above. (c) The numbers of nuclei in the glomerular tufts are fairly normal. The corpuscles are not so large as in the above section. (d) Slight interstitial changes between the tubules, as in the above section. (e) No effusion of red corpuscles. (There is considerable autolytic change in the tubular epithelium.)

(3) *Section of left kidney, piece being removed at the post-mortem examination, the organ having been injected with preservative fluid very soon after death:* The microscopic details of this kidney were identical with those met with in the right kidney (*see* (2) above), no acute cloudy swelling or necrosis being visible in the tubular epithelium. The autolytic changes were minimal.

Summary.—The sections of the piece of kidney removed during life showed slight but definite changes, such as could be produced by some toxic agent arriving by the blood-stream. The sections of the pieces removed after death only showed the slight chronic changes met with in the sections of the piece removed during life, and were very little different in appearance from those of a normal kidney."

Comment on the Case.

This man died from an illness, the symptoms of which were typically those which have been attributed to "chronic parenchymatous nephritis"; but his kidneys were found at the post-mortem examination both to be normal macroscopically and practically normal microscopically. His actual death was perhaps due to or hastened by the infection which followed insertion of Southey's tubes, but he was steadily progressing to the inevitable end before the use of these was reluctantly resorted to. The section from the piece of right kidney taken during life at the first decapsulation operation showed some parenchymatous changes of an apparently acute nature. As the right kidney showed none of these appearances at the post-mortem examination, decap-

sulation may have restored the kidney, but did not improve the pre-renal toxæmia.

I am forced to the conclusion (unless in such cases one thrusts aside as valueless *all* microscopical evidence of the presence or absence of disease) that this long, insidious and fatal illness was not accounted for primarily by the state of his kidneys. The heart was very small and in a condition of brown atrophy; this organ, therefore, though the condition of brown atrophy is always looked upon as a secondary phenomenon and not a primary *morbus cordis sui generis*, actually showed considerably more evidence of disease than the kidneys. Even supposing the kidneys *had* shown parenchymatous changes at the post-mortem examination, there would be no more reason for considering the kidney degeneration primary than that of the heart. Incidentally, it must be remembered that there were no signs of renal inefficiency during the earlier and greater part of his illness, except oliguria (which occurs in heart disease), albuminuria (which occurs in 6 per cent. of healthy adults), and diminished excretion of chlorides (which occurs also in fevers, especially pneumonia, and is not incontestably the result of damaged kidneys). Furthermore, the œdema is not easy to explain as due entirely to the retention of chlorides, since œdema is not a feature of pneumonia, and does not even occur in obstructive suppression of urine or after removal of the only kidney. It is of great importance to note that, although towards the end of his illness the kidneys were only slightly damaged or possibly not at all, the blood-urea rose, so that an abnormally large amount of urea may be present in the blood with but slight or no changes in the kidneys.

It would, therefore, seem as if this man's illness were due to some pre-renal poison which caused exudation into the tissues, and incidentally damaged some of the tubules of the right kidney, but these latter had recovered by the time the man died.

I wish to thank Dr. Batty Shaw and Mr. Gwynne Williams for kindly allowing me to make use of their case reports, and Mr. Lawrence for his description of the microscopical sections.

Mr. D. LEIGHTON DAVIES (Cardiff).

I suppose the majority of ophthalmic surgeons will agree that there is a distinct clinical difference between a hæmorrhagic retinitis due to, or associated with, marked arterio-sclerosis on the one hand, and a retinitis due to or associated with albuminuria on the other. On the one hand we see a fundus in which the disc has a deep brick-red colour, with round, irregular or flame-shaped hæmorrhages scattered about the retina, but mostly somewhere in the neighbourhood of the blood-vessels; (it may be also that near these same vessels a few small white spots can be seen); arteries glistening and rather tortuous, and perhaps uneven in calibre, whilst the veins are full. Such is the clinical picture of a retinitis associated with arterio-sclerosis.

On the other hand we have a fundus, of which the disc is perhaps of a pink colour and the edges tend to be a little feathery; a few hæmorrhages may be scattered about, but the striking feature consists in the patches of white exudate large or small, irregular in shape and distribution, or arranged symmetrically about the macula: such is the picture of an albuminuric retinitis, so-called. But not infrequently we meet with cases which are not only difficult to place, but which, while resembling one or another of these two clinical types, are the result of some totally different cause.

Again, these two clinical groups are quite distinct in the significance which

they bear with respect to the prognosis of life. In the case of the arterio-sclerotic type we know quite well that the expectation of life is much greater than in the albuminuric type.

In face of all these considerations I really cannot see that the views enunciated by Dr. Batty Shaw are in any way helpful in the differentiation or prognosis of arterio-sclerotic and renal retinitis. In effect he says that arterio-sclerotic and renal retinitis cannot be differentiated. Let us assume for a moment that the arterio-sclerosis and the nephritis are but different aspects or results of some hypothetical toxæmia, as indeed they may be. In one person this toxæmia produces an arterio-sclerosis, in another a nephritis. Or again, it may be assumed that one form of toxæmia is responsible for the arterio-sclerosis and another toxin lies at the root of the nephritis. But whatever may be the originating cause, they have at any rate produced two entirely different clinical varieties (I use the word clinical advisedly, as opposed to pathological), each of which has its own prognostic significance. And this, again, is in consonance with what we know of other forms of toxæmia, such, for instance, as infections produced by the pneumococcus. At one time it may produce a pneumonia, at another time a meningitis or, again, a synovitis. So that although we have a common origin, yet we have widely differing clinical pictures, each having different prognostic significance.

I will now refer to one aspect of arterio-sclerotic retinitis which has already been touched upon, the relationship between sclerosis of the retinal vessels and the condition of the vessels supplying the brain. A few years ago I read somewhere that the cerebral vessels may be distinctly atheromatous without degeneration showing itself in the retinal vessels. This led me to make a systematic examination of a number of cases of hemiplegia due to cerebral arterial disease, with a view to confirming or disproving this statement. This investigation was, unfortunately, interrupted by the war, and I have not had the opportunity of taking it up again. Of fifteen genuine cases of apoplexy which I examined, nine cases showed marked arterio-sclerosis, the most marked being in a female aged 41, who had a systolic blood-pressure of 230 mm. Hg and a trace of albumin in a urine of low specific gravity, together with hæmorrhagic retinitis. In four cases the signs of retinal arterio-sclerosis were slight, whilst in two cases the arteries appeared to be perfectly normal. One of these cases was that of a woman aged 45 who had been hemiplegic for three years, in whom the blood-pressure was only 160 mm. Hg (D. 120). The other case was that of an old man aged 71 who had had a stroke one year previously, and whose blood-pressure was only 160 mm. Hg (D. 110). But what is more germane to this discussion was the fact that out of these fifteen cases only one patient showed hæmorrhagic retinitis, the case already alluded to. Of course one has seen many cases of hæmorrhagic retinitis which have been followed by stroke at varying periods. I have not been able to gather all my cases together, for these fifteen cases represent only workhouse patients and inmates. It would, however, be interesting to know what is the frequency with which hæmorrhagic retinitis can be found in cases of cerebral apoplexy, and their relationship to the blood-pressure.

Mr. M. S. MAYOU

exhibited a series of pathological slides showing the various changes in the retinal vessels together with the different forms of exudation into the various parts of the retina. He raised only one point in the discussion—that of

nomenclature. It was proposed to manufacture a new term: arterio-sclerotic retinitis. Pathologically, it was not a retinitis, but a degeneration, and if a new term was introduced, care should be taken that it was a correct one, e.g., vascular sclerotic degeneration of the retina.

Dr. G. NEWTON PITT (President of the Section of
Medicine)

said it was very essential in the discussion of this question to draw attention to what had been pointed out years ago; that the vascular changes which took place in the aorta, in the small vessels, and in the arterioles were quite independent, each of the others. Though one set of vessels might be diseased it did not necessarily follow that all vessels were affected. There was a tendency to assume that, in cerebral hæmorrhage, for example, there was an association with arterio-sclerosis. But when examining cases post mortem, one found that there were a large number of cases of cerebral hæmorrhage in which the arteries, including the middle cerebral, did not show extensive disease. It was true that where there was extensive disease of arterioles there was more likely to be disease in the middle-sized arteries as well than if the arterioles were healthy; but the diseases of the two were distinct, and the presence of one did not justify a presupposition of the existence of the other. That was a very important consideration, because the same applied in regard to the kidney changes in cases of cerebral hæmorrhage. The number of cases of cerebral hæmorrhage which had also marked interstitial nephritis was very moderate, or comparatively so, though statistics varied as to the exact proportion, continental figures being lower than our own. What he wished particularly to insist upon was, that arterio-sclerosis must not be definitely correlated with cerebral hæmorrhage: though they might co-exist in the same patient, they should be regarded as separate entities.

The same applied to the retina; its vessels corresponded closely in size to those from which cerebral hæmorrhage occurred and were the only ones open to inspection which gave an indication of the condition of the walls of the arterioles in the brain. The figures Dr. Ellis gave at the first meeting on this subject showed that, taking the early cases of retinitis in which there was no evidence of inefficient kidney function and no excess of blood urea and in which the kidney functions were fairly good, the majority of the patients died of cerebral hæmorrhage. The cases having excessive blood urea and evidence of very inefficient kidneys were more likely to die of uræmia. What was required to be known was, the relation between cases in which there was death from cerebral hæmorrhage and those in which death occurred with retinal changes. At present, very few figures on the subject were available, as in the wards the retinæ were not systematically examined.

Whilst there were cases in which one felt that the retinal change indicated renal change, there surely must be many cases in which, to a physician, it was doubtful whether one was to look upon the change as vascular, or as renal. What was the experience of ophthalmologists on this point? Did they claim that the two were quite distinct? There were some cases which were definite, but there were a large number of cases with retinitis in which one would not like to say what was the state of the blood urea or the condition of the kidney, without making further examination.

The occurrence of cerebral softening and cerebral hæmorrhage in these retinal cases raised this question: Were the lesions due to rupture of a minute

vessel, or to a thrombosis? When a vessel was thrombosed there was an infarct, and in many of the cases in which a small vessel was blocked there were present the conditions for a small extravasation of blood just as much as if the vessel had actually ruptured. When a small retinal hæmorrhage occurred, he was therefore not at all sure that this indicated the rupture of a vessel. In many cases such hæmorrhages were more indicative of thrombosis.

There was no doubt that the prognosis of the conditions was distinct. Where there were retinal changes with distinct evidence of kidney inefficiency, the prognosis was infinitely more grave than when retinal hæmorrhages occurred and the kidneys were efficient. Personally, he would be much more inclined to base his prognosis on the condition of the blood urea and the kidney efficiency than simply on the appearances in the retina.

A very small proportion of cases with fatal uræmia due to interstitial nephritis showed retinal changes, and in the most extreme forms of the lesion often no retinitis developed. What was the additional factor which determined that in a small proportion there should be retinal changes, and in the majority not? There were not sufficient data available for forming a definite conclusion as to this; but merely to assume that there was a toxic condition which had produced fatal nephritis did not sufficiently explain the fact that only in a small proportion of the cases would there be this retinal change.

With regard to the blood-pressure: he did not think it followed that if there were arterio-sclerotic changes in the middle-sized arteries, the pressure in the capillaries and the arterioles was necessarily raised. In many of the cases having arterio-sclerotic vessels it was a question whether the tissues were not suffering from too low rather than from too high a pressure. And there was much evidence favouring the view that these conditions were due to defective nutrition, and that it was a cutting off the blood supply which caused exudations, as well as small hæmorrhages.

Dr. A. FEILING

said the remarks he would make were based essentially on the study of thirty cases in the last eighteen months, and they had been observed from the point of view of the physician, not from that of minute changes in the fundus oculi. All those thirty cases were referred to him by his ophthalmic colleagues, and all sought advice in the first instance because of failure of vision, not for symptoms referred to any other system of the body. For this discussion, he tried to divide the cases into those which he would call arterio-sclerotic, and those he would designate renal. In the majority he found it fairly easy, on clinical grounds, to do so. The clinical symptoms he took for differentiation were: (1) A history of any definite attack of acute nephritis; (2) the persistent presence of large amounts of protein in the urine; (3) the presence of well-marked œdema. Cases presenting all those characteristics he classified provisionally as renal. And when the cases were followed into detail, they all corresponded to the renal group, to which Mr. Foster Moore drew attention.

Of the thirty cases, he classified only five as renal—three males, two females. The average age was 43·8 years, and all had well-marked bilateral retinitis. One had had nephritis during the war, i.e., in 1918, and was for nine months in hospital before he was considered well enough to be discharged. The second patient said he was in a London hospital under Dr. Pavy twenty years ago for acute nephritis. In the other three he was unable to get a definite history of acute renal disease, yet he did not think anyone would hesitate to class them as renal. Of the renal cases, the average systolic blood-pressure was 235 mm., and the diastolic pressure 135.

In the group he called arterio-sclerotic, there were several points of difference which were of great interest. Their average age was 63·3 years, which was in contrast with 43·8 years in the renal group, and fifteen of the twenty-five were females. Of the twenty-five, 60 per cent. had the retinitis unilaterally. The average systolic pressure was 214, the diastolic 118. In both groups there was high arterial blood-pressure, well-marked thickening of the accessible arteries, and, generally, some hypertrophy of the heart which was evident upon examination by ordinary clinical methods.

After discussing various hypotheses, illustrated from his own experience, Dr. Feiling concluded that there should be hesitation about adopting any new nomenclature in these conditions; the toxic idea was only at present based on theory, and to adopt the suggested new nomenclature implied the risk of shutting one's eyes to other causes, such as the mechanical one.

MR. J. HERBERT FISHER.

Referring to Dr. Batty Shaw's opening address, I was particularly interested in his advocacy of the substitution of such terms as "minimal and maximal toxic retinitis" for arterio-sclerotic and renal retinitis. In 1915 I read a paper before the Section of Ophthalmology on the retinitis of pregnancy, in which I advocated the use of the term "toxæmic retinitis of pregnancy" instead of albuminuric retinitis of pregnancy. After all, we are in the midst of a discussion, and some nomenclature has to be adopted unless our efforts are to come to an abrupt conclusion. The obstetrician has advanced reasons for believing that the pathological vomiting of pregnancy, eclampsia, acute yellow atrophy of liver and the necrotic changes of the kidney cortex that accompany the albuminuria of pregnancy are due to a toxin, and has suggested that the toxin may be produced by perverted katabolic processes in the syncytium cells shed from the chorionic villi at the placental site into the maternal circulation. In all the various organs liable to attack, the stress of the lesions is upon the blood-vessels, and hæmorrhages in consequence are a conspicuous feature; and it seems at least reasonable to infer that the fulminating lesions in the retina in these cases accompanied by exudates, hæmorrhages and œdema, are due to the same cause, and that in considering this variety of retinitis at any rate we are getting pretty close on the scent of the nature and source of the toxæmia.

At the other end of the scale it seems legitimate to take a glance at a variety of retinitis in which the natural changes of advancing years produce such alterations in the coats of the blood-vessels of the retina that exudates in the retina, based on hæmorrhage, result; I refer to retinitis circinata—a disease of the later years of life—so chronic in character that its explanation on the basis of the altered condition of the arterial tunics seems to fulfil every requirement without invoking any conception of an absorption toxæmia. Nearly twenty years ago I brought before the Ophthalmological Society² a boy, 13 years of age, who after acute rheumatism eight years earlier had developed cardiac disease, and showed multiple aneurysmal dilatations on some of his retinal arteries, clearly indicating very advanced changes in the arterial walls; in his retina he presented an appearance of glistening exudate entirely comparable to the senile cases of retinitis circinata. More recently, at a combined

¹ *Proceedings*, 1915, viii (Sect. Ophth.), pp. 127-148.

² *Trans. Ophth. Soc. Lond.*, 1903, xxiii, p. 73.

discussion on "Diabetic Retinitis," a striking fact was confirmed, viz.: that diabetic retinitis was rarely manifested in the most serious and fatal cases of diabetes which attacked patients in the first half of life, while it was frequent in the less serious cases of glycosuria occurring in the later half of life. It would appear that the toxæmia was most intense in the cases in which retinitis failed to manifest itself, and it was generally agreed that the milder toxæmia of the other group was capable of producing the retinal changes by reason of the sclerotic changes in the blood-vessel coats which had resulted from advancing years.

Most of the speakers in the present discussion have agreed that there is an arterio-sclerotic retinitis, as well as the more familiar retinitis, which, for the moment at any rate, we must still designate as an albuminuric or a renal retinitis, and for the most part they are agreed that it is not in many instances possible to distinguish absolutely, from the ophthalmoscopic appearances, between the one and the other. Dr. Batty Shaw inclines to the view that whether albuminuria be present or absent the cause is a toxæmia. It has been shown that in one group of cases in which the kidney functions well the prognosis for life is by no means so dismal as in the other. As an alternative to Dr. Batty Shaw's view, it has been suggested in the course of the discussion that in the unfavourable cases interstitial fibrosis of the kidney initiated by some acute nephritis is the primary disorder, and that the cardiac hypertrophy, with the resulting arterial thickenings, is a compensatory phenomenon, and assists the impaired renal tissue to function, though still indifferently; while in the cases which are arterio-sclerotic and attributable to some form of as yet unrecognized toxæmia, the cardiac hypertrophy must necessarily follow to drive the blood-stream against the increased resistance, but, that with this assistance, a reasonably sound kidney still functions well and serves an admirable purpose by eliminating the toxins from the blood. Dr. Batty Shaw appears to incline to the view that the toxin is likely to be the same but of different intensity in the two varieties of disorder, both of which are capable of producing retinal changes, and these changes to a large extent indistinguishable. Such a view seems to derive some support from the fact that though life may be much more prolonged in the cases in which there is no albuminuria, yet the causes of death, when it comes, are at any rate in many instances identical with those which produce death in a comparatively few months after retinitis develops in the presence of kidney insufficiency. If we accept the toxic explanation of both varieties, it appears reasonable to expect that in the arterio-sclerotic cases with efficient kidneys, the toxin should be found abundantly in the urine, but in low concentration in the blood; while in the albuminuric cases, where elimination of the toxin by the kidney is impeded, the toxin should be found in concentration in the blood, but sparsely in the urine. Might it be that comparative records of analysis of blood and urine in the two classes of cases by chemical pathologists will put us on the track of a toxin which at present is hypothetical?

It appears, therefore, to me that in the retinitis of pregnancy we have a manifestation which is typically toxæmic in origin; that retinitis circinata affords an example of a retinitis with hæmorrhages dependent on sclerosis of retinal vessels; that in diabetic retinitis we see the effects on the retina of specific chemical toxins capable of operating only in the presence of those changes in the walls of the blood-vessels which are common to all mankind as age increases. That in arterio-sclerotic retinitis and in so-called renal retinitis we have yet to discover a toxin, which quite probably may be common to both,

and which is in greater intensity in the blood and body tissues and likely therefore to prove destructive to life at an early date only if the kidney is impaired in its function as an organ of elimination.

Dr. C. F. HARFORD.

It seems to be clear from the introductory paper of Dr. Batty Shaw and from subsequent speakers that there is no such close relation between changes in the retina and disease in the kidneys as common tradition has sketched for us. This being so, it is our plain duty to review this subject from every point of view and in the meantime we should refrain from giving the grave prognosis which has previously been suggested, as we may by this very act be accelerating the fatal issue which we desire to avoid.

It would have been most instructive if Mr. Foster Moore could have given us a series of pictures of the fundus in cases which he describes as arterio-sclerosis and those which he refers to as renal retinitis. These would have afforded us an opportunity of considering anew whether the appearances in the two classes of cases could be regarded as distinctive apart from the clinical picture presented by the physician. He (Mr. Foster Moore) has told us that the signs which he describes as due to arterio-sclerosis cannot be looked upon as pathognomonic. Should we not be right to conclude from this that the appearances in question are common to many forms of disease which give rise to changes in the peripheral part of the vascular system, such as in the kidney and in the retina, each of which possesses a highly specialized arrangement of its terminal vessels? It will be noted that I have not referred here to cardio-vascular changes, for the questions of high blood-pressure and the like involve quite a different aspect of the case. Dr. Ellis, with his interesting series of cases, has afforded us valuable material for thought, as he has given us the benefit of the most recent methods of testing renal efficiency.

I will quote here a passage from the paper by Professor Hugh MacLean, delivered at the annual meeting of the British Medical Association, published in the *British Medical Journal* for December 2, 1922, relating to cardio-vascular changes and its effect on prognosis:

"There are many subjects who show but little evidence of marked cardio-vascular changes, but in whom the renal system is hopelessly inefficient. Conversely, it is not uncommon to find patients with very marked cardio-vascular changes in whom but little evidence of renal disease can be ascertained. These points must always be taken into consideration in estimating prognosis, for, in a general way, apart from such accidents as cerebral hæmorrhage, the outlook in a patient with high blood-pressure is not so bad if the kidneys are efficient. Indeed, such patients may enjoy comparatively good health for many years, even with a blood-pressure as high as 200 mm. mercury or even more. This observation explains the curious cases one occasionally finds quoted in the literature, in which a history of high blood-pressure of 250 or over, frequently associated with retinitis and other eye changes, was not incompatible with the enjoyment of fairly good health for several years."

The conclusion to be deduced from these researches may be best expressed in the words of my old teacher, Sir Michael Foster, when he had come to an end of his lecture on some fascinating physiological problem, told with convincing force: "The matter is not yet ripe for any dogmatic statement." In spite of this we still continue to use that most dangerous weapon of prophecy, which we designate prognosis, in order to foretell the year if not the

date of a man's death. Some of us have been in touch with primitive races among whom the emotions have more than ordinary influence, and we know that the mere expectation of death in many cases is sufficient to produce this result. This condition is not unknown in human life nearer home and it should prescribe to us supreme caution in the statements which we make in public or in the secrecy of our consulting rooms. This brings me to the chief personal contribution which I can make to the discussion on this subject, mainly from my recent studies in the psychological aspects of health and disease. A year ago I wrote a paper for the International Ophthalmological Congress at Washington, which was presented there in April last, on "Psychopathology in its Relation to Ophthalmic Practice," and in this I drew attention to the close connexion between diseases of the eye and those which were looked upon chiefly as psychical disorders. Since then I have been developing the hints which I then put forward in many directions.

I propose to make two suggestions bearing upon the present discussion in the hope that they may serve as a fruitful germ of thought to others. (1) The element of *cardio-vascular disturbance* is one of the essential factors in the fatal termination in cases such as those we are considering. Without taking special account of the precise effect upon the pulse or the blood-pressure of what we may call an emotional trauma, we are all of us aware of the disastrous results of a condition of panic or passion. The blanching of the skin of the face in the one case and the reddening in the other indicate the effect on the peripheral circulation of these emotions, which may be associated with violent physical manifestations. Thus in the case of panic there may be a state akin to temporary paralysis, or in passion violent muscular tremors. These may be said to be attributable to an element of fear, which it is the duty of the psychologist to investigate. It is contended that phobias of varying amount are an essential element in advanced cases of renal disease or other serious illnesses with a reputation for incurability, especially when the severity of the case appears to be indicated by ocular complications; and this needs to be borne in mind.

(2) With regard to the changes in the peripheral circulation, let me assume that Dr. Batty Shaw's hypothesis is correct, and that we are dealing with a toxic process affecting both the kidneys and the retina. The problem which is always before us is the explanation of the selective action of various toxins producing changes in the various vital organs. A great deal of evidence is available to prove the profound influence of suggestion or auto-suggestion upon local inflammation and vascular disturbances, especially of the skin. It is contended that this is due to the effect upon the peripheral circulation of psychical processes acting through the medium of the nervous and circulatory mechanism, and that suggestions of an unfavourable kind tend to produce morbid vascular changes. It is impossible in a contribution to a discussion to do more than suggest the lines upon which inquiry might be conducted, and it is earnestly hoped that the teaching of modern psychology may be considered in its relation to these obscure problems of pathology.

Dr. BATTY SHAW (in reply) (read by Dr. IZOD BENNETT).

This discussion has been fruitful in at least one direction, viz., that it has given an opportunity for a statement of claim to those who believe that there is something behind arterio-sclerosis which is not only responsible for the occurrence of arterio-sclerosis, but which is independently responsible for other

changes such as those met with in the retina when the blood-vessels of that tissue *are altered* and for similar changes in the retina when such vascular change *is slight or quite absent*; and we must hope that impetus may be given to those who would look for some agent responsible for both, in the blood circulating within the vessels.

Important as the result of discussion is, it is but a part of a much larger question, for all that has been said against the view that arterial disease is responsible for so-called arterio-sclerotic retinitis may be equally urged against the view that the contracted kidney is responsible for what is known as uræmia. It will have struck other members of this audience that with the exception of five cases out of the twenty-seven tabulated by Mr. Foster Moore, in which no blood-pressure readings were made, there was only one patient in whom the blood-pressure was not raised above 150 mm. of mercury, i.e., blood-pressure was raised in 97 per cent. of the cases. If we believe that the rise of blood-pressure may result from the occlusion of the lumen of arteries by endarteritis which has gradually developed and become universal, the occurrence of hyperpiesis is readily explained, but as I have pointed out, this obliterative disease of the arteries must be capable of running an unexplainably fugitive and recurrent course, or we should not see the rapid falls of blood-pressure or the equally rapid rises of blood-pressure I have figured. If we believe, as we must be asked to do, that hyperpiesis can result from obliterative disease of only a part of the arterial tree—for all of Mr. Foster Moore's cases of arterio-sclerotic retinitis were free from the suspicion that the renal arteries at any rate were so diseased as to produce renal disease—we must re-write the physiology of the vasomotor system. At present we know that if the arterial system of such a large tract as one leg is blocked by ligature at amputation, the blood-pressure of the rest of the arterial tree is not raised; adjustment is made in the vasomotor system and the blood-pressure of the rest of the body remains normal, as observation has shown. It seems to me that the large proportion of Mr. Foster Moore's cases show hyperpiesis because there is some pressor body, or bodies, circulating in the blood-stream.

Dr. Hawthorne objects to the use of qualifications such as "arterio-sclerotic," "renal," or "toxic," on the ground that the problems underlying this nomenclature have not yet been solved; but I think it is apparent to all of us that there is little advantage in speaking of retinitis occurring in arterio-sclerosis and retinitis occurring in renal disease, or in toxæmia, for we know that authorities would still believe that there were retinal changes met with in arterio-sclerosis entirely dependent upon the arterio-sclerosis—a view so ably presented by Mr. Foster Moore, and if we speak of retinitis present in renal disease we should perpetuate the belief that even in these cases the renal disease actually causes the mischief. If Dr. Hawthorne hesitates to accept my view that a pre-renal toxæmia is responsible for both the arterial change and for the retinal change, directly in both cases, on the ground that no one has yet demonstrated the presence of such agencies, I would suggest instead of the term "toxic retinitis," "hyperpiesic retinitis," for hyperpiesis was present in nearly all of Mr. Foster Moore's cases of so-called arterio-sclerotic retinitis; and I can state that in eleven cases which formed part of my recent study, retinitis was present and they were all cases of hyperpiesis. I have given reasons elsewhere why I think such great confusion has arisen in the symptomatology of renal disease, and have tried to show how all such difficulties could be resolved if, instead of expecting to find regularly a well-defined picture of kidney disease behind each well-defined clinical picture,

we gave up the position and rather looked to the blood as a source, not only of the clinical manifestations, but of the kidney change. This point of view is brought out by Dr. Goldschmidt in his clinical report (p. 23) of a form of disease which though not identical with that under discussion is closely allied to it.

MR. R. FOSTER MOORE (in reply).

It is clearly impossible, in the short time remaining, to touch upon more than a few of the points which have been raised by the various speakers in this debate.

I do not hesitate to agree with Dr. Batty Shaw that retinitis may be caused by toxæmia, as in the retinitis of pregnancy instanced by Mr. Fisher, and I would add parenchymatous nephritis in general; but, on the other hand, it cannot be denied that retinitis may occur from causes which are local in the retina, as for example, the star figure which may accompany papilloedema, so that it is at least not unreasonable to suppose that retinitis may be caused by the local vascular disease.

In renal retinitis there are, I believe, two factors at work, the one a toxic factor, which manifests itself by the "cotton-wool" patches, which, histologically, are found to be due to fibrinous exudate which may infiltrate all the retinal layers; and the other a vascular factor, which manifests itself by the presence of exudate having the ophthalmoscopic characters we have described in addition to evident changes in the vessels, and which is due to small areas of hyaline degeneration in the external molecular layer of the retina. The former is seen in parenchymatous nephritis, the latter in arterio-sclerotic retinitis, and I believe both these factors may be concerned in varying degree in renal retinitis.

I believe, then, that the changes in the retina in arterio-sclerosis to which the term "retinitis" may be applied are immediately due to the impaired circulation in the retina consequent upon the local disease of the vessels, but I should by no means be willing to dispute that this disease is in its turn dependent upon a toxæmia, whether of endogenous or exogenous origin. I have referred to this point in a previous paper.¹

With regard to the immediate lesion which is responsible for hæmorrhages in the retina, I do not suppose they are due to rupture of large vessels, but imagine they are due to diapedesis from the capillaries, owing to the impaired nutrition of the endothelium resulting from defective circulation, and perhaps to changes in the blood. I believe that in these cases the blood-pressure in vessels of the size of the central retinal artery is less than the normal, even though the pressure in the brachial artery is perhaps 250 mm. of mercury; I suspect, too, that the retinal hæmorrhages which are so frequent in the blood diseases come about in the same way, through impaired nutrition of the capillary walls.

I was interested in Mr. Bardsley's important observation that the "copper-wire" appearance of arteries may disappear; such an observation is outside my own experience.

With regard to Dr. Hawthorne's remarks, we know what a terminological purist he is, but he is less of a purist when he tells us that the differentiation between renal and arterio-sclerotic retinitis is no more than guess work!

¹ *Quart. Journ. Med.*, 1917, x, p. 41.

I was very glad to find Dr. Gaskell so emphatic in his belief in a primary disease of the vessels as quite distinct from renal disease. I do not hesitate to agree with Mr. Leighton Davies that arterio-sclerotic retinitis cannot in all cases be sharply differentiated from renal retinitis. I thought I had protected myself against Mr. Mayou's criticism by pointing out that I was using the term "retinitis" in the sense that it is used in renal and diabetic retinitis; it is not a good term, but it is hallowed by long usage, and would, I fear, be difficult to dislodge; I wish Mr. Mayou would supply us with a better term—it is badly needed.

With regard to Dr. Newton Pitt's remarks, I do not think the hæmorrhages are due to the lodgment of emboli; they are different in type from the hæmorrhages which are so frequent in the retina in infective endocarditis, and which I presume are indeed due to emboli.

I was glad to hear that Dr. Feiling found, in agreement with my own figures, that 60 per cent. of his cases of retinitis in arterio-sclerosis were unilateral; this alone is surely a fact which weighs heavily against the view that toxæmia is the direct cause of the retinitis; he mentions that fifteen of his twenty-five cases were in females; this preponderance of females has been commented upon by Nettleship, Gunn and Adams and the fact of its occurrence is borne out by my own cases.

Retinal detachments are not rare in severe renal retinitis if they are specifically looked for up to the time of death; I refer to extensive bilateral detachments, and not to a collection of exudate under the retina which is of still more frequent occurrence in histological specimens, but is not to be made out with certainty by the ophthalmoscope. These conspicuous detachments are often overlooked from the fact that the physician, having ascertained the existence of retinitis, does not always continue with periodic examinations up to the time of death; I have seen two of these cases within the last six months, and was able to collect thirteen of them in two years at St. Bartholomew's Hospital.

PROCEEDINGS
OF THE
ROYAL SOCIETY OF MEDICINE

EDITED BY
SIR JOHN Y. W. MACALISTER
UNDER THE DIRECTION OF
THE EDITORIAL COMMITTEE

VOLUME THE SIXTEENTH

SESSION 1922-23

SECTION OF ORTHOPÆDICS



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1923

Section of Orthopædics.

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SECTION OF ORTHOPÆDICS.

CONTENTS.

October 3, 1922.

T. H. OPENSHAW, C.B., C.M.G., M.S.	PAGE
President's Address : Traumatic Spondylitis...	1

November 7, 1922.

H. A. T. FAIRBANK, D.S.O., F.R.C.S.	
Ischæmic Paralysis ...	11
P. MAYNARD HEATH, F.R.C.S.	
(1) Traction Fracture of the Small Trochanter ...	12
(2) Subluxation of the Inner End of the Right Clavicle ...	12
W. T. GORDON PUGH, M.D., B.S.	
Fracture of the Small Trochanter ...	12
D. M. AITKEN, F.R.C.S.	
Osteo-chondritis of the Hip ...	18
W. ROWLEY BRISTOW, F.R.C.S.	
Transplantation of the Hamstrings... ...	18
R. C. ELMSLIE, O.B.E., F.R.C.S.	
Congenital Deformity of the Upper Limbs and Feet ...	18
PAUL BERNARD ROTH, F.R.C.S.	
Injury to Epiphysis of Left Acromion Process ...	14
E. LAMING EVANS, C.B.E., F.R.C.S.	
Tendon Transplantation for Talipes ...	14

December 5, 1922.

DISCUSSION ON OPERATIVE TREATMENT OF DISLOCATED HIPs, CONGENITAL AND PATHOLOGICAL.

Mr. H. A. T. FAIRBANK (p. 15), Mr. R. C. ELMSLIE (p. 24), Mr. W. H. TRETHOWAN (p. 24), Mr. D. M. AITKEN (p. 24), Mr. A. B. BANKART (p. 25), Mr. R. A. RAMSAY (p. 25), Mr. T. P. NOBLE (p. 25), Mr. H. A. T. FAIRBANK (reply) (p. 25).

ACTON DAVIS, F.R.C.S.	
Case of Multiple Exostoses ...	26

February 6, 1923.

R. C. ELMSLIE, O.B.E., F.R.C.S.	
(1) Case of Hæmophilic Arthritis of the Knee ...	27
(2) Case of Arthritis due to Dental Sepsis diagnosed and treated as Tuberculosis ...	28
PAUL BERNARD ROTH, F.R.C.S.	
Two Cases of Kohler's Disease ...	28
H. A. T. FAIRBANK, D.S.O., F.R.C.S.	
Unusual Form of Syndactyly ...	29

D. M. AITKEN, F.R.C.S.							PAGE
Traumatic Osteo-arthritis of Neck treated by Bone-graft	30
MAYNARD HEATH, M.S.							
Late Result of Beef-bone Graft of Humerus...	30
WALTER GRIPPER, M.B.							
A Case of Congenital Subluxation of Humeri	30
W. T. GORDON PUGH, M.B., B.S.							
Two Cases of Fractured Neck of Femur in Training-ship Boys	31

March 6, 1923.

DISCUSSION ON THE OPERATIVE TREATMENT OF SPASTIC PARALYSIS.

Mr. A. S. BLUNDELL BANKART (p. 33), Mr. T. H. OPENSHAW (President) (p. 37), Dr. GEORGE RIDDOCH (p. 37), Mr. E. MUIRHEAD LITTLE (p. 37), Mr. E. LAMING EVANS (p. 38), Mr. H. A. T. FAIRBANK (p. 38), Mr. ROCYN JONES (p. 39), Mr. GORDON PUGH (p. 39), Mr. W. ROWLEY BRISTOW (p. 40), Dr. A. FEILING (p. 40), Mr. R. C. ELMSLIE (p. 40), Mr. NAUGHTON DUNN (p. 41), Mr. P. JENNER VERRALL (p. 41), Mr. BANKART (in reply) (p. 41).

April 17, 1923.

C. MAX PAGE, D.S.O., F.R.C.S., M.S.Lond.							
Four Cases of Flexion Contracture of the Forearm treated by a Muscle-sliding Operation	43
PAUL BERNARD ROTH, F.R.C.S.							
(1) Case of Renal Dwarfism shown after Operation for Genu Valgum	45
(2) Case of Ocular Torticollis	46
B. WHITCHURCH HOWELL, F.R.C.S.							
Case of Snapping Hip	46
H. A. T. FAIRBANK, D.S.O., M.S.							
(1) Case of ? Charcot's Knee	47
(2) Case of Dislocation of Patella outwards, secondary to Osteomyelitis of Femur	47
G. PERKINS, M.Ch., F.R.C.S.							
Case of Pseudo-Coxalgia in an Adult	48

May 1, 1923.

R. C. ELMSLIE, O.B.E., F.R.C.S.							
Case of Intracapsular Fracture of the Neck of the Femur	49
PAUL BERNARD ROTH, F.R.C.S.							
Case of Osteitis Deformans	49
B. WHITCHURCH HOWELL, F.R.C.S.							
Case of Tendon Transplantation	50
W. H. OGILVIE, M.S.							
(1) Case of Renal Dwarfism	51
specimen of Synostosis of Phalangeal Joint ? Congenital in Origin	51

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Section of Orthopædics.

President—Mr. T. H. OPENSHAW, C.B., C.M.G., M.S.

Traumatic Spondylitis:

PRESIDENT'S ADDRESS.

By T. H. OPENSHAW, C.B., C.M.G., M.S.

ON assuming the Chair as President of the Orthopædic Section of the Royal Society of Medicine, my first duty is to express my sincere thanks for the honour conferred upon me by the Council.

My next duty is to congratulate the Orthopædic Section on having had such a distinguished, energetic and tactful President as Mr. Laming Evans, to whose energy and courtesy we owe the fact that this Section has been recognized as a full Section of the Royal Society of Medicine.

My next duty is to call the attention of the members of this Section to the fact that the British Orthopædic Association, under the distinguished Presidency of Sir Robert Jones, will hold its meeting in this building on the 20th and 21st of this month. I trust every member here will make a point of attending the meeting.

The status of an orthopædic surgeon has materially advanced of late; there was a time when it was as much as anybody's reputation was worth to be known as an orthopædic surgeon!

The scope of orthopædic surgery has been considerably enlarged during the war, but there are still many subjects connected with it upon which a discussion is advisable, and in which uniformity of treatment should, if possible, be carried out. It is not conducive to the welfare of the patient or to the dignity of the profession that two consultants should give diametrically opposite opinions as to whether a certain operation should be done or not.

It is, I think, advisable that, so far as possible, the treatment of any particular condition should be determined and generally accepted. Take, for instance, the question of hammer-toe. Some surgeons advise amputation, others excision of the interphalangeal joint, others excision of the head of the proximal phalanx, and others division of the flexor tendon and splinting. Those of us who know what *can* be done usually, I think, adopt the last method, but it would be well if there could be a universal consensus of opinion as to its advisability.

Again, take the question of amputation of the toes. If, for any reason, the great toe is so badly damaged that it has to be amputated, is it or is it not advisable to amputate all the other toes? Or, conversely, if the four lesser toes have to be amputated, is it or is it not advisable that the big toe should be amputated also? I am one of those who believe that the great toe should be amputated, but I may be in a minority. The question, I think, should be discussed, and some agreement reached if possible.

Again, many feet have been amputated because a bullet which passed through the tarsus has produced enormous exfoliation of new bone in the sole, pressing on the digital nerves, and thus rendering progression impossible. No apparatus or support enables the patient to walk with comfort. What should be done for these cases? I have several times turned a triangular

flap outwards to the outer border of the foot, removed the plantar fascia, and, along the external intermuscular septum, got down to the bony outgrowth, which I have successfully chiselled off, so saving the patient's foot and enabling him to walk with comfort.

This evening I am bringing before you the subject of "Traumatic Spondylitis, or Fracture-dislocation of the Spine." I do not propose to deal particularly with those cases of fracture-dislocation of the spine which have resulted in severe compression or laceration of the spinal cord. In these cases there is complete loss of motion and sensation below the injured spot, and the diagnosis is easy. There are many other cases, however, in which the spinal cord is not so compressed or lacerated as completely to destroy transmission of all nervous impulses. In these cases the injury to the vertebral column may have resulted only in compression and laceration or irritation of one or more nerves. When this condition occurs in the cervical region, the patient will usually complain of an impairment of power of mobility in the neck, a diminution of free range of movement in the neck, pain on certain movements, and sometimes a projection of one or other of the cervical vertebræ. When an X-ray photograph is taken of this condition, it may show a definite backward displacement of one vertebra only; or the whole of the neck with the particular vertebra may be displaced backwards, one or other of the articular or other processes may be broken, and irregularity of adjacent surfaces result.

I pass round the X-ray prints of a patient, K., who fell from his horse in the South African War, and was laid up for some months, but it was not until he hurt his back in 1916 that the fracture of the neck was diagnosed. The X-ray print shows definite increase in the intervertebral space between the fifth and sixth cervical vertebræ, with a compression, subluxation and irregularity of the fifth cervical vertebra. At the present time this patient has no symptoms which point to his cervical injury.

I also show the X-ray print in the case of a patient, N., in which is seen a dislocation of the fifth cervical vertebra backwards for a distance of three-eighths of an inch. The patient fell from his horse in India about nine months ago, and has since suffered from gradually increasing paralysis of the forearm muscles of the right arm, and of the shoulder muscles. The grip power in his right arm had become so feeble as to render him unfit for duty, and he returned to England. When seen by me in May, 1922, he had absolute loss of power of motion in the neck, which was held stiffly, marked wasting of the right arm, and, particularly, of the muscles of the forearm and shoulder. In consultation with Sir James Purves Stewart I wrenched the neck under anæsthesia, and as is shown by these two prints which I pass round, the displaced vertebra has been partially restored to its normal position. Some days after the manipulation he had some loss of power in the other arm, but this was transient, and he has also recovered from the original paralysis of the right arm. This is the only time that I have ever wrenched a neck vigorously with the idea of replacing the cervical vertebræ in position, and it is satisfactory to note that the operation has been attended by a complete cure of the patient. The patient's neck can now be moved in normal directions, though the range of movement is still somewhat restricted. The weakness of the right arm, however, has almost completely disappeared, and the patient's grip is equal on the two sides.

The usual form of treatment to be adopted in cases of fractured cervical spine, not immediately fatal, should be, I think, the wearing of an accurately-moulded leather collar, so as to prevent the spine from moving, and so stretching and irritating particular nerve trunks.

I show the X-ray photograph from the case of a patient, T., who was hit in the back of the neck in an aeroplane crash, and it shows a separation of the fifth and sixth cervical vertebræ. The patient complained of acute pain in the right breast at one spot, about 5 in. below the clavicle and 2 in. from the middle sternal line, when he bent his head to the left, and of the same acute pain in the left breast when he turned his head to the right, and of pain in both breasts when he bent his head forwards. These pains have completely disappeared as the result of wearing a leather collar.

Some two years ago a patient came to me at the London Hospital with the following history: She had just got out of bed, became giddy, and fell backwards, striking her head on the edge of an iron bedstead. She was laid up for a few days, and then noticed that the right arm was becoming weaker and increasingly painful. The pain persisted, and the loss of power progressed. An X-ray photograph was taken, and showed dislocation of the neck at the fifth cervical vertebra. With the idea of replacing the vertebra and securing it in position, I made an incision over the middle line of the neck, and wired the two laminae together. The wire was screwed up and left in, and power in the right arm steadily returned; after four months I removed the wire. I regret to say that the weakness of the arm is again returning, and I presume some further operative treatment will be necessary. To wire the laminae together in a severe case of displacement of the lower cervical vertebræ is, I believe, the best method of securing and maintaining them in position. In my opinion the wire should be left *in situ* permanently.

We have therefore at our disposal, so far as the neck is concerned, three methods of treatment: (a) By manipulation to wrench the vertebra back into its place; (b) to put up the head and neck in plaster of Paris for a time, and afterwards to prescribe a properly fitting leather collar; (c) to procure the proper apposition of the vertebræ by means of a wire passed round the laminae.

In the dorsal and lumbar regions the diagnosis of fracture-dislocation may present some difficulty. The symptoms may be pain, tenderness, rigidity, irregularity of the spinous processes and deformity, angular curvature, kyphosis, or lateral curvature, and hyperæsthesia of the intercostal nerves. The *pain* may be referred along the intercostal nerves (the so-called "girdle" pains), or along the lumbar nerves, or along the sciatic nerve, and may lead to a mistaken diagnosis of rheumatism, lumbago, or sciatica. It may be worse on movement, and often steadily increases in severity. There will generally be found some *tenderness* on pressure upon the spinous processes of one or two vertebræ. *Rigidity* is usually located at the injured parts, and may extend upwards or downwards over the space of two or more vertebræ. When the patient bends forward, the spinous processes do not separate in a normal manner. It must be remembered that the pain and rigidity do not necessarily commence at once, and that the condition of the back may be overlooked, owing to the presence of another injury. Some *irregularity* of one or more of the spinous processes is usually to be seen, or there may be a kyphotic or lateral curvature. *Hyperæsthesia* may mark out exactly the course of the intercostal nerve which corresponds to the seat of the fracture-dislocation, and even in slight cases of irritation the area of skin supplied by the dorsal branch of the corresponding branch of the intercostal nerve will be hyperæsthetic. This hyperæsthesia, if present, is a most valuable sign. It demonstrates the presence of neuritis in the intercostal nerve, and as the exact area of skin supplied by this nerve is beyond the knowledge of a malingerer, its precise demarcation is a proof of the genuineness of his symptoms.

In other cases, where the diagnosis is doubtful, or where an injury of the back is suspected, an X-ray photograph should be taken. Where there is a history of pain, rigidity, and a hyperæsthetic area, an X-ray photograph is imperative. X-ray plates must be *clear*, or they are worse than useless. Over and over again I have had patients with rigid backs and hyperæsthesia, and have been told that they had been X-rayed, and that the X-ray plate showed nothing. The photographs should be taken stereoscopically, and should be examined in the stereoscope. It is only in this way that slight changes can be seen, or the absence of any change be proved.



FIG. 1.—F.

The detection of malingering is essential.[†] Symptoms of pain and rigidity, and altered gait, are sometimes perfectly simulated, but it does not fall within the ability of a malingerer to mark out exactly the patch of skin which should be hyperæsthetic when the dorsal branch of an intercostal nerve is the subject of neuritis or irritation.

The X-ray plate may show the following deviations from the normal : (a) Compression of the body of the vertebra anteriorly or laterally ; (b) wedge formation of the vertebra ; (c) obliquity of surfaces ; (d) irregularity of adjacent surfaces of two or more vertebræ ; (e) gaping of front part of intervertebral space ; (f) lipping, usually of the anterior edges, of the upper or lower surface ;

(g) absorption of bodies centrally; (h) kyphosis, separation of ribs; (i) angular curvature; (j) ankylosis and synostosis; (k) lateral deviation; (l) rupture of supra- and interspinous ligament; and most of them are shown in the plates exhibited.

X-ray plates are exhibited from cases of the following patients, and prints from some are reproduced :—

F., thrown from his horse whilst steeplechasing in 1908; struck shoulders and back of head; was laid up for six months; gradually recovered. He went out to the Front

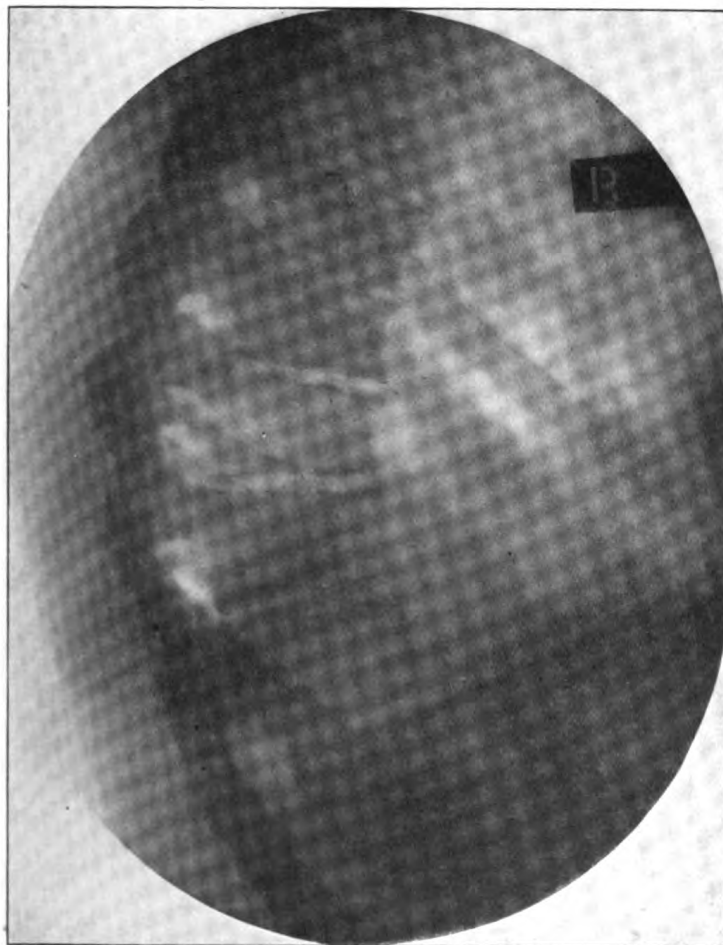


FIG. 2.—L. L.

in the war; fell into a trench in 1915 and wrenched his back; was laid up with continual pain and rigidity until seen by me in 1916; wore a corset for two years. Complete recovery. X-ray plate shows a wedge formation of the tenth dorsal vertebra, owing to compression of the anterior half of the body (fig. 1, p. 4).

K. was buried in 1916 by a falling dug-out. When first seen by me in 1918 he complained of gradually increasing unsteadiness of gait. Knee-jerks normal; weakness of legs and definite hyperæsthesia of dorsal branches; was trephined by another surgeon with some benefit, but still has weakness of legs and incoördination of leg muscles. X-ray plate shows lipping of the bodies of the eighth, ninth and tenth dorsal vertebræ,

with wedging of the ninth dorsal vertebra. This patient also has a subluxation of the cervical spine.

N., injured back while steeplechasing in 1912; was laid up for some months. In 1915 fell into trench and wrenched his back again. Was invalided home. Had continuous pain until put into corset by me in 1915: wore corset thirty months. Complete recovery. X-ray plate shows irregularity of the surfaces of the tenth dorsal vertebra.

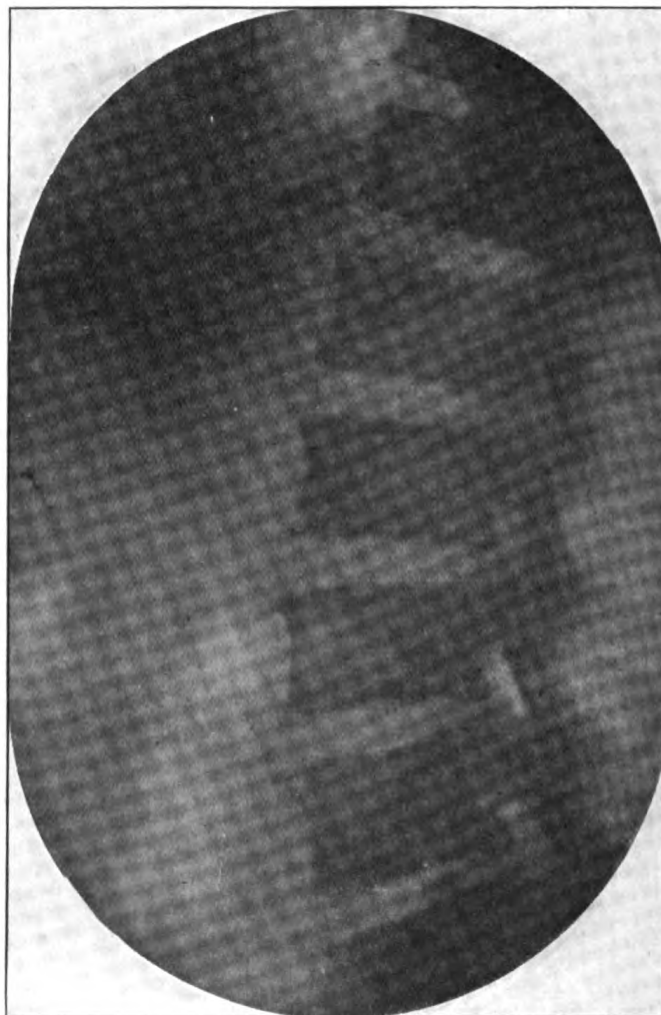


FIG. 3.—C.

L. L. was thrown from his horse in 1915; complained of pain and weakness in the back; there was irregularity of the dorsal spines; had continuous pain for one year, until put into leather corset. Complete recovery after wearing corset for eighteen months. X-ray plate shows wedging of the tenth, eleventh and twelfth dorsal and first lumbar vertebrae, and well marked kyphotic curve and irregularity of the surfaces of these vertebrae (fig. 2, p. 5).

G. gives a history that when shot in 1915 he fell backwards into a trench. Has a scar over the external occipital protuberance. No symptoms from the head injury or from the thigh at the present time. Has had pain in the back ever since he was shot. X-ray plate shows definite kinking to the left of the eleventh and twelfth dorsal

vertebræ, separation of the eleventh and twelfth ribs, lipping and synostosis of the eleventh and twelfth dorsal and first lumbar vertebræ, and compression of the anterior edge of the eleventh dorsal vertebra, with marked dorso-lumbar kyphotic curve.

C. was buried by a shell in 1917; has complained of pain in back ever since. X-ray plate, taken in 1920, shows definite lipping of the second, third and fourth lumbar vertebræ (fig. 3, p. 6).

T. T. crashed in an aeroplane in 1917; had concussion for twenty-four hours, and wrenched his back at the same time; complains of persistent headache and pain in the back ever since the date of the injury. X-ray plate shows absorption of the central part of the bodies of the tenth and eleventh dorsal vertebræ (fig. 4).

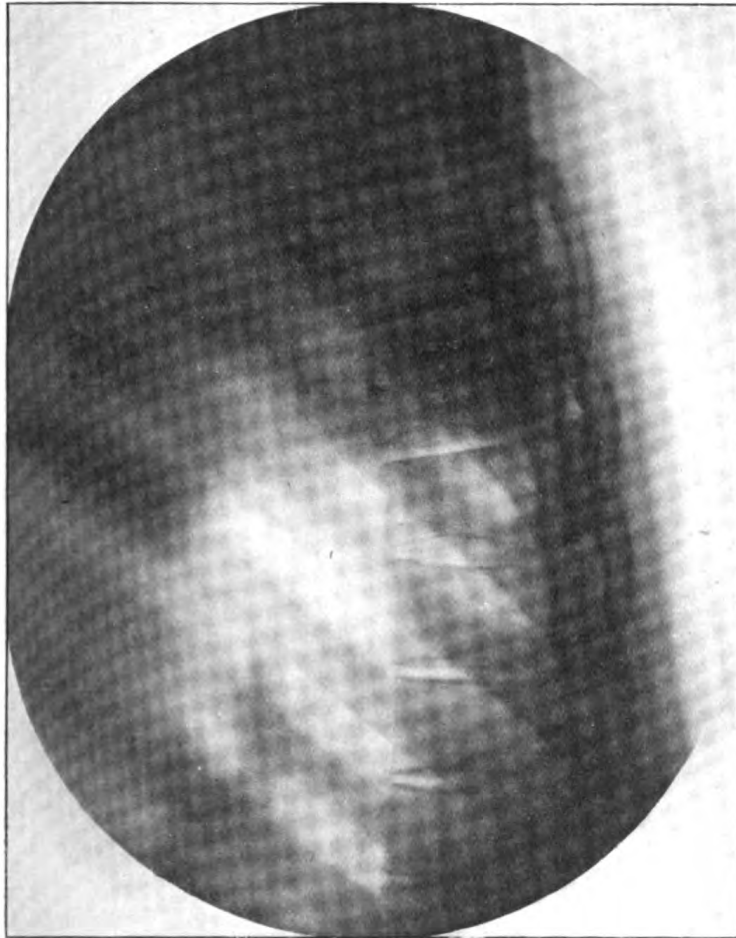


FIG. 4.—T. T.

In all these cases a spinal corset has been worn with marked benefit, and some are completely cured already.

The cases most difficult to diagnose are those of injury to the lumbar spine. In the neck there will be definite symptoms referred along one or more nerve trunks. In the dorsal region there will be some deformity. In the lumbar, and particularly in the lower lumbar region, however, there will rarely be deformity. The hyperæsthetic area may be confused, and the pain may easily be mistaken for sciatica, and the diagnosis of rheumatism may quite reasonably be made.

The following are cases of injury to the lower lumbar spine:—

C. was struck on the back by a hoist of coal in 1917. Had continuous pain in the back until 1921, when the diagnosis was first made, and he was ordered to wear a corset. Ever since the case was diagnosed and the corset supplied, he has been free from pain. X-ray plate shows opening of the front part of the intervertebral space between the third and fourth, and fourth and fifth lumbar vertebræ, with lipping of the adjacent edges of these vertebræ, also ankylosis of the fourth and fifth lumbar vertebræ on the right side.

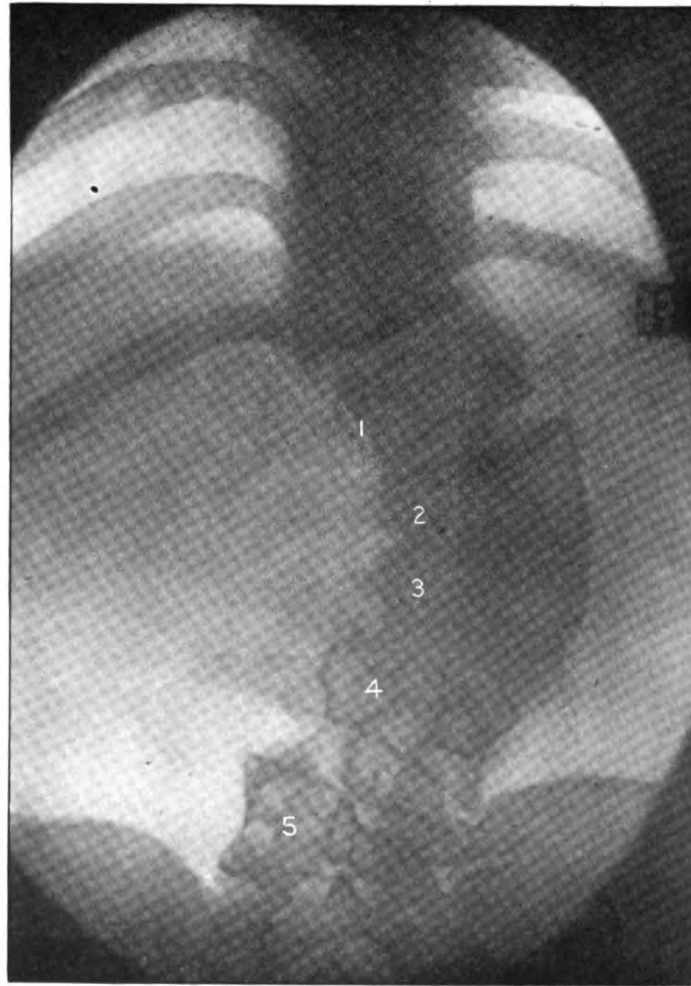


FIG. 5.—T.

T. was buried in 1916. Was first seen by me in 1920. X-ray plate showed a definite gap to the right between the first and second lumbar vertebræ; the second lumbar vertebra is a little compressed from above downwards on its left side, so as to make it wedge-shaped; the second and third lumbar vertebræ are smashed together, and both are compressed on their left edge; the upper edge of the fourth lumbar vertebra is very oblique; the lower edge of the fourth and the upper edge of the fifth lumbar vertebræ are normal (fig. 5). There is no deposit, and no erosion pointing to inflammation or to long-standing disease.

H. was buried in 1916. Was under medical treatment with pain in lower lumbar

and gluteal regions until 1921. X-ray plate shows synostosis of the fourth and fifth lumbar vertebræ (fig. 6).

B. was thrown from a mule in 1916; unconscious for twenty-four hours. X-ray plate taken in 1922, shows erosion and irregularity of the adjacent surfaces of the eleventh and twelfth dorsal vertebræ, synostosis of the fifth lumbar vertebra with the sacrum on each side, synostosis of the fourth and fifth lumbar vertebræ on each side—the right side being most involved—and marked obliquity of the fourth lumbar vertebra (fig. 7).

H. was buried in 1916. Has had pain in the back ever since. X-ray plate shows obliquity and compression of the fifth lumbar vertebra. Was first seen in 1921, and has worn a corset for one year with distinct benefit, but has not yet free mobility of the lumbar space.



FIG. 6.—H.

Even when an X-ray photograph seems to show something abnormal about the fifth lumbar vertebra, some surgeons are inclined to say that such abnormalities are congenital. The fifth lumbar vertebra is undoubtedly liable to variability, but obliquity of surfaces and synostosis are not congenital deviations from the normal. It has long been an axiom that effects of injury are manifested where a movable portion of the spine joins a fixed one. It is intelligible, therefore, that injury should take place, not only near the twelfth dorsal vertebra, but also near the fifth lumbar vertebra.

As regards treatment, the spine should be put at rest:—

(1) In the neck: (a) By a leather or felt collar; (b) by wiring (the wire should be left in).

(2) In the back: (a) By recumbency (for severe symptoms, e.g. paralysis); (b) by a leather corset with steels and arm supports; (c) by wiring (for increasing kyphosis).

(3) In the lumbar region: (a) By a leather corset; (b) by lacing the spinous processes together by silver wire.

As regards prognosis:—

(1) Suppuration is rare, unless there is some suppurating open wound elsewhere. In not one of the cases shown was there suppuration.

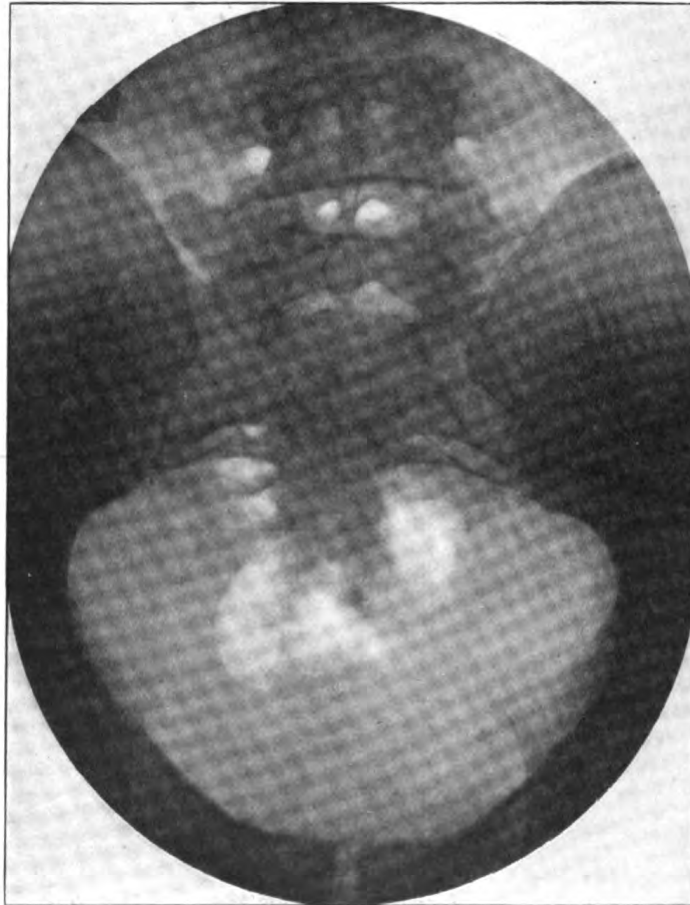


FIG. 7.—B.

(2) The pain diminishes quite quickly after the spine is immobilized.

(3) The hyperæsthesia quickly begins to diminish in extent of area.

(4) The cure is slow: eighteen to thirty months must be allowed.

My object in bringing these cases before you is first to emphasize the importance of examining for hyperæsthetic areas, and of judging by their position, and by the accuracy with which they are marked by the patient, whether the symptoms of pain and rigidity are genuine or not, and secondly to point out the advisability of giving due importance to the statement that the patient has been buried, and of putting the spine at rest in a corset as soon as possible.

Section of Orthopædics.

President—Mr. T. H. OPENSHAW, C.B., C.M.G., M.S.

Ischæmic Paralysis.

By H. A. T. FAIRBANK, F.R.C.S.

THE case is that of a boy, aged 9, who sustained a fracture of the lower end of the right humerus in December, 1921. No history of the treatment then received is available. He was admitted to the Hospital for Sick Children in January, 1922, with typical ischæmic contracture of the right forearm. There was flexion of the wrist and interphalangeal joints with some hyper-extension of the metacarpo-phalangeal joints, all of which were stiff with only a small range of painful movement. The long flexors of the hand were just acting but there was no power in the interossei. The general nutrition of the hand was very defective and the skin purple. Sensation to cotton wool was lost over the whole hand on both aspects and to pin-prick over the same area, except over the hypothenar eminence and the ulnar side of the dorsum. Radiograms showed an old supracondylar fracture in very bad position. Only 10° of movement, from 90° to 100° , were possible at the elbow-joint; there was a scar apparently from a deep pressure sore on the anterior aspect of the forearm. Treatment by gradual stretching and physio-therapy has produced considerable improvement; but there is still anæsthesia in part of the median nerve distribution, and the median intrinsics remain almost completely paralysed; while movements at all the joints are defective, and there is only slight power in the long flexors. Opinions are invited as to whether or not the median nerve should be explored.

DISCUSSION.

Mr. W. R. BRISTOW thought exploration of the median nerve should certainly be carried out, with the object of obtaining a return of the valuable median sensation in the hand. The physiological division of the nerve was probably in the region of its muscular branches to the pronator radii teres and long flexors, so that little could be done to secure a return of muscular power. Mr. Bristow also showed sections illustrating the differences in the changes which took place in muscle: (a) In a peripheral nerve lesion, and (b) in ischæmic contracture.

Mr. PAUL BERNARD ROTH suggested operating on the bony deformity first, since in his experience of these cases the deformity was often responsible for the paralysis—the brachial artery and median nerve being stretched over the bony projection. If this were done the nerve might recover.

Mr. FAIRBANK (in reply) said he preferred exploration of the nerve. For the bony deformity at this late stage he was content to perform a simple osteotomy.

Traction Fracture of the Small Trochanter.

By P. MAYNARD HEATH, F.R.C.S.

SKIAGRAMS are shown of a boy, aged 15, who slipped off his bed and landed on the floor on his buttocks. He immediately experienced great pain on the front and inner side of his right thigh and found he could not raise his right leg. There was no deformity, but active flexion of the hip-joint was impossible either when lying or standing. In walking the leg is swung forwards. Attempts to flex the hip cause pain shooting down the inner side of the thigh. There is tenderness behind the great trochanter and also in Scarpa's triangle on deep palpation. X-rays reveal a fracture of the small trochanter with very little separation. No retentive apparatus has been employed, and the boy has returned to school, where he takes things easily. He is now (five months after the accident) playing Association football, but there is still some wasting of the thigh and buttock. Ludloff's sign (inability to flex the thigh when sitting, with deficient flexion, only about 30°, when lying down) is still present. This is a rare condition, this case being the twenty-eighth recorded.

Subluxation of the Inner End of the Right Clavicle.

By P. MAYNARD HEATH, F.R.C.S.

THE patient, a man aged 30, gave no history of direct injury, but the condition was thought to have occurred during the unaccustomed exercise of bowling. The swelling has not altered since first observed last June. Examination shows a prominence of the inner end of the right clavicle, and X-rays reveals no enlargement of the bone. I ask for suggestions as to treatment.

Mr. A. H. TUBBY said he had had a similar case in a man aged 55, and he agreed that treatment presented a difficult problem. In his case he had eventually operated and scraped out a quantity of gelatinous material which proved on microscopic examination to be thyroid tissue. There was no evidence of a growth elsewhere. For Mr. Heath's case he suggested exploration if ordinary treatment by counter-irritation failed.

Fracture of the Small Trochanter.

By W. T. GORDON PUGH, M.D., B.S.

SKIAGRAMS are shown from a boy, aged 14, who had previously been under treatment for acute osteomyelitis of the lower ends of both femora and of the right tibia, from which he made a good recovery. He was discharged with good movement. In July, 1921, three months after discharge, he was readmitted to Queen Mary's Hospital with a history that while running and in the act of lifting the left thigh he heard a crack and felt a severe pain in the inner part of the left groin. There was no deformity or shortening, but all thigh movements were slightly limited and painful. X-rays revealed a fracture of the lesser trochanter, which united in the displaced position after treatment by flexing the knees over pillows. When last seen five months later there was full movement.

Osteo-chondritis of the Hip.

By D. M. AITKEN, F.R.C.S.

RADIOGRAMS are shown of three different stages of this disease in three members of the same family: T. M., aged 7, male, A. M., aged 13, and R. M., aged 20, females. In addition to the hip condition A. M. has a slight luxation forwards of the upper ends of both radii, and R. M. has developed considerable limitation of movement of both radii associated with luxation of the heads of the radii and scoliosis. X-ray plates of the last show arthritic changes of the head of the femur in addition to some mushrooming.

DISCUSSION.

Mr. FAIRBANK suggested that the cases were of arthritis rather than of true pseudo-coxalgia. The appearances in the X-rays suggested to him an ulcerative or absorptive arthritis.

Mr. LAMING EVANS inquired whether there were any signs of rickets in the three cases.

Mr. AITKEN (in reply) said that there was no evidence of rickets in any of the cases. While he agreed that in the eldest patient there were definite arthritic changes superimposed on the original osteochondritis, he adhered to his original diagnosis.

Transplantation of the Hamstrings.

By W. ROWLEY BRISTOW, F.R.C.S.

THE patient suffered from a gunshot wound of the thigh in 1918. He had a large scar on the front of the thigh; the quadriceps extensor femoris was not functioning, and he was unable to extend the knee. The biceps and some of the inner hamstrings were transplanted to the front, and attached to the patella at a hospital in the country. The patient states that he is worse as result of this operation, for although he can strongly extend the knee-joint he can no longer flex it more than a few degrees on walking. He is unable to flex the knee against gravity except when lying on his face, when 15° to 20° of flexion are possible—the transplanted muscles and the remaining hamstrings both acting strongly. The vastus internus, which has recovered, remains flaccid. It therefore appears that the transplanted hamstrings still act with the flexors. Dr. J. Sainsbury (in charge of the Physical Training Department at Shepherd's Bush) thinks further treatment by physical means, by which the patient has already been treated for nine months at another hospital, unlikely to be successful. While there is undoubtedly a mental factor in the case, I am inclined to think the only available treatment is to undo the transplant.

Mr. FAIRBANK, Mr. ELMSLIE, Mr. PUGH and others, agreed that the transplant should be undone.

Congenital Deformity of the Upper Limbs and Feet.

By R. C. ELMSLIE, O.B.E., F.R.C.S.

THE patient came under my care in 1922. He then had bilateral talipes equinus with much wasting of the legs. The hands were held fully pronated, with the elbows extended and wrists flexed. There was marked wasting of the

14 Roth: *Injury to Acromion* ; Evans: *Tendon Transplantation*

shoulders, arms, and forearms, but there was some power in the flexor and extensor muscles of the forearms and hands. I treated the left foot (which was more severely affected) by osteotomy of the fibula and of the neck of the astragalus, followed by wrenching and plaster. The hands were manipulated and extended by splinting. An arthroplasty was performed on the right elbow.

I still have the question of performing an arthrodesis of the shoulder under consideration, but up to the present I am concentrating my efforts upon attempting to get the patient's hands into such a position that he will be able to feed himself.

Injury to Epiphysis of Left Acromion Process.

By PAUL BERNARD ROTH, F.R.C.S.

THE patient, a girl, aged 16, complains of pain in the left shoulder, which has lasted intermittently for four months, and is relieved by resting the arm. On examination at hospital the tip of the acromion was found to be acutely tender, there was marked limitation of abduction, and all movements of the shoulder produced pain. X-ray examination showed partial fusion of the left acromial epiphysis with displacement upwards, whereas on the right side the epiphysis remained distinct from the diaphysis. The patient has improved with rest to the arm in a sling, but pain recurs on her using the arm contrary to advice.

Tendon Transplantation for Talipes.

By E. LAMING EVANS, C.B.E., F.R.C.S.

THE patient is a girl, aged 9, in whose case I have performed a tendon transplantation for paralytic talipes equino-varus. Treatment by manipulations and instruments having failed, I have transplanted the tibialis anticus tendon to the base of the fifth metatarsal, with a satisfactory functional result.

Section of Orthopædics.

President—Mr. T. H. OPENSHAW, C.B., C.M.G., M.S.

DISCUSSION ON OPERATIVE TREATMENT OF DIS- LOCATED HIPS, CONGENITAL AND PATHOLOGICAL.

Mr. H. A. T. FAIRBANK.

(I) CONGENITAL DISLOCATION.

OPEN operations performed on congenital dislocations of the hip-joint are of three types, each with its own particular purpose. The objects of these operations are respectively :—

- (1) To effect reduction.
- (2) To prevent relapse in a hip already reduced (i.e., making an upper lip to the acetabulum).
- (3) To relieve pain in an old unreduced dislocation.

(1) *Open Reduction.*

All surgeons have now discarded such operations as the Hoffa-Lorenz [1]—in which the acetabulum was deepened by a gouge, and if necessary the head of the femur was pared to fit it—and Lane's [2] operation, in which no attempt was made to get the femur into its proper position, but a new acetabulum was gouged out for it beneath the anterior inferior iliac spine. I have only once performed this latter operation. Ludloff [3] opened the joint on the inner side through an incision along the adductor magnus, reduction being effected by the aid of a hook pulling on the neck of the femur; this method of approach has obvious disadvantages. Burghard [4] in 1901 and again in 1903 published important papers on this subject, and gave sound directions for reduction by the open method without doing unnecessary damage to soft parts and without, of course, removing any cartilage or bone. He insisted on the necessity for dividing the psoas as part of the procedure. He also excised an elliptical portion of the capsule and closed the hole by sutures. Sherman [5] operates through an anterior incision and, later, in those cases with much anteversion, does a subtrochanteric osteotomy with rotation of the lower fragment, the upper fragment being held by a nail driven into the bone and retained there for six weeks. Bradford [6] has suggested the use of a shoe-horn as a means of directing the head of the femur down to the acetabulum; in some cases he has passed catgut through the neck of the femur and the capsule close to the acetabulum, in an attempt to hold the head in position. Galloway [7] is so disappointed with the results following manipulative treatment that he advocates wholesale open operation even in young children,

[December 5, 1922.]

using a posterior incision and a blunted carpenter's gouge in place of a shoe-horn. He also thinks there is no necessity for fixing an age limit for cases suitable for operation. I entirely disagree with him in making no attempt to select cases suitable for open operation. Manipulative reduction is undoubtedly the method of choice at the present time, and in my opinion will always remain so.

Indications.—The cases suitable for open reduction are in my opinion those of from 3 to 6 years of age which have resisted two attempts at manipulative reduction. In unilateral cases over 6 years of age greater care is necessary in selection, while no bilateral cases over 6 years should be operated upon. Manipulations and stretching of muscles sufficient to allow the leg to be put in the Lorenz position should invariably precede operation, which should be performed some three weeks after such manipulation, the leg being maintained in the Lorenz position during the interval.

Operation.—The child is tilted slightly to the opposite side by a sandbag under the sacrum, and the full abduction position reduced somewhat, to facilitate correct placing of the incision. The usual anterior incision is made between the tensor fasciæ femoris and sartorius, the upper end being continued along the anterior half of the iliac crest, as Smith-Petersen [8] advises. A broad sharp osteotome is a convenient instrument for cutting the muscles from the periosteum, as it is sharp enough for this purpose and damages the periosteum less than a knife. The front of the joint is cleared and the psoas partially or completely divided near the lesser trochanter by means of a hernia knife or probe-pointed bistoury. The joint is opened on the anterior aspect in a line with and rather low on the neck. The capsule bounding the lower margin of the isthmus is notched with a hernia knife, and the isthmus dilated by opening a pair of scissors or other double-bladed instrument. Reduction is then attempted by the usual manipulation, aided if necessary by a spoon-shaped lever, as little damage as possible being done to the cartilage of the bones forming the joint. The incision in the capsule is closed carefully. The thickness of the capsule varies enormously and is by no means always increased. If the child is fit enough to stand prolongation of the operation, an upper lip to the acetabulum is fashioned as described below. The muscles are sutured back into place and the wound closed in the usual way. The leg is fixed in plaster of Paris either in the Lorenz position, if fairly stable, or in a further degree of abduction, the so-called "axillary position." The plaster cast does not include the knee as a routine. The cast is cut off in a fortnight for the removal of stitches and a fresh cast applied.

The usual after-treatment consists in retaining the leg in at least right angle abduction for six months and applying further casts with slightly diminished abduction for another two to four months. Walking is permitted after the first six weeks unless extreme abduction has become necessary, in which case the leg is brought to a right angle after three months and retained in this position for another three months or more, walking being permitted during this period. The later treatment is precisely the same as that employed after manipulative reduction. In difficult cases the operation is a severe one, and precautions to avoid shock should always be taken.

Anatomical Points.—At operation certain anatomical points have been noted. The "isthmus" is usually definite and its dilatation a necessary part of the operation; in some cases, however, attempts at reduction have completely failed in spite of free opening of the isthmus; in these it seemed that every muscle passing in a vertical direction from pelvis to femur was

resisting descent of the latter bone, and that nothing short of free division of these muscles would enable reduction to be accomplished. Such free division as appeared to be necessary did not seem to be justifiable, as, after all, the stability of the joint, immediate and remote, must depend to a large extent on the condition of the muscles around. The ligamentum teres is present in some and absent in others, but is of no importance whatever. The gluteus minimus is closely applied to the portion of capsule surrounding the displaced femoral head and may be distinctly tendinous; it probably plays a part in preventing further migration of the femur. The reflected head of the rectus was in one case found arising from the capsule and had no direct attachment to the bone, its direction being parallel to the surface of the ilium. In another case a tendon was found crossing the inside of the joint in front of the ligamentum teres: this was apparently the obturator externus. In yet another a well-marked ridge-like fold of synovial membrane and capsule was seen passing up from the region of the lesser trochanter to the lower margin of the head, along the under surface of the neck.

Lastly, I would call attention to a condition seen in a bilateral case with considerable displacement of both femoral heads. This condition does not seem to have been described previously. It consisted of a more or less horizontal ridge lying just above and behind the acetabular margin, with which it formed a "V," open backwards. At first it was mistaken for the acetabular margin till it was found to be soft and the acetabular margin was seen at a slightly lower level. It is formed by a fold of synovial membrane and capsule which wrinkle as the head of the femur descends towards the acetabulum. The fold disappears when the head of the femur is forced upwards and the capsule stretched, as would be the case when the child is standing. As reduction is attempted this fold rises in front of the descending head, and is pushed by the latter over the upper part of the rounded acetabular margin; in this way it undoubtedly acts as an obstruction to reduction in these difficult cases with great shortening. Presumably this condition cannot be present unless the femoral head is displaced well beyond the acetabular margin, and the dilated capsule is applied to the dorsum ilii over a considerable area. The patient in the case in which this condition was first noted took the anæsthetic very badly and unfortunately died before completion of the operation. At the post-mortem examination one was able to verify the impression gained during the operation, and also to investigate a precisely similar condition which was present in the opposite hip. It is difficult to see how this tendency to heaping up of the synovial membrane in front of the head of the femur during attempted reduction can be avoided, as it only occurs, as we have already stated, in those with much shortening, that is, just those cases in which it would be impossible to get the head into the joint except over its upper and back margin.

(2) *Operation for making an Upper Lip to the Acetabulum.*

Jackson Clarke [9] published a method in which he freed the cotyloid ligament from the upper margin of the acetabulum from within the joint and then inserted sutures to pull the ligament outwards. Albee [10] turns down an osteo-periosteal flap at the upper acetabular margin, and holds it down with bone wedges cut from the tibia, each graft being fixed by a bone peg. He uses an external angular incision, turning the trochanter up with the attached muscles.

When, in 1912, I began operating on the cases with a view to making an

18 Fairbank: *Operative Treatment of Dislocated Hips*

upper lip to the acetabulum, a periosteal flap was turned down somewhat roughly so as to include minute fragments of bone, and this flap was held in place over the capsule by stitches, no separate bone-graft being used. Only one (fig. 1) of these early pre-war cases operated upon ten years ago can be traced, and this case is shown to-day. The result is excellent radiographically, and apparently functionally, but the function is difficult to estimate since the other hip, treated by manipulation only, has relapsed. This case was reported last year at a meeting of the British Orthopædic Association [11]. In recent cases a bone-graft cut from the ilium has been made use of, in addition.



FIG. 1.—Girl. Bilateral congenital dislocation. Aged 5½ at time of open operation on right hip; periosteal flap turned down above acetabulum. Bone-graft not used. Radiogram shows result nine and a half years later.

Indications.—(a) When relapse has followed manipulative reduction and the patient is still 6 years old or younger, i.e., young enough for fair prognosis. Most careful selection of cases over 6 years suitable for operation is necessary. (b) Patients of 3 to 6 years old (perhaps older) with marginal displacement only, should be operated upon forthwith, i.e., three weeks after manipulative reduction. In these cases the upper acetabular margin is flattened by the pressure of the femoral head, which lies in contact with it, and as a result the lip is more feebly developed than in those with a greater amount of

displacement of the head. Reduction by manipulation in such a case is easy, but the joint is found to be extremely unstable: relapse is certain to occur if nothing further is done. (c) This operation should be coupled with that of open reduction whenever possible. Manipulative reduction, when possible, is invariably performed three weeks previously and the Lorenz position maintained until the patient is on the operating table again.

Operation.—The joint is exposed as in the operation described above, but the capsule is not opened. A curved incision is made in the periosteum parallel to and half an inch above the upper margin of the acetabulum, curving down more behind than in front. The periosteal flap thus marked out is turned down together with a flake of bone. This flake is necessarily in fragments, but each of these is attached to the periosteum. It is difficult to place the osteotome in the correct plane for cutting an ideal bone-flap. The separation is carried down to and just beyond the acetabular margin. Care is taken to avoid breaking through the articular cartilage into the joint. Since the margin of the socket is ill defined and rounded, it is not easy to hinge the flap at exactly the right spot. The tendency in my hands has been in the direction of making the artificial lip too high. The reflected head of the rectus is left attached to the flap and turned down with it. Three thread stitches are inserted through the margin of the osteo-periosteal flap and the capsule, the former being held well out over the latter. A bone-graft, either crescentic or shaped like a small segment of a circle, is cut from the dorsum ilii a little below the crest: it includes the periosteum, outer compact layer of bone and some of the cancellous tissue beneath. If the whole thickness of the ilium is included at the lower part of the graft no harm results. This graft is laid on top of the osteo-periosteal bone-flap, medulla to medulla, its straight edge being well pushed into the angle of the bony cleft formed when the flap is turned down. The graft is easily bent to fit. The graft is held in contact with the flap by a small bone peg also cut from the ilium, or by a stitch or two, or both. The muscles are stitched back into place and the wound closed. Plaster is applied with the leg in rather more than 90° abduction. The wound is dressed in a fortnight, a fresh cast applied, and the treatment carried on as outlined above.

It should be noted that the joint is not opened unless open reduction forms a part of the operation, and also that the periosteal flap and bone-graft are not intended to forcibly retain the head of the femur in place at once, as in the operation Mr. Trethowan performs. Reduction is maintained by the position of the limb, the flap and bone-graft simply acting as the foundation for the growth of an exaggerated upper lip to the acetabulum.

The reason why this operation should not, in my opinion, be regarded as the routine procedure in the older cases is that the additional risk of such an operation is hardly justified when it is remembered that absorptive and other changes are frequent sequelæ of reduction by all methods in older children. Of the two operations the second would seem to be more frequently justifiable in children over 6 years than the first or open reduction.

Note.—Although 3 to 6 years are mentioned in the indications for both the above operations, I have only once operated by the open method in patients under 4 years of age. Occasionally some special difficulty is met with, usually in a bilateral case, which may call for open operation even at this early age.

Results of these Operations.

Of eleven hips in which open reduction was attempted only three remain reduced. Two showed good X-ray and functional results two and a half and four and a half years after reduction: they were then lost sight of. The third developed arthritis and became ankylosed, but later became slightly mobile again. A fourth developed anterior displacement of the head of the femur as a late result, and then passed into the hands of another surgeon, who, I believe, eventually got a fair result. Another resulted in "anterior reposition" with good function. Not much to offer in the way of results!

The acetabular lip-forming operation has been performed on eleven hips. One, without a separate bone-graft being used, has been shown to-day with a satisfactory result. Three others are satisfactory so far and promise well.

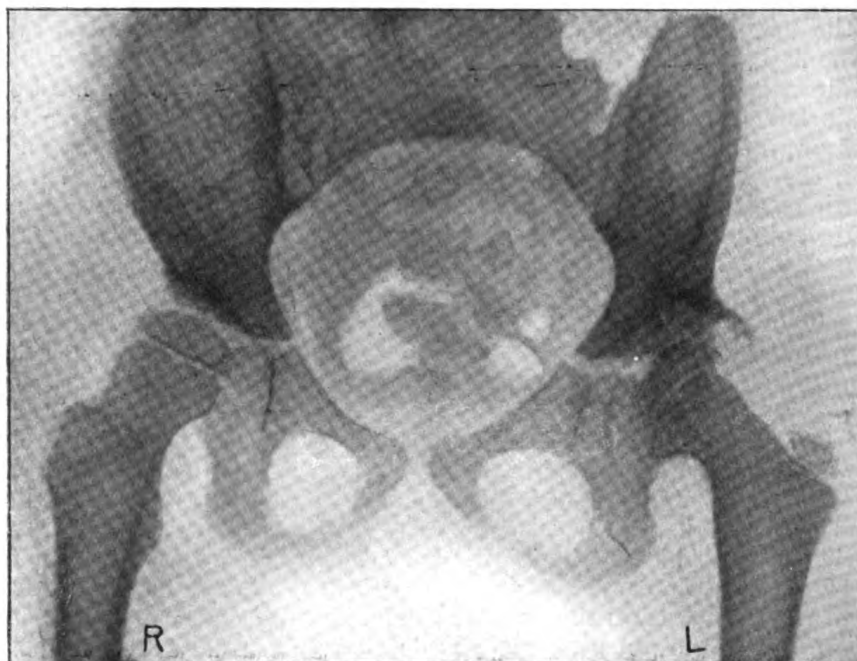


FIG. 2.—Girl aged 4½. Congenital dislocation of left hip. Radiogram taken seventeen months after operation for making upper lip to acetabulum, bone-graft being used. Note well formed upper lip of left acetabulum. The right hip now looks somewhat unstable.

(Two of these have been shown at the meeting, figs. 2, 3, 4.) Four have resulted in "anterior repositions," one showing marked signs of "absorptive arthritis." One relapsed, while the result in the others is unknown. Again, the results are not brilliant, but they are in some respects encouraging. To judge from the late radiographic appearances in some, nothing might have been done to fashion an upper lip, yet in a few the upper lip is redundant. In spite of failures, I think the lines to work upon are those which aim at improving the placing and fixation of the flap and graft, at the same time avoiding traumatism of the articular structures to the greatest possible extent.

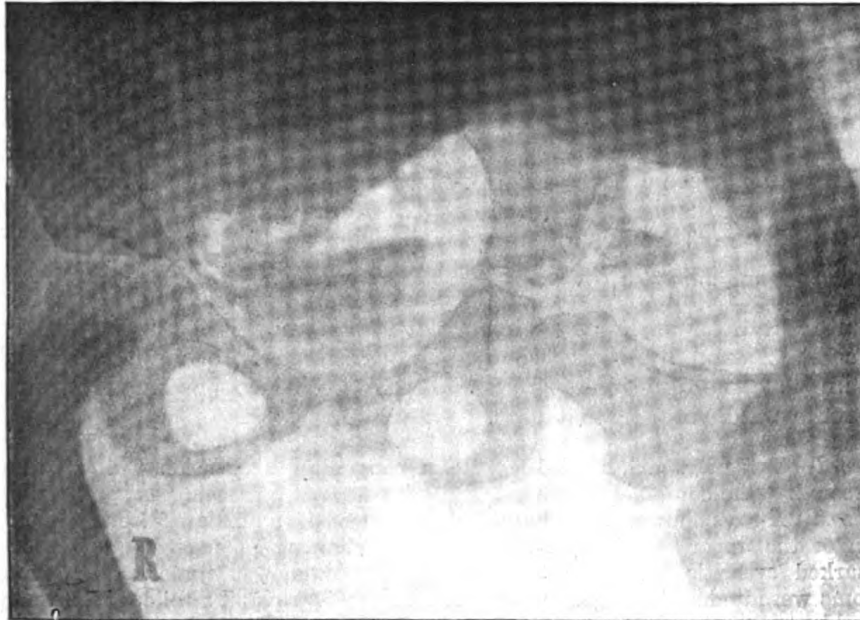


FIG. 3.—Girl aged 5½. Congenital dislocation left hip. Radiogram, with hip in plaster one week after operation for making upper lip; bone-graft from ilium fixed over osteo-periosteal flap.



FIG. 4.—Same as fig. 3. Nine months after operation, showing head of femur in place and good upper lip to the acetabulum.

(3) *Operation for the Relief of Pain.*

The pain from which the untreated or imperfectly cured case sooner or later suffers is, in my opinion, undoubtedly due to arthritis, and not to stretching of the capsule. There is no evidence of increase of deformity with the onset of pain. We cannot discuss to-day all the operations which might be performed in these cases. I think the pain must be severe before an operation becomes justifiable. I am inclined to think that simple excision of the head of the femur through an anterior incision is the best method to adopt. Retention of the leg in abduction for two or three months is followed by the use of a calliper splint for at least a year. It is possible that detaching the trochanter and fixing it at a lower level, as suggested independently by Whitman [12] and by Platt [13] when operating for other hip conditions, might improve the functional result. I have here two specimens of femoral heads removed during the last year.

Case I—that of a girl, aged 13, with a congenital dislocation which had never been treated. She complained of pain and stiffness and increasing limp for the past two years. X-rays showed a fairly well-shaped head in a shallow false acetabulum at the site of the upper acetabular margin, but no changes to suggest the presence of well-marked arthritis. During the months the child was under observation the symptoms were undoubtedly getting worse; she could not walk a mile. After tenotomy of the adductors and abduction of the leg one month previously, the head of the femur was excised through an anterior incision. Capsule very thick, one-third of an inch at least; synovial membrane thick, and in a condition of marked villous arthritis; head of femur slightly flattened over a curved area on the inner and upper aspects; cartilage was pitted in places; ligamentum teres absent; false acetabulum remarkably smooth and apparently covered by good articular cartilage; capsular isthmus well marked; an instrument passed through it made out the true acetabulum to be small and apparently full of villous growth.

Case II.—Patient, a girl, aged 17, had her unilateral congenital dislocation reduced by me at the age of 2½. Four and a half years later the function was perfect, and the joint considered satisfactory. Four years ago, that is ten years after reduction, she began to complain of pain after an attack of appendicitis; the mother states that a diagnosis of tubercle was made and a splint ordered. When seen eighteen months ago the hip was almost fixed in an adducted position. X-rays showed the head of the femur resting opposite the upper lip of the acetabulum, i.e., in a position of "anterior reposition," with marked alteration in the outline of the bone. The pain had then disappeared, but re-appeared later and gradually got worse. At operation the capsule was not more than one-eighth of an inch thick, and was closely applied to an irregularly shaped femoral head; synovial membrane almost everywhere unrecognizable; ligamentum teres very thick and strong; practically no cartilage left on head of femur, which shows deep groove across the top in an antero-posterior direction, and considerable new bone formation around the neck. False acetabulum small compared with head of femur, but well covered with fibro-cartilage; entrance to old acetabulum could not be found.

Both the above patients are now walking in callipers, and are free from pain.

Dr. Reich, of Cleveland, and other surgeons who have recently visited Vienna, have spoken to me with enthusiasm of an operation devised by Lorenz, modified by his assistant, Hass, and performed by both of them, for such cases as those just mentioned. As it may be new to those present, as it was to me, a brief reference may be of interest. The operation consists in an oblique sub-trochanteric osteotomy, the line of section passing upwards and forwards rather than upwards and inwards. The wound is closed, the thigh abducted, and by

manual pressure the upper end of the shaft, and with it the bone immediately above the section, is pushed in towards the acetabulum. After three months in a plaster cast at 45° abduction the patients walk without apparatus of any kind. The results are said to be surprisingly good. For an ununited fracture of the neck of the femur, the head of course being in the joint, the procedure is slightly modified, the section of the bone being then made obliquely upwards and inwards.

(II) PARALYTIC DISLOCATIONS.

Careful selection of cases suitable for operation is necessary, as in many the extent and severity of the paralysis contra-indicate any attempt being made to stabilize the hip. In the last three years I have had four cases, and only considered one suitable for operation. Reduction is, of course, easy; moderate abduction is sufficient to prevent the femur from slipping up.

The operation performed was much the same as that for making an upper lip to the acetabulum in congenital dislocations, but wedges of bones were inserted to hold the flap and graft down, as in Albee's operation. In this case the mistake was made of making the lip too high, as can be seen in the skiagram. After such an operation the leg is fixed in plaster of Paris in moderate abduction and slight internal rotation.

(III) DISLOCATION FOLLOWING ARTHRITIS.

No attempt has been made to effect reduction by operative measures in old-standing cases. In early cases reduction can easily be accomplished by simple abduction under an anæsthetic, and no operation is necessary. In an old-standing case a modified arthroplasty was attempted, with poor result. As a rule one has been content with obtaining abduction by tenotomy of the adductors and gradual correction of deformity or by an osteotomy. In one or two I have excised a portion of the obturator nerve with the idea of diminishing the tendency to adduction. I regret that I have not been able to bring before you any brilliant and new operative methods, but I hope these few remarks may serve their purpose, namely, to act as a foundation for discussion.

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Mr. R. C. ELMSLIE

said he had had little personal experience of the open operation for congenital dislocation and in cases which had come under his observation the results were disappointing. In two children he had formed an upper lip to the acetabulum by bone-graft, but in both the dislocation recurred. He congratulated Mr. Fairbank on the excellent result which had been obtained in the patients shown to illustrate this operation. The effect of reduction might be highly satisfactory for some years and yet the ultimate result might be unsatisfactory. He had recently seen a woman whose age was in the late twenties and whose dislocation was reduced by Kocher in 1902; the hip had remained stable for twenty years but she was now developing arthritis. He remarked on the fact that Mr. Fairbank had omitted to mention arthrodesis, which he thought might be tried more often where there was marked deformity or pain.

Mr. W. H. TRETHOWAN

agreed with Mr. Elmslie that the results of open reduction were a little disappointing. He did not usually sew up the capsule, which might account for some relapses. It was hazardous in these cases to maintain the limb in the Lorenz position for long owing to the tendency to contracture, and in avoiding this the limb had sometimes been brought down prematurely. As to the grafted-lip operation there were two difficulties to combat, viz., the anteversion of the head and neck and the bad acetabulum; relapse was as much due to the one as to the other. It was difficult to get the grafting done if the limb was already in the Lorenz position. For this reason he opened the joint freely, levered the head into position with the spoon and used a stout graft, taken from the front of the iliac crest and fixed with ivory pegs, to hold it in this position. In a boy, 10 years old, he had reduced the dislocation completely, inverted the limb for six months and afterwards corrected the position by osteotomy. He regarded fixation of the hip-joint as a serious disability and for this reason would not advocate arthrodesis. He regarded Mr. Fairbank's results as stimulating.

Mr. D. M. AITKEN

had done excision in dislocation following paralysis and old sepsis. He did not think that in children from 3 to 4 years of age it ought ever to be impossible to reduce by manipulation. Many adults got about well without pain; others had pain from lordosis. He had seen a patient of Sir Robert Jones in whom trans-trochanteric osteotomy of both femora had been done and the limbs arranged to correct the lordosis, with excellent result. He would like to know the mortality of the open operations.

Mr. A. B. BANKART

said that if the femur was fully reduced and kept in position until there was evidence of stability there was no likelihood of relapse. Some cases were quite irreducible. In older patients the joint got stiff after reduction, first from spasm and then from arthritis. Reduction by the open method was more likely to be followed by stiffness. In the cases in which he had done the open operation he had used a posterior incision, and treated the joint as if no open operation had been performed. The Lorenz position was not a normal attitude for the hip and he thought the limb should be turned in as early as possible. With regard to one of Mr. Fairbank's cases he suggested that the femur was not being kept down by the graft but by new bone which would have been thrown out under any circumstances.

Mr. R. A. RAMSAY

also commented on the Lorenz operation and position, and showed two children in whom reduction of the dislocation by manipulation had been followed by pseudo-coxalgic changes in the femoral head and marked stiffness. A weight-extension applied for six months had resulted in free movement, but the changes in the head remained.

Mr. T. P. NOBLE

showed a patient too old for reduction in whom he had done a sub-trochanteric osteotomy, and placed the limbs in wide abduction and hyperextension. He had done excision for pain in the back.

Mr. H. A. T. FAIRBANK (in reply)

said that the results of open reduction were not good ; he had done ten before the war but only one since. Mr. Trethowan's method of lip-grafting would probably have been useful in cases in which he had abandoned open operation owing to the joint being very unstable. He thought it probable that difficulty in bringing down the limb after operation was due to arthritic changes, and not to matting of the surrounding structures. With regard to mortality, one child had died, but death was probably largely due to the anæsthetic. He had considered arthrodesis, but unless the patient had violent pain he would leave her alone ; he would prefer excision to arthrodesis. It was highly important that the profession should realize that these patients should come under treatment at the earliest possible age, not when 5 or 6 years old, as was so often the case. Personally he had not always been able to reduce double dislocations in children at the age of 4, once even he had been unable to do this at the age of 3. If the Lorenz operation failed and relapse occurred he would do the lip-grafting operation. He did not agree with Mr. Bankart that new bone was always thrown out after reduction, forming a roof to the acetabulum ; he had seen some cases in which this did not occur. Pseudo-coxalgic changes did not interfere with cure, but the mushroom head remained.

Case of Multiple Exostoses.

By ACTON DAVIS, F.R.C.S.

PATIENT, a boy, aged 11, with upper two-thirds of left humerus involved by tumour, 26 in. in girth and 7 in. in length; noticed at age of 3; girth increasing by 1 in. yearly. Other swellings affected both ends of femora, lower end of right radius, both scapulæ, both ends of tibiæ, and lower end of left fibula. Two brothers, two uncles, one aunt and grandfather have exostoses.

The parents would not allow amputation, and he was considering removal of upper end of the humerus with insertion of graft later.

Mr. R. C. ELMSLIE said he would be loth to amputate, and would make incisions, leaving a wide strip of skin and lifting the soft parts. The main difficulties would be the musculo-spiral nerve and hæmorrhage near the shoulder-joint.

Section of Orthopædics.

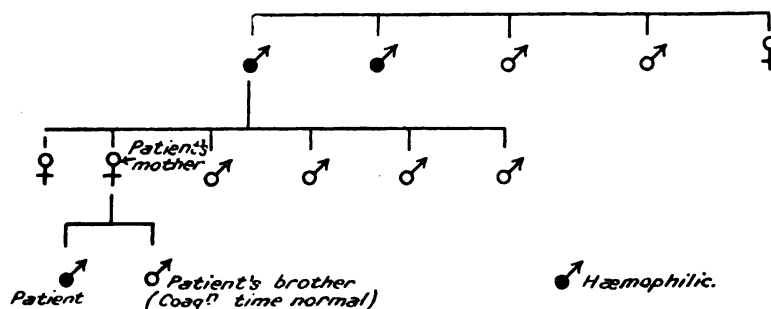
President—Mr. T. H. OPENSHAW, C.B., C.M.G., M.S.

Case of Hæmophilic Arthritis of the Knee.

By R. C. ELMSLIE, F.R.C.S.

PATIENT, a boy, aged 9, one of a family of bleeders, this fact being shown in the accompanying diagram. Symptoms of hæmophilia commenced at the age of 2, with bleeding from the gums, the right ankle-joint became affected at the age of 5 and the left knee-joint at the age of 6. The patient has several times been into St. Bartholomew's for treatment.

At the age of 5, injections of horse serum and transfusion of the mother's blood was tried. At the age of 6, these were repeated on two occasions



without altering the coagulation time of the blood, which was 4.6 times the normal. He was then treated by intradermal injections of horse serum, and the coagulation time fell to normal. Following these treatments he was free from attacks of bleeding for nearly two years.

An acute effusion into the knee-joint occurred one week ago, and the boy is at the present time being rested on account of this.

The treatment of this patient by intradermal injections was described by H. W. C. Vines, in the *Quarterly Journal of Medicine*, April, 1920, p. 259, "Anaphylaxis in Treatment of Hæmophilia."

Case of Arthritis due to Dental Sepsis diagnosed and treated as Tuberculous.

By R. C. ELMSLIE, F.R.C.S.

PATIENT, aged 20.

November, 1920: Pain and swelling of the right knee: temperature raised in the evenings. Went to Whitstable, where she was treated for tubercle with plaster and splint.

July, 1922: Returned to work, with knee on plaster splint.

October, 1922: Recurrence of pain. Went to St. Bartholomew's Hospital. Knee then stiff and swollen; forced extension painful; flexion movement absent. Teeth X-rayed; four extracted; streptococcus isolated from the roots and vaccine given. No local treatment since October.

Knee has improved very greatly, and is now very little swollen; movement free from full extension to right angle.

Two Cases of Kohler's Disease.

By PAUL BERNARD ROTH, F.R.C.S.

Case I.—A boy, aged 5, first seen on January 18, 1923, was said to have fallen and hurt his left foot at Christmas; he had since walked with a limp, and his foot was noticed to be swollen. Family history revealed nothing

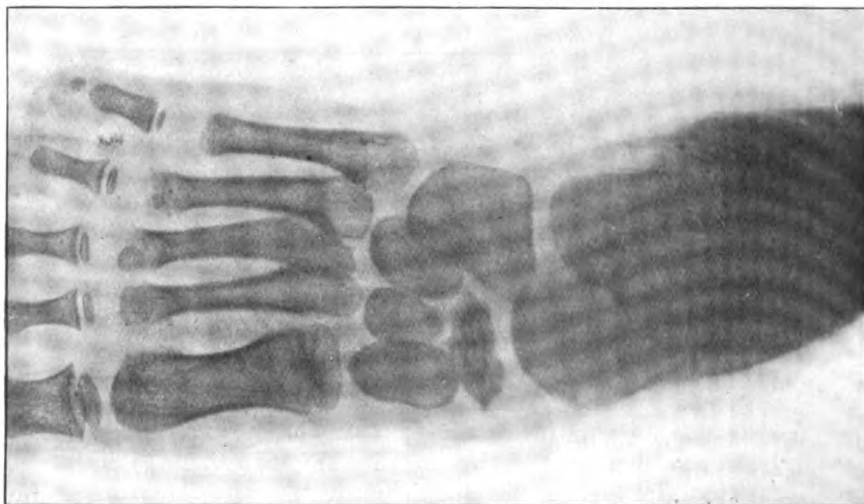


FIG. 1.—Radiogram of Case I, antero-posterior view.

abnormal. Examination was negative, except that there was considerable tenderness to pressure over the scaphoid bone. Radiograms of both feet by Dr. Bracken showed that whereas the right scaphoid bone was normal, the left (*see* figs. 1, 2) was distinctly diminished in size and extremely dense.

Case II.—A boy, aged 7, first seen on November 3, 1922, had been sent by Dr. Cathcart Irwin as a case of left Kohler's disease, with the history that the trouble had begun last June, had abated, had recurred again in August, had again abated, and had then relapsed, for the second time, recently. Family history: Seven of mother's near relatives tuberculous; patient's brother, aged 13, had a tuberculous eye, and his sister had died of tuberculous peritonitis when $4\frac{1}{2}$ months old. On examination: Tenderness to deep pressure over the scaphoid of left foot. Radiograms showed that *both* feet were affected, the scaphoid bones being small and dense, the appearance suggesting that they were only partly formed.



FIG. 2.—Radiogram of Case I, lateral view. •

Unusual Form of Syndactyly.

By H. A. T. FAIRBANK, D.S.O., F.R.C.S.

R. M., BOY, aged 16 months, a twin; the other, a boy, being normal. No history of congenital deformity in family.

Right hand: First, second and third fingers fused at terminal segments, the proximal portions being separate. The terminal segments show contraction rings so that the whole deformity looks as though it had resulted from a string tied round near the tips of the three fingers. Left hand: Normal. Right foot: Congenital contraction rings of second and third toes. Left foot: Congenital

talipes equino-varus now well on the way to cure. Partial webbing of second and third toes. X-ray: Right hand shows terminal phalanges of united fingers unfused.

Traumatic Osteo-arthritis of Neck treated by Bone-graft.

By D. M. AITKEN, F.R.C.S.

PATIENT, J. E. G., was blown up in a mine in France on July 26, 1916. A month later he fell and again hurt his neck. In 1917 he was discharged from the Army as suffering from spinal caries. In December, 1921, he was admitted to the hospital at Shepherd's Bush obviously in pain and supporting his chin on his hands like a case of acute cervical caries. Skiagram showed crush fracture between fifth and sixth cervical vertebræ and osteitic changes in several vertebræ. Kept at rest with plaster collars for nine months, but the slightest movement caused pain. Bone-graft was inserted from second cervical to first dorsal spine in October, 1922. Patient has been free from pain since the operation.

Late Result of Beef-bone Graft of Humerus.

By MAYNARD HEATH, M.S.

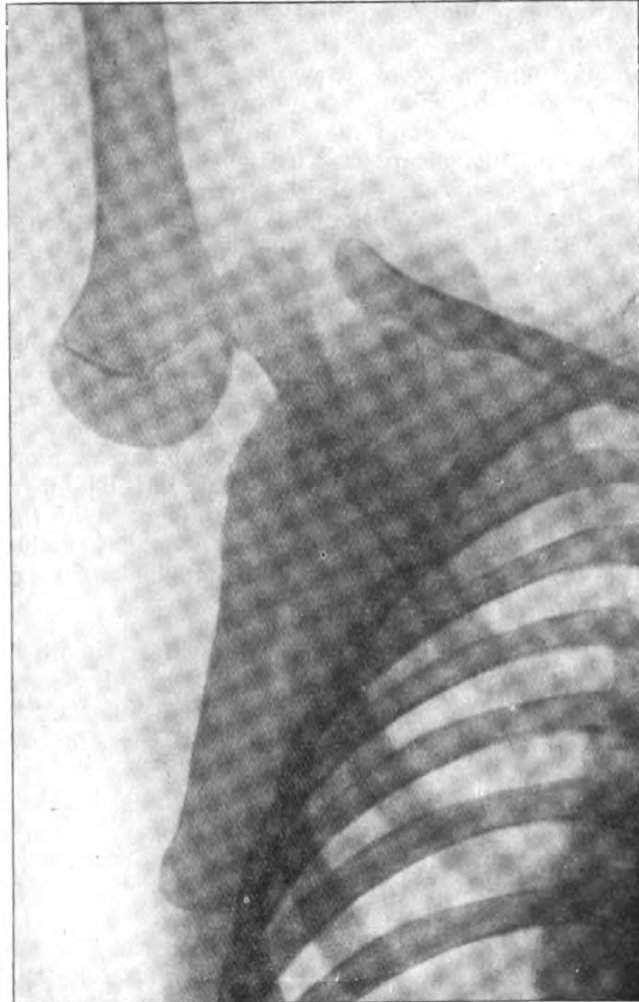
THIS boy was shown at the meeting of this Section on November 1, 1921.¹ The upper end of the humerus had been resected for fibrocystic disease and replaced by a beef-bone graft inserted subperiosteally. The function of the arm is very good though abduction is slightly limited owing to angulation of the shaft of the humerus. X-rays show that the graft has been completely absorbed and replaced by bone which closely restores the original contour of the humerus.

A Case of Congenital Subluxation of Humeri.

By WALTER GRIPPER, M.B.

THIS girl, well grown and aged 9, is an inmate of an orphanage. The condition first came under observation owing to the nurse, when applying a vaccination dressing, noticing that if either arm was raised higher than the shoulder a distinct jerk in the joint could be felt as the head of the bone slipped in and out of place. The child feels no pain and there is apparently no disability at present. There is no history of injury or strain. The skiagrams of both shoulders show a similar appearance. The glenoid cavity is apparently defective, its surface forming almost a straight line with the axillary border of the scapula, and with the arm raised there is a considerable interval between the glenoid fossa and the head of the humerus.

¹ *Proceedings*, 1922, xv (Sect. Surg., Sub-sect. Orth.), p. 7.



Case of congenital subluxation of humeri.

Two Cases of Fractured Neck of Femur in Training-ship Boys.

By W. T. GORDON PUGH, M.D., B.S.

Case I.—F. F., aged 15, fractured the neck of his left femur on March 15, 1922, by falling on the hip a distance of 4 or 5 ft. over banisters. On the next day he was able to walk, but with some pain. Subsequently he remained in bed until admission to hospital three weeks later. On admission he lay with the left limb abducted and the great trochanter prominent; there was shortening of half an inch. Flexion movement was limited to about 90° ; abduction, internal rotation and extension were limited; external rotation was increased. Movements were rather painful, but could be carried out without much spasm.

There was local tenderness over femoral neck. X-rays showed a fracture of the neck of femur at its middle, with slight upward displacement of the lower fragment producing some degree of coxa vara. The position was improved by abduction under an anæsthetic, and when examined on October 5 the shortening was less than half an inch and movements in all directions were normal. The walking Thomas splint was omitted in December.

Case II.—A. F., aged 15, also sustained a fracture of the neck of the left femur. On June 28, 1922, he was jumping over a rope at drill and slipped in alighting, with the result that his thighs were widely opened and he fell astride on the deck. There was not much pain, but he could not stand on the left leg. He was admitted to hospital on the same day. The limb was in a position of extreme external rotation (90°); the trochanter was $1\frac{1}{2}$ in. higher than on the uninjured side. A skiagram showed a fracture at the junction of the neck with the great trochanter, the neck lying at right angles to the line of the shaft.

The position was corrected under an anæsthetic; he was treated on an abduction frame and not permitted to use the limb for six months. About a month after getting up he complained of slight pain in the joint, and a skiagram showed some absorption of the upper part of the head of the bone. The shortening is about one-third of an inch and movement at the joint is free. On account of the recent pain he has now been supplied with a calliper.

Section of Orthopædics.

President—Mr. T. H. OPENSHAW, C.B., C.M.G., M.S.

DISCUSSION ON THE OPERATIVE TREATMENT OF SPASTIC PARALYSIS.

Mr. A. S. BLUNDELL BANKART.

SPASTIC paralysis is due in the great majority of cases to a permanent defect of the upper motor neurons. It is, therefore, essentially an incurable condition, and the most that can be expected from surgical treatment is relief from some of its most obvious mechanical or functional disabilities.

The cases with which we have to deal are chiefly, though not exclusively, the cerebral palsies of children—diplegias, hemiplegias, paraplegias, and less often monoplegias. These children, in consequence of their spastic condition, suffer from certain contractures of the limbs, and it is to the relief of these contractures that surgical treatment is almost entirely directed.

The typical contractures of the lower extremity are plantar-flexion of the foot (spastic equinus), flexion of the knee, and adduction of the thigh; while in the upper extremity pronation of the forearm and flexion of the wrist are most characteristic.

We cannot, however, afford to ignore other features of cerebral paralysis, for it is upon these far more than upon the contractures that the ultimate prognosis depends.

In a large proportion of the cases there is some degree of mental deficiency. It need hardly be said that we do not operate on idiots. But mental impairment of less degree is no contra-indication to treatment, since the state of the child may be materially improved thereby.

Spastic paralysis, as its name indicates, is not merely spasticity, but also paralysis. In other words, there is loss of motor power distinct from that which results from the rigidity of the limbs. The amount of motor weakness varies in different cases, and when it is pronounced it is a serious factor in the prognosis.

Involuntary movements—athetosis, variable spasm, and associated movements, when well marked, are a contra-indication to the ordinary methods of treating spastic contracture. Their treatment constitutes a separate problem which has not yet emerged from the experimental stage. Lastly, progressive disease is a definite contra-indication to surgical treatment.

Physiologically, spastic contracture is a tonic reflex contraction of the muscles—a spinal reflex released from the normal inhibitory control of the higher centres of the central nervous system. It disappears during deep anæsthesia and when the reflex arc is interrupted in any part of its course.

It is remarkable how long this physiological contracture may persist without any permanent change in the structure of the muscle. It may persist in this state for years. But eventually an anatomical or structural shortening is added to the physiological contracture, and the deformity becomes fixed. When structural shortening exists, it must be treated by direct attack upon the shortened muscle, by stretching it, or by dividing or lengthening its tendon. But when we are dealing with pure physiological contracture, we have a choice of methods. We may attack it either on the afferent or on the efferent side of the reflex arc.

Non-operative treatment is useless. Most of these cases have been treated for months or even years by massage and manipulations and sometimes electricity before the patients come to us, and I have never yet seen any permanent benefit result from such treatment.

The attack on the afferent side of the reflex is known as Foerster's operation, and I will deal with that next, although it does not come next in chronological order. In 1908 Foerster suggested that as spasticity was a spinal reflex, its intensity might be diminished by cutting off a sufficient number of the afferent impulses entering the spinal cord at the appropriate level. He, therefore, advised, and many surgeons have since practised, division of certain of the posterior nerve roots in cases of spastic paralysis.

This procedure is no doubt physiologically correct, but it is in advance of our knowledge of localization in the posterior roots. For, whether you regard spasticity as being equally distributed in a limb, and the contractures as due to the preponderating action of the more powerful muscles, or whether you consider that some muscles are more spastic than others, in either case the contracture disabilities are local, and you cannot localize the effects of posterior root section to particular groups of muscles. So that, in spite of the most elaborate directions worked out by Foerster and others for particular cases, the results of this operation have been generally very disappointing, and I believe I am right in saying that it has been abandoned or at any rate reserved for exceptional cases by nearly every orthopædic surgeon.

So we are left with the attack on the efferent side of the reflex, and I would include on the efferent side both the nerve and the muscle. For I want to emphasize the fact that whether you attack the nerve or the muscle, the principle of the operation is the same—namely, to diminish the effective action of the preponderating muscle or muscle group.

One can diminish the effective action of a muscle by destroying it, or part of it, or by lengthening its tendon, so that its effective range of action is diminished. The transplantation of a muscle to a place where it is not wanted seems to me to be a needlessly elaborate and clumsy way of accomplishing the same thing. The simplest way of destroying muscle is to cut out its nerve supply. The simplest way of lengthening a tendon is to cut it across and allow the resulting scar to organize in continuity with the tendon, though in some cases a plastic operation is preferable.

¶ Now, for many years orthopædic surgeons have claimed that it is possible to set almost any spastic child upon his feet, and to get him walking somehow, provided only that he has sufficient intelligence to try to use his legs. This result has been attained usually by a long course of treatment, the details of

which are, of course, well known to you. Briefly, in a typical case of cerebral diplegia, the adductors are divided, and the thighs widely separated, the hamstrings are divided, and the knees kept straight on splints, and the tendo Achillis is lengthened by a plastic operation.

Originally, the adductors were divided by simple tenotomy. But it was found that the contracture returned so rapidly that many surgeons made an open incision and excised portions of these muscles; and for the same reason portions of the hamstrings have been excised. Nevertheless, it was found that the tendency to recurrence of the contractures was so great that it was necessary to keep the limbs for a very considerable time in some form of apparatus with the thighs fully abducted, and the knees extended.

But even then the patient's troubles were not over, for in order to prevent relapse many surgeons found it necessary to put these patients into walking instruments for a long time after the correction of their deformities. How long such instruments must be worn before all danger of relapse is over, I do not know. But I have seen patients wearing them for years, and I have seen also cases of relapse in patients who had been treated without instruments, or who have discarded them too soon. Lastly, it must be admitted that the gait of a spastic patient wearing double instruments to the pelvis, and with the knee-joints locked in extension, is a very poor apology for normal progression.

Now, instead of dividing or lengthening tendons, one can diminish the effective action of muscles by lessening the number of fibres that take part in their contraction. This, after all, is the most natural way of regulating the strength of muscular contraction. For the strength of contraction in a given muscle is not due to any variable property of the contracting fibres, but is determined solely by the number of fibres brought into action by the stimulus. So that, by destroying a sufficient number of muscle-fibres, we can diminish the strength of contraction of a muscle or muscle-group to any extent we please.

In spastic contracture we have a powerful group of muscles in a state of tonic contraction so intense that it holds the part in a position of deformity, and prevents movement in any other direction. By destroying a sufficient part of this muscle-group, we abolish the excessive contraction, and get rid of the contracture deformity, while leaving sufficient muscular power for all practical purposes. The simplest way of destroying muscle fibres is to cut out their nerve supply, and this is the principle of Stoffel's operation for spastic contracture.

I will not give details of these operations in different regions. It is enough to say that for adductor contracture of the thigh one resects one or both divisions of the obturator nerve, for flexion contracture of the knee one resects the nerve tracts to the long head of the biceps, the semimembranosus, and part or the whole of the semitendinosus; while for the equinus contracture of the foot one resects one-half or two-thirds, more or less, of the nerve tracts to the gastrocnemius and soleus. In each case the relief of the contracture is immediate and permanent, yet sufficient muscular power is retained to adduct the thigh, flex the knee, or raise the heel from the ground when required. Similarly, the pronation and flexion contracture of the upper extremity may be relieved by destruction of a sufficient part of the nerve tracts to the pronators and flexors.

To Stoffel we owe the development of the "cable theory" of nerves. He demonstrated that in the main nerve trunks of the limbs the various nerve tracts maintain their identity and independence at least for a considerable

distance, and that the cross-section anatomy of a large nerve is fairly constant in any given situation. So that, given a knowledge of the position of these tracts, we can pick out and isolate from the main nerve trunk the nerve supply of any required muscle-group, and we can then destroy as much or as little of it as we please.

The ease with which any main nerve trunk can be exposed in a limb, and the fine adjustment and variation that are possible in dealing with the nerve tracts, impart to this operation both a simplicity and an adaptability that are, I think, unattainable by any other method of operating.

As examples of variations from what one has come to regard as the typical operations, I would instance cases in which there is severe flexor spasm of the toes, making walking difficult and painful, others in which there is peroneal spasm causing pronounced eversion of the foot, and yet others in which there is persistent internal rotation of the thigh. In each case the contracture is disposed of by resecting a sufficient part of the appropriate nerve tract—the flexor tract to the toes on the deep surface of the internal popliteal nerve, the peroneal tract in the middle of the external popliteal, and the tracts to the internal rotators of the thigh in the superior gluteal nerve.

I need hardly remind you of the consequences of destroying the nerve supply to a muscle. You know that the muscle immediately becomes completely and permanently paralysed. Such a muscle is pale, flabby, and atrophic. It has no tendency to shorten. On the contrary, it is very easily stretched, and, when stretched, it remains elongated and relaxed.

And this is the outstanding feature of Stoffel's operation as compared with operations directly upon muscles or tendons—that the effect of the nerve destruction is immediate and permanent; there is no tendency to recurrence of contracture in the paralysed muscles; there is, therefore, no need to keep these patients in abduction frames or other apparatus; they can be got up immediately after the operation; and they require no walking instruments afterwards. In a word, Stoffel's operation abolishes the prolonged mechanical treatment and after-treatment of spastic paralysis, and in particular the use of all forms of splints and apparatus.

With regard to the end-results, it must be remembered that spasticity is but one of the disabilities of cerebral paralysis. Even after the removal of their contractures, patients in the severer cases never walk quite naturally, and they are apt to be awkward and ungainly. Yet, to take a child who cannot even stand upright, and to put him on his feet and get him walking somehow, is at least a result worth having, while in less severe cases there is often a very striking improvement in stability and locomotion.

Fortunately, with the exception of idiots, every child instinctively tries to use his legs, so that the only after-treatment required is practice and encouragement, which in most cases can quite well be given at home.

In the case of the upper extremity the conditions are different. A patient with one sound arm and one disabled one will use the sound arm for everything that he can do with it, and there is hardly anything in the ordinary daily routine of life that cannot be done quite well with one arm. So that in hemiplegics, at least, there is no natural incentive to use the affected arm after operation, and we depend much more upon the patient's intelligence and co-operation in order to obtain a useful result. Still, some of these patients do show a very considerable improvement as regards function, and the cosmetic result alone is sometimes worth having, for the characteristic pronation-flexion contracture is an unsightly deformity, and the appearance of the limb is often much improved by the relief of these contractures.

In conclusion, I would submit :—

(1) That Stoffel's operation is by far the best method of treatment which has yet been devised for the relief of spastic contracture.

(2) The actual operation in each region is quite a minor operation and easy of performance.

(3) It admits of a degree of precision and adjustment to the needs of individual cases, such as cannot be obtained in any other way.

(4) Its results are immediate and permanent.

(5) The very nature of the resulting intentional paralysis makes recurrence of the contracture impossible.

(6) Prolonged after-treatment and the use of splints or other apparatus are entirely unnecessary.

The PRESIDENT (Mr. T. H. OPENSHAW)

remarked that he had always operated on spastic paraplegic cases by division of tendons and muscles. He considered lengthening of the tendo Achillis by subcutaneous tenotomy invariably successful, but the danger lay in over-correction. It was necessary to remove a large portion of the hamstrings at the knees in order to avoid rapid reunion and relapse: simple division by the open method was not sufficient. A large portion of the adductor longus should be removed, and the adductor brevis and gracilis freely divided by open operation. In severe cases in which the flexion of the knee and hip had been allowed to persist for many years, lengthening of the adductors and hamstrings would not be a sufficient enough measure to enable the patient to walk upright. In such cases it would be found that the psoas muscle was contracted and it must be divided. He had repeatedly divided this muscle at its insertion to the small trochanter through a posterior vertical incision at the lower border of the gluteus maximus muscle. He had performed Foerster's operation in two cases, but the operation was exceedingly difficult and severe, and the result was uncertain.

Dr. GEORGE RIDDOCH

said he had been impressed by the results of Mr. Bankart's operations. He would have liked to hear more of the methods of preventing contractures in spinal cord lesions, and pointed out that in such lesions there was a period of flaccid paralysis before the spasticity came on; during the former period treatment by splintage, &c., should be instituted to prevent the occurrence of contractures during the latter period. When the reflex arc was being restored slight stimuli frequently resulted in spasm and must therefore be avoided.

Mr. E. MUIRHEAD LITTLE

said he regretted that he had had no personal experience of Stoffel's operation. It had been his practice to treat spastic palsy on the lines outlined by the President, with satisfactory results when the patient's intelligence was adequate. Foerster laid down as necessary conditions for his operation the existence of severe spasm and good intelligence. Of course this combination was very rare, but he had had one such case in which the operation cured the spasm, but the patient seemed to have no power of balance and when last seen could not walk alone. It should be remembered that in spastic paralysis the whole muscular system was generally involved, including even involuntary muscular

tissue. This was shown by the absence of any bleeding of importance during and after extensive myectomies and the almost constant complication of constipation. Stoffel's method, if not an ideal one, represented in his opinion a great advance.

Mr. E. LAMING EVANS

referred to four cases of posterior root section which he had performed upon cases of spastic paraplegia and had reported to the International Congress of Medicine in 1913. The ultimate results were not encouraging, and rhizotomy could not be classified as a routine operation for the relief of spasmodic muscular contracture. He pointed out that the treatment of spasmodic muscular contracture by exsection of the obturator nerve was long antecedent to Stoffel, and that the division of branches of the other motor nerves after they had left their parent trunk was but an extension of the principle of that treatment, and was not Stoffel's method, which consisted in division of nerve fibres in the trunk itself. He had never performed a true Stoffel's operation, he had preferred a freer dissection, and the exposure of the muscular branch after it had left the parent trunk. The proof of specific muscular branches by electrical stimulation was more certain. He had had success in overcoming pronator spasm in hemiplegia and adductor spasm in spastic paraplegia. Where adaptive shortening had occurred he thought it necessary to perform a plastic operation upon the tendon: he had combined this with neurectomy in cases in which severe spasmodic contracture was still present. He thought splints and retention apparatus would still be required, especially in dealing with the spasm of the hemiplegic hand. Tendon transplantation had proved useful in restoring muscular balance. Re-education was essential after all methods of treatment.

Mr. H. A. T. FAIRBANK

said he did not entirely share Mr. Bankart's optimism with regard to the Stoffel operation. A method, or combination of methods, must be selected for each case. He asked Mr. Bankart's opinion as to the most suitable age for operating. Personally he thought that if the child could sit up and at least make an attempt at walking, operation on the legs might be done at the age of 3 years. With regard to the arm he was not sure whether delay to the age of 6 years or more was not advisable. Cases of patients aged 10 to 14 years were much more difficult to deal with. In spastic equinus secondary contracture should be sought for, and if present open lengthening of the tendo Achillis done. If the equinus was purely spastic the case was suitable for Stoffel's operation. He had yet to learn that the Stoffel operation on the internal popliteal nerve gave any better results than open tendon-lengthening when properly done. In older cases it might be necessary to combine the two operations. In valgus cases he had obtained some good results by several methods, including Stoffel's, and many unsatisfactory results. He usually exsected half the nerve supply to the peronei, exposing the various branches by a long incision, and not tackling the external popliteal nerve itself, as Mr. Bankart had done. The Stoffel operation gave good results in flexion of the knee, but here again the older cases required tenotomy of the hamstrings in addition. For adductor spasm the Stoffel operation was undoubtedly good. When both branches of the obturator nerve required resection, he preferred the abdominal extraperitoneal route, which was easy and

well removed from sources of infection. For inversion of the leg he had usually done Jones's operation, but had once exsected portions of the superior gluteal nerve through a vertical incision above the trochanter with satisfactory result. In the arm the paralytic element as opposed to the spastic was often marked, and accounted for the disappointing results. Operation on the median nerve had not given good results. In certain cases he favoured more extended trial of transplantation. For instance, when voluntary power in the extensors was very feeble and there was marked spasm of the flexor carpi ulnaris, this muscle should be grafted into the extensors of the fingers rather than entirely wasted by division of its nerve supply. He had tackled the nerve supply to this muscle in three cases without much satisfaction. He usually attacked the motor nerves after they had left the parent trunk rather than the trunk itself. He would like to know whether Mr. Bankart thought it necessary to exsect more than three-quarters of an inch of the nerve fibres when only a portion of a branch was being removed. In a young child it was difficult to split the nerve for any distance on account of the small size of the branches. He agreed as to the disappointing results following Foerster's operation—an operation attended with a considerable mortality. He did not agree that splints were unnecessary: after treatment of equinus he used rectangular "tin shoes" with boots wedged on the inner side. Some of the older cases with flexion of the knee required walking-irons for a time.

Mr. ROCYN JONES

said that from his limited experience he agreed almost entirely with what Mr. Bankart said about the value of Stoffel's operation for spasticity in the lower limbs, but for the upper limb he was not sure that Stoffel's operation was the best surgical procedure. It was only in the lesser degrees of spasticity that any improvement of function could be expected to occur in the arm, and he had not seen any great benefit when Stoffel's operation had been performed. Tendon transplantation seemed to him a better operation, performed as for dropped wrist in irrecoverable musculo-spiral paralysis, i.e., attaching the pronator radii teres to the carpal extensors, the flexor carpi radialis to the thumb extensors, and the flexor carpi ulnaris to the extensors of the fingers. There had been far greater improvement under this operation than by the use of Stoffel's. As for spasticity in the lower limbs, Stoffel's operation in the main was much the best, but he rather favoured elongation of the tendo Achillis as well: this was done subcutaneously with subsequent splinting, the foot being kept at right angles and the knee extended. Hamstring and adductor spasm were sufficiently relieved by Stoffel's operation alone.

Mr. GORDON PUGH

asked whether Mr. Bankart or any of the neurologists present had had experience of decompression in cerebral spastic paralysis. He referred to the work of Wyeth and Sharpe, who reported favourably on the operation in cases resulting from birth traumatism and showing an existing increased intracranial pressure on ophthalmoscopic examination confirmed by lumbar puncture. The aim was to relieve the pressure on the cortex of the cyst-like formation which resulted from the primary supracortical hæmorrhage. Cases in which the condition was due to lack of development of the brain or to meningitis were not operated upon. In a series of 1,026 patients examined one in every four had shown

definite signs of increased intracranial pressure. These cases, 236 in number, aged between 2 and 6 years usually, had been decompressed; twenty died and thirteen showed no improvement, but the results in the others were stated to have been very satisfactory both physically and mentally.

Mr. W. ROWLEY BRISTOW

said that he agreed with Mr. Bankart in the main, and pointed out that he was the first among the orthopædic surgeons in London to popularize Stoffel's operation. He said that in his experience it was always safe to divide completely both branches of the obturator nerve, and that he himself preferred to divide it above the foramen by the extraperitoneal route. He suggested that the division of some nerves—for example, the superior gluteal—was not always very easy, particularly in a fat subject. He further pointed out that it was unwise in the present state of knowledge to make definite statements about individual nerve division, as the whole subject was still fairly recent, and the results largely depended upon the experience of the individual surgeon. Because a man was overcoming a spastic equinus to-day by elongating the tendo Achillis, it did not necessarily follow that he would be wise in continuing this if he found that division of the tract in the internal popliteal nerve gave as good, or better, result. He thought that more investigation was required, and expressed himself as very hopeful of the results of this operation, which he regarded as being of wide application.

Dr. A. FEILING

said he would discuss the subject briefly from the neurologist's point of view. He had had the privilege of seeing some of Mr. Bankart's cases, and Mr. Bankart had operated on cases for him. He was glad to hear that the majority of opinions seemed to condemn Foerster's operation for the relief of spastic paralysis in children. The results he had seen were very disappointing, and entirely out of proportion to the severity of the operation. Hence Stoffel's operation, as described by the opener of the discussion, was to be welcomed. He was sorry that Mr. Bankart had not included for consideration other forms of spastic paralysis besides the cerebral palsies of children, for in selected cases he felt sure that there was a field for usefulness for the Stoffel operation. In selecting cases, however, for this form of treatment two important principles should be borne in mind. First, the diagnosis must be clear both as to the pathological cause of the disease as well as of the anatomical site of the lesion. And, secondly, the lesion must not be a progressive one. He concluded by briefly describing a case of spastic paraplegia in an adult female, the subject of the chronic spinal type of disseminated sclerosis, in whom Mr. Bankart had brought about very considerable relief by means of carefully designed operations on the peripheral nerves. In this case, from being bedridden with the legs in a state of spasmodic flexion, the patient had been provided with two straight legs, stiff it was true in extension, but capable of serving as useful props.

Mr. R. C. ELMSLIE

pointed out the necessity for accurate diagnosis before operating, as he had operated, naturally without benefit, upon two cases of congenital syphilis. Since then he had had the Wassermann reaction performed upon all cases.

He thought it would be a mistake to scrap the old operation, especially in the case of the calf muscles, on account of the difficulty of judging the proportion of the nerve to exsect. He asked whether flexion at the hip and knee were not interrelated, and what could be done for contracture of the psoas. He advocated re-education after Stoffel's operation.

Mr. NAUGHTON DUNN

said that they were apparently all agreed that the most effective operative treatment to overcome spastic deformities was by direct operation on the muscles or their motor nerves. About the same time as Stoffel was doing his original work he had operated on a series of cases of spasmodic flat foot by crushing the motor nerve branches of the peroneal muscles. The results were excellent in that the patient walked early without splintage, and during temporary paralysis of the peroneal muscles the invertors of the foot had an opportunity of developing and restoring the arch. In spastic paralysis, however, the problem was complicated, as Dr. Riddoch had pointed out, by the fact that the extensors of the limb were, in the severer cases, also in a state of hypertonus, and their range of voluntary control was limited. He could hardly believe that Mr. Bankart would dispense with splintage and re-education in these cases. The main point he wished to emphasize was that the prognosis in spastic paralysis depended on the degree of voluntary control of the limb rather than on the particular operation performed to overcome the spasm of the muscles responsible for the deformity. The other point to which he wished to draw attention was the fact that occasionally flexion of the knee and equinus deformity of the foot might be secondary to simple adductor spasm. This could be demonstrated by seating the child on a table and observing that voluntary extension of the knee and dorsiflexion of the foot were present. In these cases the knee flexion and the equinus were assumed by the child in order to make locomotion possible in the presence of adductor spasm, so that only operation to overcome the latter was necessary. Mr. Bankart had given a careful and interesting review of the treatment of a very difficult type of case.

Mr. P. JENNER VERRALL

said he had had good results from Stoffel's operation in the legs of spastic children, but not in the arms of adults. He had successfully treated flexion at the hip by section of the psoas at its insertion, leaving the iliacus intact.

Mr. BANKART (in reply)

said that Dr. Riddoch was talking of recoverable lesions, while he (the speaker) had devoted his paper to permanent conditions in which contracture would recur, even after prolonged splintage, as soon as the splints were removed. He agreed that Stoffel was not the first to divide the obturator nerve for adductor spasm, but he was the first to formulate a definite plan of treatment by neurectomy for spastic paralysis, and it was the best treatment that had yet been devised for physiological contracture. When there was anatomical shortening the muscle must be attacked directly, and he sometimes combined this measure with nerve section. In the actual section of the nerve there was a tendency to do too little rather than too much. He divided both divisions of the obturator nerve in the thigh, and personally was disinclined to perform

the abdominal operation. There were no age-limits for the operation, but he rarely saw cases younger than 2 or 3 years of age. The true Stoffel operation was carried out upon the main nerve trunk and not upon nerve branches. He had had no experience of decompression in this class of case. He doubted the value of such a procedure when spastic paralysis was well established. He never used splints after Stoffel's operation, except when this was combined with operation upon the muscles or tendons. He agreed that some after-treatment might be beneficial, but he emphasized the fact that after Stoffel's operation prolonged after-treatment and particularly the use of cumbersome walking instruments were entirely unnecessary.

Section of Orthopædics.

President—Mr. T. H. OPENSHAW, C.B., C.M.G., M.S.

Four Cases of Flexion Contracture of the Forearm treated by a Muscle-sliding Operation.

C. MAX PAGE, F.R.C.S., M.S.Lond.

I HAVE now operated on ten cases by this method. In the last few I have carried out the most complete operation and up to date the results in these appear to be the best.

The technique is as follows: A skin incision is made from 3 in. above the inner condyle on the line of the ulnar nerve to the inner side of the subcutaneous border of the ulna at the junction of the middle and lower thirds of the forearm. The ulnar nerve is first isolated at the level of the elbow and transposed to the front of the joint out of harm's way. The attachments of the whole flexor group of the forearm are then systematically detached from their origin, the supracondylar ridge cleared and the common tendon cut close to the internal condyle and stripped from the lateral ligament, the elbow-joint being usually opened at this stage. Next, the aponeurosis on the ulnar side of the ulna is cut through in its whole length close to the bone. The muscle mass so loosened is raised with a raspator; any definite tendinous origins below the coronoid process of the ulna are divided, the insertion of the brachialis anticus being fully exposed. If the flexor longus pollicis is contracted the process of muscle-stripping is carried across the interosseous membrane, so that the attachment of the thumb flexor to the front of the radius can also be raised. Lastly, the bicipital fascia is cut through if it appears to offer any opposition to the descent of the muscle group. If care is taken to keep close to the bone no important vessels are divided, and no damage can be done to any nerve except the anterior interosseous and the terminal part of the internal cutaneous. The separation of muscles and fascia is carried out to such an extent that nearly full correction of the contracture, apart from phalangeal joint deformities, is possible at once. The whole muscle group will be made to descend an inch or more from its origin. The hand is put up in the corrected position on a metal splint, which is replaced in a few days' time by a properly fitting plaster mould.

Voluntary control of the mobilized muscles is lost or becomes very weak for a few days after the operation, and is then gradually recovered. The

plaster mould is worn for most of the day during muscle re-education, so as to enable the extensors to recover tone and become effective opponents. Careful splintage and the usual physiotherapeutic measures must be employed in order to complete the cure.

Case I: Flexion Contracture of the Right Forearm.—History: Gunshot wound right arm, 1917, followed by complete paralysis in distribution of the median nerve below the wrist. In 1920 an autogenous graft on the median nerve was attempted. Treatment subsequently by splintage and massage to correct contracture of the flexor muscles.

State on November 27, 1922: Anæsthesia in median nerve distribution, no faradic response in median intrinsics. The thumb, index and middle fingers were held flexed at the interphalangeal joints and could not be passively or actively extended.

Operation on November 30, 1922: The ulnar nerve was displaced forward; the flexor group of muscles was then completely detached from its origin including the deep attachments of the flexor sublimis and profundus digitorum and flexor longus pollicis. The fingers were kept straight on a splint for four weeks, only being removed for massage. Still under treatment. Contracture corrected. Function of the hand good.

Case II: Flexion Contracture of Forearm associated with Injury to the Ulnar Nerve.—History: Gunshot wound left elbow, August, 1918, followed by ankylosis of left elbow and partial lesion of the ulnar nerve. The ulnar nerve was sutured in 1920.

State on July 18, 1922: No faradic reaction in ulnar intrinsics; ulnar sensory loss. Contracture of all fingers of left hand. Thumb normal.

Operation on August 3, 1922: Ulnar nerve explored; a 5-inch gap was present; on account of elbow fixation no attempt was made to repair this. The flexor group of muscles, except flexor longus pollicis, was detached from its origins and the contraction of the fingers corrected. Except when removed for massage the hand was kept extended until December 12, 1922, after which the splint was only worn at night. Contracture corrected. The function of the hand is good, taking into consideration the complete loss of conduction in the ulnar nerve.

Case III: Flexion Contracture of Forearm secondary to Ischæmic Paralysis.—History: Gunshot wound arm, 1917; no injury to bone or nerves. Treatment by splintage was carried out for a year.

State on September 2, 1922: Obstinate flexion contracture of all the fingers of the right hand. Electrical reaction to the median and ulnar nerves normal.

Operation on September 7, 1922: Ulnar nerve transposed, the attachments of the flexor group, except flexor longus pollicis, were detached, allowing of correction of the deformity. The hand was put up in full extension five weeks. The correction of the contracture is not quite complete. The hand is fairly useful but the movements of the fingers are not fully independent.

Case IV: Flexion Contracture of Forearm following Hemiplegia.—History: Gunshot wound of the head, 1918, followed by hemiplegia from which there was gradual recovery in the leg and partly in the arm. The flexors of the forearm of the right side remain contracted. Massage and splintage treatment of deformity carried out for several years.

Operation on March 13, 1922: The common origin of the flexors was divided and some of the deep attachments were also separated. The flexor longus pollicis origin was not touched. The hand was kept in the fully extended position for three weeks after operation. The result is not very satisfactory; there is fair correction of the deformity but the control of the fingers remains inco-ordinate.

DISCUSSION.

Mr. WHITCHURCH HOWELL found the operation useful when the scar was above the elbow, but when it was below that joint he preferred to excise the scar in the muscles.

Mr. ROWLEY BRISTOW thought the operation was useful in selected cases where there was no contracture of the capsule in the finger joints.

Mr. T. H. OPENSHAW (President) said that in similar cases he excised the scar tissue, not once only but sometimes three times, and gradually got the fingers and wrist into good position by plaster of Paris splintage and massage. If the nerve was injured it should be resected and end-to-end suture obtained. The scar and the nerve injury should be dealt with as separate conditions. He also thought that when the injury was in the forearm, detachment of the flexors from the internal condyle would not relieve the contracture.

Mr. MAX PAGE (in reply) said he did not think that the local excision of scar was a very satisfactory procedure in the type of case he had shown. In these instances, there was for various reasons a general shortening of the muscles of the forearm. The procedure he put forward was a substitute for general tendon lengthening; it had the advantage over the latter operation of not interfering with the normal independent movement of the tendon. As could be seen in the cases he had shown, the flexor muscle group became securely fixed again soon after operation without any material interference with muscle function and yet gave sufficient lengthening to allow of the correction of considerable deformity.

Case of Renal Dwarfism shown after Operation for Genu Valgum.

By PAUL BERNARD ROTH, F.R.C.S.

K. A. P., AGED 13, sent to me by Dr. Maurice Davidson; very small for her age, thin and sallow, with extreme genu valgum (10 in. between malleoli), enlargement of internal condyles, and characteristic urine (pale, specific gravity 1005, albumin one-half on boiling and standing). Radiogram: Internal condyles enlarged, spongy looking transparent bone, and patches here and there in diaphysis where bone appeared almost missing. Lower ends of ulnæ similar.

The urea concentration test was applied. If 15 gm. urea are given by mouth the percentage of urea in the urine and in the blood two hours later should be normally 2 and 0.03 respectively; the normal urea concentration factor (i.e., percentage of urea in the urine, divided by that in the blood) is thus 66.6, the kidneys concentrating the urea 66.6 times. In the case shown, the test was made in June, 1922, and repeated in January, 1923. The percentage of urea was 0.9 and 0.8 respectively in the urine, and 0.091 and 0.124 in the blood; the urea concentration factor was thus 10 and 6.45 respectively. In other words, her kidneys last January were only doing one-tenth of the urea concentrating work they should be doing, and the condition had become worse notwithstanding the rest in bed. As the patient was completely crippled, however, double supracondylar osteotomy was performed in February under gas and oxygen. Nothing untoward resulted.

Case of Ocular Torticollis.

By PAUL BERNARD ROTH, F.R.C.S.

D. T., AGED 8, was sent up by the L.C.C. school medical officer as a case of wry-neck; with history of having had wry-neck from birth and of having attended Paddington Green Children's Hospital when 3 months of age to have massage applied to the right side of neck.

On examination it was observed that, though the patient carried her head laterally flexed to the right, there was no shortening of the right sternomastoid muscle, and that she was wearing badly centred glasses. She was sent to see Mr. Hine, who reported that this was a case of true ocular torticollis, due to a left convergent strabismus. He has now corrected this by suitable glasses, and patient is being trained to hold her head straight and is improving rapidly. Mr. Hine remarks: "With glasses off left eye is markedly up with head straight, and comes into middle of palpebral fissure when head is turned over to right. It is rather difficult to explain this, but there is no doubt the glasses correct it. I suppose holding the head over to right tends to set up a natural impulse to pull left eye down to bring eyes a bit more level."

DISCUSSION.

Mr. A. B. BANKART said that in view of the history he had obtained from the mother he regarded the case as one of ordinary torticollis in which the contracture of the sternomastoid had been overcome by the massage which had been applied during two-and-a-half years' attendance at the Children's Hospital in infancy.

Mr. H. A. T. FAIRBANK said he had seen two cases of ocular torticollis in the last few years, and these closely corresponded with the case shown. There was no change in the sternomastoid, and if the child were told to follow the finger the axes of both eyes remained directed at the finger until it passed over to the right side when that of the left eye would aim upwards and to the right. This served to differentiate these cases.

Case of Snapping Hip.

By B. WHITCHURCH HOWELL, F.R.C.S.

PATIENT, a cellarman, sustained a gunshot wound of the left buttock in March, 1918, and this was followed by a snapping right hip. The entrance scar is in the middle line of the buttock, just above the gluteal fold, and the small exit scar in the groin over the origin of the adductor. There is also an elliptical scar from well below the anterior superior iliac spine to the junction of the upper and middle thirds of the thigh, indicating an operation for snapping hip performed at Headington by Mr. Girdlestone in July, 1918. There is wasting of muscles of left lower limb without actual shortening; full range of knee movement with definite lateral mobility, especially on inner side; and slight limitation of flexion at hip. X-ray negative. The snap appears to be due to the slipping of the gluteus maximus over the great trochanter, and is not connected with the tensor fasciæ femoris. There is no apparent telescoping. The joint is very insecure, so that patient is unable to lift barrels, &c. Opinions are desired as to the best treatment.

Case of ? Charcot's Knee.

By H. A. T. FAIRBANK, D.S.O., M.S.

H. W., MALE, aged 48. Broke right patella in 1914. Wired. Wire since removed. Had trouble in left knee in 1916; excised at Guy's Hospital. Complains of insecurity of right knee. No pain.

Right knee shows great enlargement of bones; marked varum deformity with obvious ulceration of articular surfaces. Abnormal mobility very marked. Very little excess of fluid. X-ray shows gross changes in joint with loss of bone on the inner half of the joint and enormous development of nodules of bone in and around the joint.

Left knee shows excellent result of excision, with bony ankylosis.

Eyes: Pupils react slowly to light.

Knee-jerk present on right. Plantar reflex-flexion both sides.

Wassermann reaction negative.

Should this knee be excised?

The general opinion expressed by speakers in discussion was in favour of excision, as even if bony union did not occur the straightened knee would be much easier to control by apparatus.

**Case of Dislocation of Patella outwards, secondary to
Osteomyelitis of Femur.**

By H. A. T. FAIRBANK, D.S.O., M.S.

F. W., BOY, aged 6. History: Operated upon when 7 weeks old for "cellulitis" of left leg. First seen April, 1922, with marked genu valgum and outward dislocation of patella, there being a thick scar on the outer side adherent to lower third of femur. April 27, 1922: Excision of scar. May 24, 1922: Osteotomy on left femur for correction of knock-knee. Position of patella much improved, but later found to be going outwards again, with strong tendency to relapse to condition of genu valgum. X-ray shows some slight thickening of lower third of femur with epiphyseal line apparently normal. Patella not ossified.

The genu valgum and tendency to relapse to this state are regarded as due to the scar rather than to previous damage to the outer part of the epiphyseal line. In view of the strong tendency to genu valgum opinions are asked as to the best time for operating on the displaced patella.

The general opinion expressed by speakers in discussion was in favour of operating on the displaced patella soon.

Case of Pseudo-Coxalgia in an Adult.

By G. PERKINS, M.Ch., F.R.C.S.

PATIENT, a male, aged 22, sought advice on account of slight pain in his left leg. The onset of the pain was insidious, and the duration about six months. He had never before, to his knowledge, had anything the matter with the leg, although his tailor had told him two years ago that it was short.

On examination of the left hip, tenderness was found over the head of the femur on palpation. The hip at rest was slightly everted; there was no adduction deformity or fixed flexion. All movements were limited in the outer half of their range, but throughout the inner half they were painless and free. The amount of true and apparent shortening was $1\frac{1}{2}$ in., and the trochanter was correspondingly raised. The radiogram showed well marked flattening and widening of the head of the femur. No arthritic changes could be seen, and the interarticular space was of normal depth.

The patient was an athlete; and during the week before the consultation had danced two whole evenings, and had played a game of Rugby football.

Section of Orthopædics.

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Case of Intracapsular Fracture of the Neck of the Femur.

By R. C. ELMSLIE, O.B.E., F.R.C.S.

DR. E., aged 69, fell on September 22, 1922, sustaining an intracapsular fracture of the left femur. Next day, under an anæsthetic, the hip was abducted, rotated inwards and fixed in plaster of Paris from the toes to the pelvis, with a wing of plaster up the right side of the chest to prevent the left hip adducting. The plaster remained on until November 18. After this, massage movements and re-education were commenced in bed, and the patient began to walk on the limb at the end of three months from the date of the fracture.

X-rays taken April 4, 1923, show sound bony union with slight coxa vara deformity. There is good range of movement in the hip joint; the walk is almost natural and the patient is able to walk considerable distances and get up and down stairs without difficulty.

Further X-rays were shown of a second patient, a woman aged 43, who sustained an intracapsular fracture on April 9, 1922: she was treated on exactly similar lines, and she walks with a slight limp with very good functional use, except for a considerable coxa vara deformity, which limits abduction of the hip.

Case of Osteitis Deformans.

By PAUL BERNARD ROTH, F.R.C.S.

MRS. S. H., aged 61, was sent to me on April 27, 1922, by Mr. Ellwood. Patient informed me that the *right* leg had been bending outwards below the knee for fifteen years or more, but that during the last two to three years the bend had become much worse. "The bone seemed to bend under her." There was a large amount of pain attached to it. Though she was more or less relieved by rest, quite often, even when in bed, the pain would come on very badly and keep her awake; it was a dull, aching pain, much worse in windy weather. She was now scarcely able to walk about her home because of the pain, and hardly ever went out. The pain, bend, and weakness were all increasing.

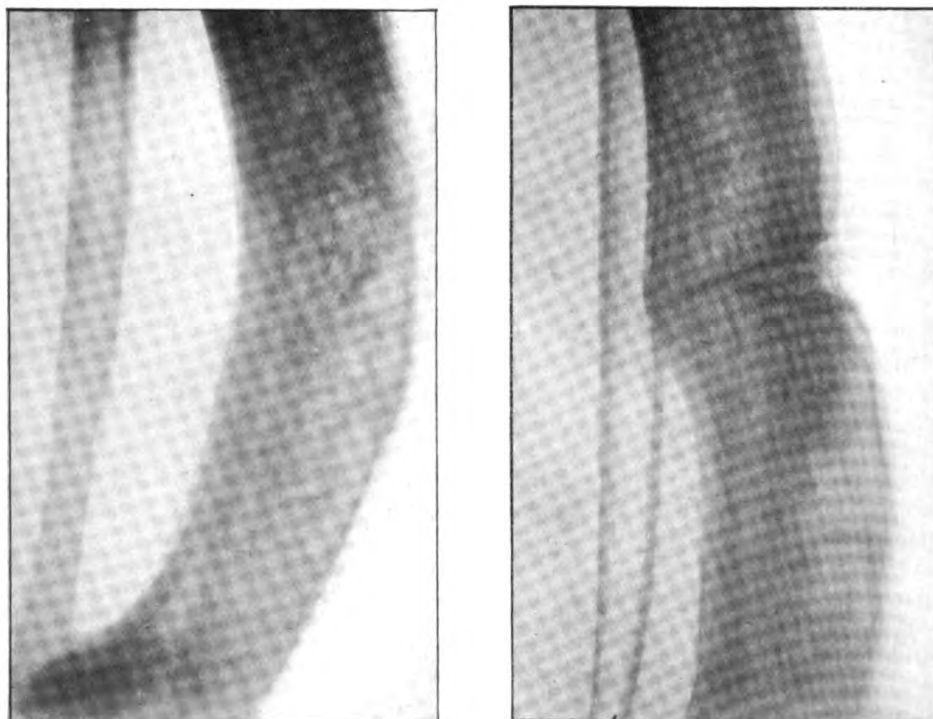
During the last year the *left* leg had begun to go in the same way; the condition had begun acutely, "as if she was going to have an abscess in the

[May 1, 1923.]

50 Roth: *Osteitis Deformans* ; Howell: *Tendon Transplantation*

leg." She had attended a hospital and had been advised a year ago to have the right leg cut off.

Examination.—Right leg: Typical osteitis deformans. Tibia much enlarged, and bowed forwards and outwards, the curve forming one-fifth of the circumference of a circle. Left leg: Tibia upper half was enlarged anteriorly.



Before operation.

After cuneiform osteotomy showing bony union.
Osteitis deformans of right tibia.

I decided to perform cuneiform osteotomy of right tibia, and then to apply a walking instrument. Operation carried out on June 7, 1922, and plaster applied. Plaster removed on June 20, 1922, and tin night shoe applied; union had already begun.

October 6, 1922: Fitted satisfactorily with Thomas's calliper knee splint.

April 20, 1923: Very good bony union with leg straight; walked about in house for three hours a few days ago with splint off without discomfort. X-ray shows sound bony union.

Case of Tendon Transplantation.

By B. WHITCHURCH HOWELL, F.R.C.S.

W., AGED 31, sustained a gunshot wound of the left arm in June, 1917, which caused musculo-spiral paralysis. The nerve was sutured in the following September, and recovery followed, except in the extensor longus pollicis.

In January, 1923, the flexor carpi radialis was inserted into the extensor longus pollicis, which was freed from its groove in the radius and divided transversely, a direct subcutaneous pull being thus obtained. The limb was placed in plaster of Paris with the wrist hyper-extended, and early treatment by faradism and re-education adopted.

DISCUSSION.

Mr. D. M. AITKEN congratulated Mr. Howell on the excellent result which had followed his boldness in taking the tendon out of its groove to obtain a straight pull.

Mr. T. H. OPENSHAW (President) regarded the operation as a distinct advance.

Case of Renal Dwarfism.

By W. H. OGILVIE, M.S.

P. M., AGED 17, motor apprentice. History: Only child. Has always been considered small for his age, but learned to walk and talk at the usual time, and at school was good at lessons, and played cricket and football. Suffered from children's complaints, but no history of illness involving more than a week in bed. His head has always been square. He first noticed stiffness in his knees in 1921, but was able to walk, and began work as an apprentice. In June, 1922, he tripped on a platform while going to work, and sprained his ankle. He was in bed for a week, and when he got up was unable to walk. Since then he has never walked without assistance, but has been better during the last fortnight. He suffered from nocturnal enuresis till the accident, but since then this has disappeared.

Present condition: Boy very small for his age, looking about 12. External genitalia well developed, but pubic hair scanty, and voice childish. Muscular development very poor; very little subcutaneous fat. Skeletal system: Head square. Obvious enlargement of all epiphyses, most marked at ankles and wrists. Marked genu valgum, tibiae making an angle of 30° ; this has developed during last two years. Movement at joints little affected, but flexion at ankle, extension at knee, and abduction at hip all slightly limited. Reflexes all normal. Blood-pressure: Diastolic, 70, systolic, 122. Urine: Strongly alkaline, containing pus cells and triple phosphates; specific gravity, 1009; albumin 2 per 1,000, no sugar, no casts. *Bacillus coli communis* in large numbers. Blood urea: 2.7 gr. per 1,000 cubic centimetres, or nine times the normal amount. Wassermann reaction negative. Gait: Will not walk without support, and then only stiffly and slowly.

X-rays: Kidneys show no evidence of calculi; all epiphyseal spaces wide and ragged in outline; no cupping of diaphyses.

The history and appearance are typical of a renal dwarf. There is probably a functional element in the inability to walk.

**Specimen of Synostosis of Phalangeal Joints
? Congenital in Origin.**

By W. H. OGILVIE, M.S.

SOURCE: The skeleton of a hand shown is from an anatomical subject in the dissecting room at Guy's Hospital. A similar condition was present in the other hand, but not in the toes of the feet. The body was that of a man aged 58, who died of "cardiac failure and chronic bronchitis."

52 Ogilvie: *Specimen of Synostosis of Phalangeal Joints*

Description: The thumb is normal. The first interphalangeal joint of the index appears normal, but only a limited amount of flexion is possible. The first interphalangeal joints of the middle, ring, and little fingers are completely ankylosed. In addition, the second phalanx of the little finger is only 3 mm. long, so that the tip of the distal phalanx only reaches just beyond the first interphalangeal joint of the ring finger, instead of to the second joint. In the wrist, the os magnum and unciform bone are fused.

X-rays (with specimen) show the above abnormalities. No evidence of arthritis.

Points of interest: In the circumstances, no history was obtainable. There is no evidence of rheumatoid arthritis in the other articulations of the hand, while the remaining joints in the body appeared normal. The shafts of the bones show neither thickening round the articular margins, nor atrophy. They are well formed, and at the joint-line show a contour resembling that of the head and base of the corresponding phalanges. There is no ulnar deviation at the metacarpo-phalangeal joints.

Is this an old arthritis, or a congenital condition? The fusion of bones in the wrist suggests a congenital condition.

PROCEEDINGS
OF THE
ROYAL SOCIETY OF MEDICINE

EDITED BY
SIR JOHN Y. W. MACALISTER

UNDER THE DIRECTION OF
THE EDITORIAL COMMITTEE

VOLUME THE SIXTEENTH

SESSION 1922-23

SECTION OF OTOTOLOGY



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1923

Section of Otology.

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SECTION OF OTOTOLOGY.

CONTENTS.

October 20, 1922.

SOMERVILLE HASTINGS, M.S., and Major W. S. TUCKER, R.F., D.Sc.	PAGE
An Attempt to Standardize Tests for Hearing	1
E. D. D. DAVIS, F.R.C.S.	
A Temporal Bone from a Case of Tuberculous Lateral Sinus Thrombosis and Extra-cerebellar Abscess	5
Sir JAMES DUNDAS-GRANT, K.B.E., M.D.	
Case of Acute Suppuration in one Ear subjected to Early Operation on Account of Complete Deafness of opposite Ear	6
ARCHER RYLAND, F.R.C.S.Ed.	
Case of Absolute Bilateral Deafness, with almost Complete Loss of Vestibular Activity	7
G. J. JENKINS, F.R.C.S.	
Tinnitus associated with Facial Spasm	8

November 17, 1922.

ALBERT A. GRAY, M.D.	
Some Cases of Otosclerosis with an Unusual Symptom (Otosclerosis Paradoxa) (Abstract)	9
T. B. LAYTON, D.S.O., M.S.	
"The Disease of not Listening, the Malady of not Marking" (Abstract)	12
Cases and Specimens shown	14

January 19, 1923.

A. LOGAN TURNER, M.D., and J. S. FRASER, M.B.	
Labyrinthitis as a Complication of Middle-ear Suppuration (Abstract) ...	15
Sir JAMES DUNDAS-GRANT, K.B.E., M.D.	
Case of Complete Nerve-deafness due to Syphilis of Internal Ears; Caloric and Rotation Tests Negative, Galvanic Positive	16
F. J. CLEMINSON, F.R.C.S.	
Case of Otitis Media with Facial Palsy, following Scarlet Fever; Specimens (Malleus and Incus) shown	17

Sir JAMES DUNDAS-GRANT, K.B.E., M.D.	PAGE
Case of Vertigo, with Fixation of the Ossicles, cured by Ossiculectomy	18
NORMAN PATTERSON, F.R.C.S.	
Parotid Fistula following Mastoid Operations	19
SYDNEY SCOTT, M.S.	
Ossification of Incus to Tegmen	20
E. LOWRY, M.B.	
Case of Acquired Atresia of the Auditory Meatus	20
Sir JAMES DUNDAS-GRANT, K.B.E., M.D.	
Case of Vertigo (simulating "Ménière's Disease") with Anomalous Nystagmus Reactions...	20

February 16, 1923.

G. J. JENKINS, F.R.C.S.	
Otosclerosis and Osteitis Deformans: A Pathological and Clinical Comparison (Abstract)	21
J. F. O'MALLEY, F.R.C.S.	
Case of Necrosis of the Left Temporal Bone, involving Facial Nerve and Labyrinth, following Triple Infection of Scarlet Fever, Measles and Diphtheria, in a Child aged 7	29
H. J. BANKS-DAVIS, M.B.	
(1) Parotid Fistula in the Scar of an Old Mastoid Wound	30
(2) Laceration of Meatus and Tympanic Membrane produced by a Celluloid Knitting Needle	30

March 16, 1923.

F. J. CLEMINSON, M.Ch. (Shown by).	
Case of Acousticus Tumour (Right); Operation by Sir Victor Horsley in 1912; Removal of Tumour; Recovery	31
F. M. R. WALSH, M.D. (Shown by). (Introduced by Mr. F. J. CLEMINSON).	
Specimen of Brain and Acousticus Tumour	32
F. M. R. WALSH, M.D.	
Acousticus Tumours	32
WILFRED TROTTER, M.S.	
Surgical Treatment of Eighth Nerve Tumours (Abstract)	37

April 20, 1923.

E. D. D. DAVIS, F.R.C.S.	
The Morbid Anatomy and Drainage of Otitic Meningitis	43
Sir JAMES DUNDAS-GRANT, K.B.E., M.D.	
(1) Case of Complete Deafness dating from a Fall	47
(2) Case of Deafness greatly increased after a Fall	48
(3) Case of Long-standing Deafness attributable to Falls on the Head: Improvement	49

Contents

v

May 18, 1923.

Exhibited by W. M. MOLLISON, M.Ch.	PAGE
An Instrument for assisting the Deaf	51
FREDERICK SYDENHAM, F.R.C.S., and DAN MCKENZIE, M.D.	
Epidemic Cerebro-spinal Meningitis associated with Acute Suppuration of the Middle Ear	51
DAN MCKENZIE, M.D.	
(1) Epileptiform Seizures subsequent to Operation for Temporo- Sphenoidal Abscess	52
(2) Otitic Pterygo-maxillary Abscess induced by Thrombo-phlebitis of the Jugular Bulb	53
T. H. JUST, F.R.C.S.	
Brain Abscess due to Otitic Infection; Right Temporo-sphenoidal Abscess without Clinical Signs	54
SYDNEY SCOTT, M.S.	
(1) Left Temporo-sphenoidal Abscess; Amnesia for Names of Objects ...	55
(2) Cerebellar Abscess; Sudden Coma and Apnoea; Recovery after Operation during Artificial Respiration	56
(3) Cerebellar Abscess Five Weeks after Onset of Acute Otitis Media, Right Side	57
W. M. MOLLISON, M.Ch.	
Case of Vertigo cured by Opening the External Semicircular Canal ...	60
T. H. JUST, F.R.C.S.	
(1) Sequestra removed from the Region of the Eustachian Tube during a Radical Mastoid Operation	61
(2) Section of Ependymal Glioma growing from the Floor of the Fourth Ventricle, simulating a Cerebellar Abscess, in a Case of Bilateral Chronic Suppurative Otitis Media	62

CORRIGENDUM.

Proceedings, No. 10, August, 1923 (Section of Otology, p. 57).

In Mr. Sydney Scott's case of "Cerebellar Abscess: sudden Coma and Apnoea: Recovery after Operation during Artificial Respiration," lines 3, 4: instead of "Artificial respiration was applied and the operation *stopped*, but breathing remained *suspended*," read the following: "Artificial respiration was applied, and the operation *continued*, while breathing remained suspended."

This correction was received too late for insertion.

The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

LONDON

JOHN BALE, SONS AND DANIELSSON, LTD.,
OXFORD HOUSE,

83-91, GREAT TITCHFIELD STREET, OXFORD STREET, W.1.

Section of Otology.

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An Attempt to Standardize Tests for Hearing.

By SOMERVILLE HASTINGS, M.S., and Major W. S. TUCKER,
R.E., D.Sc.

(I) INTRODUCTION.

By SOMERVILLE HASTINGS, M.S.

SEVERAL years ago a Sub-committee of this Section spent a good deal of time investigating the tests that are usually employed in the examination of deaf people, and in endeavouring to determine which were of most value.¹ None of the methods usually employed for estimating the hearing power of an individual, for instance the tuning fork, conversational voice, whisper, acoumeter, appeared to be really satisfactory. They were all influenced by the relation of the ear to the waves of sound, reflections from surrounding objects and interference by extraneous sounds. Moreover, it is difficult or impossible to maintain the same source of sound in different observations.

About three years ago Major W. S. Tucker, who has kindly come here this afternoon, invited me to join him in trying to work out some better method of the estimation of the hearing capacity. Major Tucker, whose method of locating German guns by the sound of their firing had done so much to win the war, was desirous of estimating the hearing capacity of aeroplane listeners.

We have together tried a good many different methods, and the apparatus we are showing to-day will need certain modifications to fit it for clinical use. It depends on the following principle: A wireless valve can be made to give out vibrations of any frequency; the current from the valve passes to a telephone and by means of a Wheatstone's bridge resistances are interposed until the sounds are no longer heard. Its advantages are: (1) That the test is but little affected by slight extraneous sounds; (2) that a pure sound of any frequency and intensity can be obtained; and (3) that the amount of energy set free by it can be determined.

J. P. Minter, in the *Physical Review* of February, 1922, describes a similar method, and gives extensive results but very few details of the apparatus used.

As I am not a physicist, I think it best at this point to ask Major Tucker to describe the apparatus to you in detail, and demonstrate the test on an individual, after which, I shall briefly indicate some of the results we have obtained by its use on deaf people.

¹ *Proceedings*, 1917-18, xi (Sect. Otol.), p. 4.

(II) DEMONSTRATION OF STANDARD APPARATUS FOR TESTING HEARING.

By Major W. S. TUCKER, R.E., D.Sc.

One of the new operations required of the fighting services during the war was that of listening for enemy aircraft. In general, the personnel for this work was selected in a very casual kind of way, and the chief qualification seemed to be the general unfitness of the men for any more strenuous occupation. In the later stages, some crude tests were applied to select the best men, but proficiency and intelligence in listening were not recognized by any increase of pay. The installation now to be described was designed primarily for testing such listeners, and it is hoped that there will be a listening qualification properly recognized—for it is an occupation requiring careful training, skill and intelligence.

Our efforts towards producing a standard of good listening were brought to the notice of Mr. Somerville Hastings, and his interest in the matter has resulted in our modifying our designs, so that, if satisfactory, they can be employed on the deaf.

The following considerations determined the form of this apparatus:—

- (1) The sound must be that of a pure musical note for whatever part of the scale the ear has to be tested.
- (2) It must be reproducible, as regards pitch and intensity, from day to day.
- (3) There must be a logical scale of intensity.
- (4) The signals conveyed to the ear must be intermittent at will, and some indication of the listener, other than oral, must be such that no sound interferes with the process of listening.
- (5) The sound should be produced so close to the ear that intensity of the sound cannot be affected by reflections within the room.
- (6) Outside disturbing sounds should be eliminated.
- (7) The process of getting the limit of acuity should be a rapid one, to prevent the subject from educating himself too well to the operation of listening, or from losing acuity through fatigue as the result of concentrated effort.

Since electrical methods are in general so well under control, an electrical method was selected, and the sound was generated by an oscillatory electric current actuating the diaphragm of a telephone.

Recent wireless research has presented us with an excellent generator of oscillatory electric current. The electric circuit employed is shown in the diagram exhibited.

The important constituent of this is the wireless valve, in which the space inside is rendered a conductor of electricity by the particles or electrons emitted from an incandescent filament. If a battery is used as a supply of current and connected to a metal plate and to the hot filament in the valve, an electric current will pass. If a metal wire screen be interposed between the filament and the plate, the current will stop, but if in some way a small alternating voltage be applied between the filament and this screen (called the grid)—the grid now allows the current between the filament and metal plate to pass with very much increased intensity. Here the grid and filament are connected by a coil of wire, and the plate and filament are connected by what is called a tuned circuit consisting of another coil with a condenser across its terminals. In this tuned circuit we get a great readiness for electric current to be generated at a given

frequency. A slight disturbance in this circuit reacts in the coil in the grid filament circuit, producing an alternating voltage between filament and grid, thus causing a large alternating flow between plate and filament, and since the tuned circuit above referred to responds easily to this, oscillatory current in the latter is thus built up to a high value, and can then be used to operate a telephone and produce a sound. For demonstration of this a loud speaking telephone can be used.

The tuned circuit already referred to can have its note altered by varying the condenser so that a range of notes can be obtained from 300 per second (G above middle C of the piano) to some very high pitch only just distinguishable. You will notice how pure the musical note is.

It would not be difficult to produce similar apparatus for getting the upper limit of audition, but in this case it is difficult to get a pure note over so high a range.

By altering the glow of the filament we can alter the loudness of the note, so that the instrument is well under control. But if we keep the filament at a steady glow, we do not get the same amount of oscillatory current for all pitches, as can be seen in the diagram. We can, however, level out this curve by altering the filament glow.

We thus have our oscillatory electric current. We now pass this current into a Wheatstone bridge circuit in order to reduce it to a value suitable to our requirements. The telephone used for the signal is the usual wireless head telephone, with two receivers, and one of these is disconnected, so that the sound only enters one ear, while the other is blocked up, and so is protected from disturbing noises. When the bridge is in balance, there is no current in the telephone and no sound. We alter the fourth arm of the bridge from its balance value of 1,000 ohms resistance to 10,000, and the more we throw it out of balance the louder is the sound. The diagram exhibited shows how the sound intensity varies with the bridge reading.

When the subject is being tested he sits down with closed eyes, and he places one hand on the table. Then the signal is put in the telephone by means of a silent key, and if the subject hears he raises his finger, lowering it when the sound ceases. We then reduce the sound till the finger just fails to respond to the signal, and the acuity value is read off on the scale.

The apparatus before you shows the telephone circuit and key used in the method thus described.

The apparatus however will do more than this. An arrangement is made by means of which a disturbing sound of any desired pitch or intensity is maintained in the signal telephone, and the signal is transmitted as before. A new reading is thus obtained. In this way paracusis is tested and compared between different individuals and for different ears.

With regard to the standardizing of the sound, an instrument is inserted in the current generator for measuring the current output, and a definite value for this current is laid down, so that the same sound can be reproduced day after day.

Further, if the telephone has to be replaced by another, a method has been devised for comparing it with the one previously used, and, in addition, apparatus has been devised and is now being perfected for measuring the sound in absolute units of energy, so that ultimately we hope to derive a measure of that energy which is just capable of affecting the ear of any subject—either deaf or of acute hearing—and of showing how this alters with age or with varying physical condition.¹

¹ The diagrams shown will be reproduced in a later communication.

(III) RESULTS OBTAINED BY STANDARD APPARATUS FOR TESTING HEARING.

By SOMERVILLE HASTINGS, M.S.

The results obtained by this apparatus have been extraordinarily constant.

Qualitatively they have agreed with the results obtained by other methods. I mean that if a person hears better with one ear tested by tuning fork, voice, or the acoumeter, he will hear better with the same ear tested by Major Tucker's apparatus. Quantitatively the tests differ according to the type of deafness, as we should expect.

We have made a good many observations on the effect on the same ear or opposite ear, of a loud disturbing sound. We have found that in all types of deafness examined, a disturbing sound in the opposite ear makes hardly any difference in the power of hearing in the one being tested. If, however, the disturbing sound is in the same ear, the intensity of the signal has to be increased some seventeen to twenty-four times, before it is appreciated in the case of a person with normal hearing. Cases of internal ear deafness and those with perforation of the membrana tympani need eight or nine times the usual stimulus.

In chronic catarrhal deafness we have found that most patients need an increase in the intensity of sound of about nine or ten times, before it is perceived, and one or two who had never noticed that they heard better in a noise needed an increase of about five times. In the case of patients with paracusis who were well aware they could hear better in a noise, only from 1.7 to 2.3 times the stimulus was required. We have never yet found an individual who could really hear "better in a noise."

DISCUSSION.

Dr. WILLIAM HILL considered that the instrument would be very valuable, and the exhibitors were to be congratulated on the clear exposition they had given. He supposed that in perfecting the instrument for the use of the profession it could be made fool-proof.

Dr. LOGAN TURNER said he understood that when the buzzer was placed against one ear, the hearing in the other ear was not diminished. When experimenting with Bárány's noise apparatus on a normally hearing individual, he had found the hearing in the non-obstructed ear slightly reduced from the normal. The apparatus now demonstrated possessed an advantage in this respect.

Mr. G. J. JENKINS also congratulated the exhibitors on this valuable piece of work; it had been the want of otologists for many years, and many had tried to establish some such standard. He had himself tried to do so by means of an induction coil, getting a variation of intensity of sound by separating the two coils; but he found he could not produce, by a mechanical apparatus, a make-and-break sufficiently rapid to give more than 4,000 to 5,000 vibrations per second. He therefore turned his attention to the valve referred to. A friend of his had been working at such apparatus in Edinburgh for some time, but had given it up a short while ago, after hearing that work on these lines was being done in America. There was no doubt that the present apparatus did give a new test for hearing, and it would be advisable that the Section should take an active interest in the matter, and help the workers to arrive at a complete standardization. He asked the probable cost of the apparatus. It would not take the place of tuning forks entirely.

Mr. SYDNEY SCOTT also expressed his interest in these investigations, which, taken into consideration with those carried out in America by Dr. Gordon Wilson and others, would yield important new data in elucidating problems of hearing.

Mr. SOMERVILLE HASTINGS (in reply) agreed that the action of the buzzer in one ear did not depreciate the hearing in the other ear. The padding of the ear-pieces prevented any but the slightest bone conduction. The estimations were in terms of energy. The normal was the average of a number of people with good acuity.

Major TUCKER (in reply) said that an instrument for medical testing could probably be produced for £15 or £20. Listeners when employed for anti-aircraft work had to come up to a certain standard which was based on an average of apparently normal hearers. The instrument had shown great diversity in hearing between people supposed to be normal. Subjects were also tested to see whether their ears were of equal value—a matter of great importance in anti-aircraft work.

A Temporal Bone from a Case of Tuberculous Lateral Sinus Thrombosis and Extra-cerebellar Abscess.

By E. D. D. DAVIS, F.R.C.S.

A BARMAN, aged 37, was admitted to the medical ward on February 9, 1922, with temperature 103° F., respiration 36, pulse 116. Consolidation of whole lower lobe of left lung. Rusty sputum pronounced, and repeated vomiting of three days' duration. Diagnosis: pneumonia. Temperature subsided by crisis on seventh day and chest signs disappeared. Patient intelligent and not drowsy. No history of a rigor, but complained of occipital headache. Sputum: No T.B. found. Patient sent to convalescent home, but returned a few days later, on March 23, complaining of headache and vomiting. Temperature 101.4° F.

March 31: First examination of ear and nose. Suppuration of right middle ear of long duration. Granulation tissue polyp occupying posterior superior wall. Neither mastoid tenderness nor œdema. Hearing: Voice, 2 ft.; bone conduction normal. Slight nystagmus on looking to right increased by irrigation with hot lotion. Pupils equal and reacting. Optic discs: Outline blurred; frontal headache; patient drowsy and slow, with occasional vomiting. Stiffness of neck. Deep reflexes increased. No paralysis. Mastoid suppuration, with possible posterior fossa abscess, diagnosed. Operation strongly advised, but refused. Cerebro-spinal fluid: Increase in cells 61 per cubic millimetre, chiefly lymphocytes. Albumin: 0.2 per cent. excess of globulin. Wassermann reaction negative.

Patient became unconscious and died suddenly on April 24.

Post-mortem.—Cortex and its meninges normal. An abscess containing about 2 drachms of pus was found on the surface of the right cerebellar hemisphere, forming a slight cavity within the hemisphere and surrounding the right lateral sinus. The dura, over an area corresponding to a five-shilling piece extending from the tentorium to the foramen magnum, was covered by pale granulation tissue and a number of small white tubercles could be seen on the inner surface of the dura. The temporal bone was extensively necrosed in front of the lateral sinus groove, and a sinus led outwards into an abscess in the mastoid process. The lateral sinus was replaced by granulation tissue and showed old thrombosis. The cerebellum was otherwise normal. No tubercles were seen in the sylvian fissures or on the cortex. Petrous bone: Chronic suppuration of right middle ear with polyp on posterior superior wall. Mastoid full of pale granulation tissue and pus. A large sinus, in lateral sinus groove, extending into the abscess in the posterior fossa. The outer mastoid plate of bone was perforated in the suprameatal triangle, but there was no subperiosteal abscess. The mastoid antrum was full of granulation tissue but

most of the bone disease surrounded the lateral sinus. Sections of the granulation tissue and dura were stated to be tuberculous. Lungs: Bronchopneumonia, old tuberculous foci and pleuritic adhesions. Nothing to suggest infarction such as is seen in lateral sinus thrombosis.

Mr. SYDNEY SCOTT said that he believed cases like Mr. Davis described must be rare. Hitherto he knew of only one recorded case of tuberculosis of the lateral sinus. This was published in the *Proceedings of the Royal Society of Medicine*, 1916, ix (Section of Otology), p. 84. He had shown the histological preparations demonstrating tuberculosis in the lateral sinus, the contents of which were removed when the child was about 5 years of age. She was now 18 years old, and her father, a well-known medical man, said she was in perfect health.

Case of Acute Suppuration in one Ear subjected to Early Operation on Account of Complete Deafness of opposite Ear.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

C., A MIDDLE-AGED man, with complete deafness in the left ear, radical mastoid operation and precedent facial paralysis, developed acute suppuration in his right middle ear, and for the moment was practically quite deaf. In view of the serious possibility of permanent total deafness if by mischance the right ear failed to recover, I considered it advisable to perform Schwartz's operation a little more than a week after the onset, in spite of the absence of "mastoid" signs, in order to leave nothing to chance. The wound was closed, excepting a small opening at the lower part through which a strip of the gauze plugging passed. When this was removed a small drainage tube was inserted. Very rapid subsidence of the discharge, and healing of the perforation with restoration of hearing followed.

I would suggest that complete deafness of the opposite ear is an indication for expedition in performing the simple mastoid operation, though, in general, a contra-indication for the radical operation.

DISCUSSION.

Dr. WILLIAM HILL said this case raised a very important point, namely, whether middle-ear suppuration could not have its course shortened by operation. He had never regretted operating on such cases, though he had had occasion to regret having put off operation. The Schwartz method was an operation there was no need to hesitate about doing; it was a simple and easy procedure. A year ago he had had a case in which the whole discharge ceased three days after Schwartz's operation had been done. At the same time he slit up the roof of the meatus, and there was no further suppuration, neither was there any subsequent swelling. Mr. Heath advised operating in these cases, and he called his particular operation a conservative one. His (Dr. Hill's) view, however, was that it was far from being conservative, for in many of the cases dealt with by Mr. Heath it was an unnecessarily destructive proceeding. There were no complications following Schwartz's operation, and it was well worth while to consider whether doing a Schwartz might not prevent the development of more serious symptoms. Possibly the operation had been too much neglected.

Mr. H. J. BANKS-DAVIS said that after a Schwartz operation there was the possibility of the post-aural mastoid wound not closing, with a resulting visible fistula. This could never occur if the post-aural wound was closed and meatal drainage was employed as in the "Heath operation," where any continuance of the aural discharge could be more easily dealt with by the patient than if the discharge exuded from behind

the ear. In women seeking employment this was often a serious disadvantage and a great disfigurement; closing these fistulae was not always an easy matter.

Dr. LOGAN TURNER said that the present day tendency was to operate earlier than formerly in acute middle-ear suppuration. He preferred the Schwartze operation to the so-called Heath method as he believed that better drainage was obtained by Schwartze's operation.

Sir JAMES DUNDAS-GRANT (in reply) said that, as this was the man's only effective ear, he worked for safety, rather than risk the possibility of total deafness. If the other ear had been fairly good, he would have left it. In answer to Mr. Banks-Davis as to the risk of a fistula persisting behind the ear, when that occurred, it was an indication that the operation was all the more called for. If the disease settled down, the fistula could be remedied by a plastic operation.

Case of Absolute Bilateral Deafness, with almost Complete Loss of Vestibular Activity.

By ARCHER RYLAND, F.R.C.S.Ed.

G. C., MALE, aged 30. First seen October 10, 1922. He joined the Army in 1914, and deafness was not noted at that time. The deafness has come on since that date and is due to congenital syphilis. The loss of hearing is complete, and, so far as investigated up to the present, the loss of vestibular activity is also apparently complete except for a very slight response on the part of the left labyrinth.

The following points may be noted :

- (1) The voice is quite uncontrolled. The cochleo-palpebral, and Lombard voice-raising tests are negative.
- (2) Wassermann: Strong positive, + +.
- (3) Eyes: Corneal nebulae; pupils unequal; left iritic margin irregular; "patches of retinitis at periphery" have been reported by oculist.
- (4) Teeth: Upper incisors Hutchinsonian in character.

DISCUSSION.

Sir JAMES DUNDAS-GRANT said he had seen slight degrees of improvement taking place under increasing doses of arsenic combined with mercury—Donovan's solution in fact—even in congenital or hereditary specific disease, though there was not much restoration of hearing. The general condition, in regard to giddiness, &c., had been improved.

Mr. ARCHER RYLAND (in reply) said that the investigation of the case was at present incomplete as he had not had an opportunity of fully recording the nature of the vestibular reactions. There was no mistaking the nature of the congenital defects, and the Wassermann test was strongly positive. The point of interest of course was the manner of invasion of the labyrinth and internal ear. The tympanic membranes on each side were scarred, and this pointed to an obsolete suppurative middle-ear condition. This was probably an illustration of a type of case, held by J. S. Fraser and others to be not infrequent in occurrence, in which the original middle-ear infection was syphilitic (though, later, probably polymicrobial), slowly invading the petrous bone and attacking the labyrinthine capsule, giving rise to a chronic form of osteomyelitis which slowly invaded the perilymph space of the labyrinth. There had been no history of sudden attack on the labyrinth, but of a gradual progress to absolute deafness and loss of vestibular function.

Tinnitus associated with Facial Spasm.

By G. J. JENKINS, F.R.C.S.

PATIENT, a female, aged 53. Spasm of left facial nerve, one year and five months. Tinnitus began with onset of the spasm. A single twitch of the muscles was associated with a synchronous noise in the left ear. Patient described the noise as a "bang." When twitches followed one another rapidly the noise was like the "popping of a motor car." When twitches were very rapid the noise became continuous and sometimes bell-like. There was pain in the left ear at the onset of the disease, when the twitching was very bad. Post-suppurative effects in the right ear. Tympanic membrane on left side: opacities. A detailed examination is being made.

DISCUSSION.

Sir JAMES DUNDAS-GRANT said he thought the murmur was perhaps a muscular one caused by the contraction of the stapedius, associated with contraction of the facial muscles. If the normal subject closed the ears and then shut the eyes tightly, a deep-toned hum was perceptible. Mr. Jenkins' patient said that the sound she was hearing was the same as that which followed the energetic shutting of the eyes in the way mentioned.

Mr. SYDNEY SCOTT said he had seen some similar cases at Queen Square, but was unable to throw light on the pathology. He believed the subjective noise might be caused by the repeated contractions of the stapedius, because in some cases the patient felt momentarily giddy during the attacks of twitching of the face. This patient said she was not giddy, but she was deaf in the opposite ear.

Mr. G. J. JENKINS (in reply) said he had shown this case because it seemed to be one in which a specific cause for the tinnitus could be made out. There was a spasm of the left seventh cranial nerve. The tinnitus might possibly be due to the sound produced by the contraction of the facial muscles being conducted to the ear, or by a movement of the pinna, but he thought it was due to the movement of the stapes produced by the contraction of the stapedius muscle. As to whether the tinnitus was due to the simple movement of the stapes or to a vibration set up in the tympanic membrane and ossicles, it was difficult to say, but he thought it was more likely to be due to movement of the stapes itself.

Section of Otology.

President—Mr. HUNTER TOD, F.R.C.S.

Chairman—Sir CHARLES BALLANCE, K.C.M.G., C.B., M.V.O., M.S.

Some Cases of Otosclerosis with an Unusual Symptom (Otosclerosis Paradoxa).¹

By ALBERT A. GRAY, M.D.

(ABSTRACT.)

DR. ALBERT A. GRAY read a paper giving the history of four cases of otosclerosis in which the patients heard better during a severe cold or if, when in a state of exhaustion, they had received some unusual stimulus.

Case I.—Patient, a woman aged 44, had begun to grow deaf at the age of 30; a year or two later she was troubled with noises in the ears and had noticeable paracusis. Inflation with the catheter produced little or no improvement in either ear. When she had a cold, however, she could hear a conversation carried on in an ordinary voice.

Case II.—Patient, a woman aged 41, had begun to grow deaf at the age of 25. Paracusis willisii was present and there was definite otosclerosis. Every year she had hay fever during which her hearing improved very much, declining again as the fever subsided. In this case an ordinary cold in the head was not accompanied by an equal improvement in hearing.

In the next two cases the improvement in hearing was only momentary; in both these cases also the patients had otosclerosis.

Case III.—Patient, a woman. Tinnitus was present in a moderate degree and there was paracusis willisii. The patient had suffered from deafness for many years. One evening in an overheated room she opened a window and stood in a cooling draught. Her hearing at once became so much better that she heard the conversation going on in the room, though previously she had heard practically nothing of it. The improvement lasted for two minutes and ended suddenly. It was the patient's only experience of the kind.

Case IV.—Patient, a woman aged 34, had fleeting tinnitus and paracusis willisii was present in a striking degree. Both tympanic membranes were normal. There was no family history of deafness. In this case deafness had begun at the age of 22 and had been gradual in onset. The patient was a total

¹ This paper is published in full in the *Journal of Laryngology and Otology*, March, 1923.

abstainer. After a drive in a motor-car during very warm weather she had become on one occasion exhausted and somewhat faint. She was persuaded to take a dessertspoonful of brandy and her hearing immediately improved so much that she could hear conversation carried on in an ordinary tone. The improvement in this case also lasted only two or three minutes. Subsequent taking of brandy, when the patient was not exhausted, did not produce any improvement in hearing.

Dr. Gray said that otologists were not yet agreed as to the causation of paracusis. It had been attributed to movements of the stapes and other ossicles which enabled sounds of smaller vibration to reach the labyrinth, but he (Dr. Gray) felt sceptical about this explanation, because in the cases he had described the improvement had occurred when there had not been anything to cause movements of the stapes and other ossicles. In the last two cases there was an unusual stimulus, during a period of exhaustion—in one case, the cool draught, in the other, the alcohol—which might have caused a sudden change in the circulation. Dr. Gray named the condition "otosclerosis paradoxa."

DISCUSSION.

Mr. J. F. O'MALLEY said that apparently Dr. Gray was not convinced as to the accuracy of the generally accepted explanation of paracusis; he (Mr. O'Malley) with many more, had accepted it as true. It was difficult to find evidence that the stapes was the responsible factor. The idea had occurred to him that the condition of the perilymph might be a factor of importance, and he would like to hear whether Dr. Gray considered that an alteration in the perilymph—especially in its quantity—was a probable factor in the causation of paracusis. He had read, in an old book, reports of examinations in which the labyrinths were devoid of fluid after death. There were certain peculiarities associated with some of these cases which would lead to the belief that there were changes in the labyrinthine fluids, and these changes must have a considerable influence on the hearing.

Mr. RICHARD LAKE said he certainly remembered one case in which the patient heard much better during a cold, but he did not remember particulars of it. Possibly the improvement might be of toxic origin, i.e., the stimulus to better hearing might be a toxin evolved during the catarrh. He had still under cognizance a deaf mute, who could hear very slightly. A few years ago she had post-influenzal pneumonia, and during the time she was practically *in extremis* in that illness, she heard better than she had ever done before. The suggestion he made to her father at that time was, that the improvement must have been due to the toxæmia from which she was suffering. Children in a toxæmic condition, if they were not really stupid, had a very acute cerebration during the illness. Dr. Gray's patients might have had a toxæmia at the time, though of mild form, and the stimulus might have acted not only on the nerve endings, but also on the central nervous system.

Mr. M. VLASTO said he thought the most scientific attitude would be to accept the facts communicated to Dr. Gray by his patients as correct. It would be serviceable if members searched their memories and records for cases of otosclerosis in which the deafness was improved under varying conditions. He asked if Dr. Gray had heard of a case of otitis media sicca in which the hearing was improved during an attack of coryza.

Sir JAMES DUNDAS-GRANT said he recalled, in this connexion, the observations made in Paris by Sir Robert Woods, namely, that during or after acute inflammation the deafness in otosclerosis sometimes seemed rather less marked. Sir Robert Woods had devised a method of injecting some irritant fluid through the tympanic membrane. One case had been described in which the result seemed to have been promising.

This indicated that the increased vascularity, even if only temporary, was the agent in the momentary improvement. Patients had told him (Sir James Dundas-Grant) that they heard better when they had a cold; he had not paid sufficient attention to it, but he would attach significance to such statements in future. Dr. Gray's patients seemed to have heard a conversational voice much better than they heard a whisper; but in otosclerosis the patient not infrequently heard the whisper better, a fact that was becoming recognized as one of the signs of otosclerosis. Perhaps he, himself, had used a more penetrating whisper than Dr. Gray did in the test, giving the high-pitched harmonics of the voice more prominence. With regard to paracusis, he asked whether Mr. Cleminson had pursued the observations with the apparatus demonstrated at the last meeting by Mr. Somerville Hastings, which seemed to prove the reality of paracusis more certainly than anything he had encountered. Experiments made in other ways had been open to doubt as to whether the patient actually had better hearing, or whether there was unconscious raising of the speaker's voice caused by the noise which was heard by the speaker.

Mr. F. J. CLEMINSON said Mr. Somerville Hastings had scarcely been available since the last meeting. He (Mr. Cleminson) saw a patient with otosclerosis, and, remembering the beneficial effect of nitrate of amyl, he tested her with the watch, and found she could hear with the right ear 3 in., with the left, 2 in. Then he applied a Siegle's speculum and produced a negative pressure until he could see not only the vessels along the handle of the malleus, but also the radial vessels from the malleus handle to the periphery of the drum. At this stage he found she could hear the watch with the right ear, 10 in., and with the left, 3 in. He continued to test her every minute afterwards, and found the duration of the improvement was three to five minutes, after which she relapsed into the state in which she was before the experiment began. That seemed to support the view that the improvement was due to a vascular change.

Mr. W. M. MOLLISON said he did not doubt that an increased peripheral vascularity had an effect on the hearing. Women often noticed that deafness became worse after the birth of each child, but just before parturition they heard better; at such times the circulation was enormously enhanced, and was consequently more vigorous in the ear. The fact that patients heard better when they had a cold might also be due to enhanced peripheral circulation. Otosclerotics also improved when given pilocarpine.

Mr. G. J. JENKINS said the problem introduced by Dr. Gray was a very far-reaching one, too large to deal with adequately in a few minutes. At the International Congress in 1913 he (Mr. Jenkins) tried to show that probably some of the symptoms of otosclerosis were due to changes in the fluid, and he used a hot-water douche for the ears of patients with otosclerosis, beginning with water of body temperature, and going up to quite a high temperature. For a short time after this hot douche patients heard better. The effect of chloroform on the hearing of patients suffering from otosclerosis should be remembered, as individuals with normal hearing had increased acuity of hearing with certain doses of chloroform. He knew of a doctor suffering from otosclerosis whose habit was to take chloroform before starting his round, although the effect only lasted about an hour at a time. Paracusis patients also heard better during yawning, possibly owing to the effect on the labyrinthine fluid.

Mr. H. TILLEY reminded members that the stimulation produced by Sir Robert Woods was induced by the injection of an iodine preparation into the middle ear.

Dr. GRAY (in reply) agreed that, in its broad application, the subject was a large one; but the purpose of the present paper was to secure interest in this particular phase: the better hearing in otosclerosis when there was present an abnormal condition, such as a cold. It had always been the custom to think of otosclerosis as being due specially to fixation of the stapes, and he regarded that as a fundamental error, as the fixation of the stapes was only one manifestation of the disease. The change in the bony capsule of the labyrinth was an atrophic change. He believed otosclerosis to be a disease of the whole organ of hearing, from the auricle to the cortex. In answer to Mr. O'Malley as

to the perilymph, he (Dr. Gray) did not know whether there was more or less perilymph than normal in otosclerosis. With regard to Mr. Lake's point concerning toxæmia, that was a very interesting matter, because it was true that many toxins did produce an effect on the vasomotor system ; the shivering and heat at the beginning of scarlet fever or measles was due to the effect of the toxin on the vasomotor system. The same explanation might be true in the case of pilocarpine, as that drug also produced a marked effect on the vasomotor system. He had never known this condition occur in chronic otitis media, it occurred only in otosclerosis. He remembered Sir Robert Woods' contribution in Paris : he injected iodine into the middle ear in chronic otitis media. Otosclerosis might have been present too. He (Dr. Gray) thought the effect was more likely to be due to stimulation of the mucous membrane of the tympanum, and there was probably a reflex dilatation of the blood-vessels in the labyrinth. With regard to the suction of the tympanic membrane, mentioned by Mr. Cleminson, in that case the effect might have been due to dilatation of the blood-vessels in the labyrinth ; but when the stapes was fixed, it was difficult to see how that could occur. Unless the stapes were movable, any suction on the tympanic membrane would be compensated for by air coming up through the Eustachian tube. If, however, there was some obstruction in the Eustachian tube it might occur. Another explanation was that there might be stimulation of the nerve endings in the tympanic membrane, with coincident dilatation of blood-vessels in the labyrinth. The same explanation would apply to Mr. Jenkins' warm douche. The important point was, that one could not look upon the deafness of otosclerosis as being due only to the mechanical fixation of the stapes. A fixed stapes was a constant factor, and if the deafness was due to that, nothing in the way of dilatation of the vessels would produce any effect. It was much more likely that in these cases there was a reflex dilatation of the blood-vessels when nasal catarrh was present. He (Dr. Gray) thought the statements made by the patients in these cases were obviously true ; in the case he described first in the series he knew it was true ; it happened in a distant relative, and he had been present when she had a cold : there was no doubt about the striking difference in the hearing during the cold.

" The Disease of not Listening, the Malady of not Marking." ¹

(HENRY IV, Pt. ii, Act 1, Scene 2.)

By T. B. LAYTON, D.S.O., M.S.

(ABSTRACT.)

MR. LAYTON said that between the malingerer and the case of functional deafness there must be at least two other stages. One was the stage of "the subconscious malingerer" ; this was the person who did not intentionally try to cheat but who gave the examiner no assistance in his examination ; the other was that of the person who had lost all interest in his surroundings and seemed never to take any notice of anything until it was forced upon him. The difference between these types might be explained by a study of the subject of attention. Ribot's classification of spontaneous or natural attention and voluntary or artificial attention, was a useful standpoint from which to study the cases. Mr. Layton believed that in functional deafness auditory attention, both spontaneous and voluntary, was entirely in abeyance ; that in the people who took no notice of anything (as exemplified by cases of severe shell-shock) all forms of voluntary attention were largely in abeyance, and of spontaneous

¹ This paper is published in full in the *Journal of Laryngology and Otology*, March, 1923.

attention partly so; that in the subconscious malingerer the voluntary attention was withdrawn but was not in opposition to the examiner, while the spontaneous attention remained normal, whereas in the true malingerer the voluntary attention was very much in action but was in opposition to the examiner instead of being in sympathy with him.

Mr. Layton said that the diagnosis of the malingerer should be made in three stages: that of observing unusual behaviour; that in which the examiner determined that the unusual behaviour was due to malingering; and that in which the examiner proved the examinee to be cheating intentionally. He thought that in civil practice some lessons might be learned from the various types of cases seen during work for the Ministry of Pensions.

First: That in the cases of severe deafness when nothing could be done for the deafness, much could be done for the patient if he was beginning to allow his attention to fail. Secondly: In considering the deafness of school children and realizing the extreme difficulty of testing hearing in children, it should be realized that it might be not that the child was deaf but that the school teacher was unable to hold his attention; and thirdly, that in some children it might be reasonable to advise special private tuition if it were proved that their hearing was normal but that they were not properly developing their powers of voluntary attention.

DISCUSSION.

Dr. A. R. FRIEL said he thought that in examining pensioners it was important to see whether the appearances in the drum corresponded with the results of the functional tests. If the appearances in the drum corresponded to the functional tests, it tended to show that the responses of the patient were correct.

Mr. M. VLASTO remarked on the omission from Mr. Layton's paper of a reference to day-dreaming. This occupied a place of much importance in the child's life, and during its sway the young person was oblivious of all that was going on around. In the adenoid child one of the distant receptors—audition—was partly in abeyance, and a vicious circle was thus set up.

Mr. G. J. JENKINS said that all individuals were non-listeners. Non-listening was a physiological process, and the Rinne and Weber tests depended upon this fact to some extent. A girl had come to King's College Hospital the previous day with complete deafness in one ear. She had to sleep in a house in which there were many rats, and next morning she was unable to hear at all with one ear. On Weber's test the sound went to the good ear; and in that ear the hearing was normal. There was no sign of disease, and the caloric test showed that she had normal vestibular reaction of the labyrinth of the deaf ear. He (Mr. Jenkins) asked her to come into the hospital and have it put right next day, and this led to hesitation, and questions as to whether she would be operated on, or hurt. There was something more than pure hysteria in the case, which was to be further investigated. But the point on which he (Mr. Jenkins) insisted was that we were all non-listeners; all were "not listening" at every moment of the day, and it was an exaggeration or aberration of this normal physiological state which in many cases was responsible for functional deafness.

Sir JAMES DUNDAS-GRANT said he had frequently noticed in dealing with deaf soldiers, that sometimes there was a very high degree of deafness accompanying a slight, removable condition, and when that condition was removed, the improvement in hearing was out of proportion to that which one was accustomed to see in civil practice. In some cases one successful Eustachian catheterization altered the aspect of the patient altogether; he brightened up at once, and took a totally different view of life. Similarly tightening up a relaxed membrane by means of collodion would have the same

effect, as also the removal of nasal obstruction. Obviously, therefore, the deafness in such cases was not solely due to organic changes, the psychical factor playing a large part. With regard to caloric tests, he agreed with Dr. Hurst, that if there was absence of vestibular reflex, the deafness should be put down as of organic origin. The converse, however, was not always true: although the vestibular reflex might be present, the deafness might be organic. He thought there was a greater degree of vulnerability of the acoustic part of the nerve than of the vestibular.

Dr. A. A. GRAY said he believed that Falstaff's words were: "This apoplexy, I take it, is a kind of deafness," which expressed the idea that there was an association between apoplexy and deafness.

THE following Cases and Specimens were shown:—

- (1) Case of complete Nerve-deafness due to Syphilis of Internal Ears.
By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.
- (2) Case of Otitis Media with Facial Palsy, following Scarlet Fever.
By F. J. CLEMINSON, M.Ch.
- (3) Specimen: Malleus and Incus removed from the Left Ear of a Child
with Chronic Otitis Media. By F. J. CLEMINSON, M.Ch.
- (4) Specimen: A Wart Horn removed from the Right Ear of a Man
aged 63. By F. J. CLEMINSON, M.Ch.

The cases will be published with the report of the next meeting (January 19) to which the discussion was deferred.

Section of Otology.

Acting President—Sir CHARLES BALLANCE, K.C.M.G., C.B., M.V.O.,
M.S.

Labyrinthitis as a Complication of Middle-ear Suppuration.

By A. LOGAN TURNER, M.D., and J. S. FRASER, M.B.

(ABSTRACT.)

THE Report, which will be published *in extenso* in the *Journal of Laryngology and Otology*, was based upon the authors' observations in the Ear and Throat Department of the Royal Infirmary, Edinburgh, and covered a period of fifteen years, 1907-21. In 10,653 suppurating ears, an affection of the labyrinth was diagnosed in 124, i.e., in 1.1 per cent. The types of labyrinthitis met with were: Circumscribed labyrinthitis, 27; acute serous labyrinthitis, 3; acute purulent (manifest) labyrinthitis, 23; chronic purulent (latent) labyrinthitis, 54; and the healed functionless labyrinth, 17. Intracranial complications occurred in 45 of the cases with inner ear disease: of these, 24 were regarded as directly traceable to the labyrinthitis, viz., 20 cases of lepto-meningitis and 4 of cerebellar abscess. Of 1,720 mastoid operations performed during the period under review, post-operative labyrinthitis supervened in 6, i.e., 0.3 per cent.

A number of lantern slides showing the pathological changes met with in the labyrinth in the different types of labyrinthitis were shown by Mr. J. S. Fraser.

DISCUSSION.

Dr. ALBERT GRAY expressed his high appreciation of the beauty of Mr. Fraser's prepared specimens, which surpassed that of any previous collection exhibited by Mr. Fraser before. He did not quite agree with one remark made by Mr. Fraser when he said that the change in the bone in one case was like that of otosclerosis. Certainly it was like that condition from the fact that the bone was less dense, but it had none of the sharp line of demarcation that typical cases of otosclerosis showed, and the bone did not stain so deeply as in typical otosclerosis.

Mr. SYDNEY SCOTT commented on the excellent organization which existed at Edinburgh for getting cases worked out and all the details recorded and classified. He said it had been interesting to see the various stages resulting from labyrinthitis, from the localized infection with a small erosion, to those in which there were granulations, and finally where the labyrinth had been destroyed and in the end obliterated by the formation of new bone. He had seen only one case in which the whole labyrinth was replaced by bone tissue, but in that case no history could be obtained; it was an ordinary dissecting room specimen, the patient having come from a Poor-law infirmary. He took it that such a condition was the final result of infection. He was not favourably inclined to the use of the word "serous" as applied to labyrinthitis. He presumed the term was applied to cases in which the clinical manifestations were mild or perhaps corresponded to a localized infection. There was a type of case he had never seen recorded in which an ordinary radical operation on a patient was performed for, say, cholesteatoma or granulations in the middle ear without signs of internal ear disease. No signs of erosion of the labyrinth would be found at the operation, and no untoward signs would arise until three or four weeks afterwards, when, on a spot on the inner wall of the cavity being touched, violent vertigo, forced movements of the head and limbs and nystagmus occurred, i.e., the fistula symptom had appeared about the fourth week after the radical operation. The semicircular canal had not been

opened at the operation; probably the outer bony wall of the external canal was very thin and the granulations formed during reparation from the Haversian canals caused rarefaction of bone which, yielding to direct pressure with the probe, transmitted changes of pressure to the labyrinthine fluid. He had seen this in five cases. He had not been disposed to open the labyrinth because he had seen that sign, and after a variable period, sometimes as long as three months, the fistula sign had disappeared in all these cases. He had been interested to note the number of cases at Edinburgh, where meningitis due to labyrinthitis had been treated by the operation of translabyrinthine drainage. Probably Dr. Turner's and Mr. Fraser's experience was the same as that of other members, that these cases were generally received too late. It could not be too well known that this form of meningitis could only be dealt with hopefully if the condition was recognized early, especially by general practitioners and physicians, who were often consulted first.

Mr. G. J. JENKINS said that the communication made was excellent. All recognized the advantage of the Edinburgh methods in being able to follow out the cases, and the organization of the clerical part of it which was practised. He agreed with what Mr. Scott said about the terms "serous" and "exudative"; at the International Congress he (Mr. Jenkins) made a point of that in dealing with meningitis. A more accurate term should be employed. These were inflammations at different stages, and the matter could be left at that. With regard to cases with perforation of the external semicircular canal coming late, at the present time he had two cases under observation. One of these patients was now almost well, and the other was on the road to recovery.

Dr. LOGAN TURNER (in reply) said that team work was what was needed everywhere, and he was always glad to feel that they in the North had been able to do something in that way. He had often wished there was more of it, so that the statistics of one clinic could be combined with those of others, and really valuable material thus obtained. With regard to the use of the word "serous," they had limited the application of it to that type of labyrinth affection in which there was still some function. Those cases in which no labyrinth response was obtained on testing they regarded as purulent.

Mr. J. S. FRASER (in reply) said that he did not think this was the occasion for discussing with Dr. Gray the question of otosclerosis; they had exchanged views many times before. He (Mr. Fraser) contended that it was an inflammatory disease. Dr. Gray said it was not. He (Mr. Fraser) did contend that the changes shown that day, in the labyrinth capsule, were an early stage of otosclerosis. With regard to organization, they in Edinburgh owed much to the secretaries who worked with the surgeons and physicians at the ear and throat department. On arriving in the morning they found that all the case-histories had been taken very well. (Dr. Turner's secretary had been working at the department for twenty years, and his own about eight years.) Therefore when he (Mr. Fraser) arrived, all he had to do was to examine the nose, ear or throat, and dictate to his secretary the conditions found there. In the same way after an operation the findings were dictated and entered in the case sheet. Both Dr. Turner and he were greatly indebted to the work of Mr. West and Mr. Sydney Scott on translabyrinthine drainage. Several of their patients would have died if this procedure had not been carried out.

Case of Complete Nerve-deafness due to Syphilis of Internal Ears; Caloric and Rotation Tests Negative, Galvanic Positive.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

[Shown at the last meeting, December 15, 1922.]

PATIENT, female, aged 21. Deaf eleven years. Stigmata pronounced. Tests indicate lesion of labyrinth without involvement of nerve-trunk.

DISCUSSION. (January 19, 1923.)

Sir JAMES DUNDAS-GRANT said the indication was that the nerve endings in the labyrinth were destroyed by the disease, but the nerve itself was sound, and responded to galvanism.

Mr. J. F. O'MALLEY said that the case was very interesting considered from the point of view of the development of the vestibular and cochlear nerves, because they developed in the same way as a posterior root nerve, so when there was disease confined to the labyrinth, the cochlear ganglion was likely to suffer total extinction, whilst the vestibular ganglion, which lay in the internal auditory meatus, might escape, although its terminations in the labyrinth were destroyed.

Case of Otitis Media with Facial Palsy, following Scarlet Fever ; Specimens (Malleus and Incus) shown.

By F. J. CLEMINSON, F.R.C.S.

[Shown at the last meeting, December 15, 1922.]

S. A., AGED 2 years and 10 months, was brought up to the Evelina Hospital on September 8, 1922, with a discharge from both ears. There was pus in the left meatus and an incomplete right facial palsy. The history given by the mother was as follows : Up to April 25, of this year, the child was well and had normal hearing so far as the parents could say. On that day she was admitted to an isolation hospital for scarlet fever. While there she developed "measles and diphtheria of the nose." She was discharged one week before her first visit to the Evelina Hospital. The facial weakness was said to be improving.

The patient was sent to have her right ear syringed by a nurse, who brought back a perfectly macerated malleus and incus, without any sign of caries. The nurse had seen the ossicles evacuated with the lotion, and had rescued them from it.

The patient is apparently totally deaf, even the loudest sounds failing to attract her attention. While under observation the condition of the lower face (especially that of the muscles which retract the angle of the mouth) has improved very noticeably.

[The ossicles were shown as a separate exhibit.]

DISCUSSION. (January 19, 1923.)

Mr. CLEMINSON said he showed the case because it was known that rapid destruction of bone might take place in cases of scarlet fever, but he had never before seen a case in which the ligaments dissolved without affecting the integrity of the bones. As already stated, the child was aged 2 years 2 months when admitted to the Fever Hospital, and she could then hear and talk, but was now a deaf-mute, and had not said a single word during her stay in the Evelina Hospital. After she had been shown at the November meeting she was found to be carrying the infection of scarlet fever, and the opposite ear, being in a state of chronic purulent inflammation, had therefore been subjected to a radical mastoid operation, and had become dry. Since then there had been no further suspicion of "carrying." The facial palsy was still showing a slow improvement.

Mr. J. S. FRASER asked what was the opinion of members as to paralysis of the face being an indication for operation in acute otitis media. In books one always read that it constituted a reason for the performance of a Schwartz operation. Did members uphold that teaching, or did they believe that if there was no other indication for operation, such as pain, temperature or swelling, the mastoid should be left unopened, it being trusted that the facial paralysis would clear up?

*

Mr. H. J. BANKS-DAVIS said he did not think the facial paralysis did clear up easily in those cases. He was impressed by two cases in particular. One was that of a nurse, who had had acute otitis media, with nothing in the way of symptoms except a slight pain in the ear. Her face was paralysed, the paralysis having come on within three days. He thought it better to do nothing, but the facial paralysis became rapidly worse, and reaction of degeneration was distinctly present. He then opened the mastoid antrum, and the condition cleared up, but only very slowly. Facial paralysis was liable to continue for a long time, and he thought the mastoid should be opened, as there might be something pressing on the nerve. When he saw aural cases with facial paralysis he admitted them as urgent cases, and opened the mastoid without delay. The other woman to whom he referred had had her facial paralysis two years before recovery under electrical treatment; yet both of these cases were apparently identical.

Mr. W. M. MOLLISON said he remembered two particular cases of acute otitis media with facial paralysis, in which free incision of the membrane cured the paralysis in a few days: one, a child, in three days, one, an adult, in ten days. He did not regard facial paralysis in otitis media as an indication to open the mastoid. If the acute otitis media had been present some days and the paralysis then developed, the case would be different. In the two cases he had mentioned the facial paralysis developed almost simultaneously with the otitis. He believed increased pressure of fluid in the tympanum was communicated to the nerve through one of the dehiscences in the Fallopian canal.

Mr. SYDNEY SCOTT (in reply to Mr. Fraser's question) said that, in principle, he would be more inclined to open the mastoid if facial paralysis set in, in association with otitis media. The class of case being discussed was that in which acute otitis media developed, and the patient sooner or later had facial paralysis. He regarded that as an indication for extra free drainage.

Dr. LOGAN TURNER said his experience agreed with that of Mr. Mollison. Facial paralysis would clear up without operation. Two cases were recorded in the *Journal of Laryngology* last year in which the paralysis disappeared without interference.

Mr. G. J. JENKINS: Is not the one in a hundred the important one, rather than the majority?

Sir JAMES DUNDAS-GRANT doubted that in every case of Bell's paralysis the condition was due to inflammation in the middle ear. In Mr. Mollison's case the tympanum was not yet open, but he presumed there were all the other signs of acute suppuration of the middle ear: bulging and redness, and in the presence of these he agreed with Mr. Mollison's line of action, namely, to open the tympanum freely, and if the paralysis did not then speedily subside, to do a Schwartze operation.

Mr. CLEMINSON (in reply) read extracts from the notes taken at the fever hospital, which he did not possess when the child was shown; these showed that the facial palsy was first noticed six weeks after the onset of the otorrhœa.

Case of Vertigo, with Fixation of the Ossicles, cured by Ossicectomy.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

PATIENT, a male, aged 24, suffered from frequent attacks of giddiness so severe as to make him to fall down. He had chronic suppuration in the left ear, which was satisfactorily dealt with by a modified mastoid operation, but the vertiginous attacks persisted and it was found that they were excited by the slightest pressure on the *right* tragus. The right malleus was found to be

absolutely tied down to the promontory by cicatricial adhesions. After the removal of the ossicles the vertigo disappeared almost entirely and it could no longer be induced by pressure on the tragus.

The explanation of the relief is probably that the malleus fixation rendered the incus and stapes immobile and that the pressure of air when the tragus was pushed in acted upon the membrane of the round window with an abnormal degree of disturbance of the internal ear owing to the loss of the safety-valve action of the stapes in the fenestra ovalis.

The hearing power for the whisper on the right side has improved from 5 ft. (before) up to 20 ft. (after the operation).

Parotid Fistula following Mastoid Operations.

By NORMAN PATTERSON, F.R.C.S.

PATIENT, a girl, aged 12. History of right otorrhœa on and off since infancy. Seen at London Hospital, September 19, 1921. History of severe pain. Purulent discharge present, right ear; redness and bulging posterior meatal wall; posterior perforation; tenderness over mastoid. Temperature 100.5° F., pulse 120, respiration 24. Operation same day. Complete mastoidectomy, bone cellular, cholesteatoma in middle ear, lateral sinus exposed and appeared healthy. September 21: Temperature 103° F., pulse 120. Wound opened up. Decomposing blood clot and sloughs removed. Lateral sinus examined but looked healthy. On November 26 the temperature, which had fallen to normal, rose to 102° F. Anæsthetic given. More sloughs removed. The lateral sinus was found to contain a parietal clot. The operation was completed by ligaturing the internal jugular vein. Examination of cerebro-spinal fluid obtained by lumbar puncture negative. Blood culture grew a hæmolytic streptococcus. For a week temperature swinging, up to 104° F. After this temperature slowly came down to normal, and patient was sent to convalescent home on November 28.

On February 1, 1922, swelling noticed in operation scar. This swelling burst; the discharge was at first blood-stained, then clear. The discharge persisted. It was not at this time realized that it came from the parotid.

May 22, 1922: Wound opened up and curetted. It was discovered that the sinus did not lead into the mastoid cavity.

Present condition: Middle ear healed. Depressed scar over mastoid with very small opening from which saliva persistently drains. The flow is increased by getting the patient to chew something, such as a sweet. The fluid has been examined by Dr. Panton, who reports that diastase is present in large amount.

The patient will shortly be readmitted, and it is proposed to cauterize the fistula.

DISCUSSION.

Mr. H. J. BANKS-DAVIS said that four years ago¹ he showed a man, aged 50, with parotid fistula, who came to the casualty department with "carache," and developed a small abscess at the tip of the mastoid. It was opened by the house surgeon and a clear salivary discharge developed from the minute scar in the neck. Healing was very slow. He tried cauterizing it, and eventually it was cured by ionization. Cauterizing might make the sinus larger, and he would suggest ionization with a metal probe in the sinus first.

¹ *Proceedings*, 1917-18, xi (Sect. Otol.), p. 55.

Mr. NORMAN PATTERSON (in reply) said that he brought the case as a curiosity, and in order to elicit suggestions as to treatment. He had intended to cauterize the fistula, but he delayed that procedure until he had shown the case to the Section. If cauterization did not prove successful, he would try ionization, and report the result.

Ossification of Incus to Tegmen.

By SYDNEY SCOTT, M.S.

THIS specimen is from a case of radical mastoid operation for cholesteatoma; bony ankylosis of the body of the incus to the tegmen was discovered.

In the case of cholesteatoma there is usually no trace of the incus. In the specimen shown the incus was perfect except where the body and posterior limb were fixed to the tegmen by a mass of new bone. The patient was very deaf, as one would expect with this bony ankylosis.

Case of Acquired Atresia of the Auditory Meatus.

By E. LOWRY, M.B.

MRS. C., aged 32, has been complaining of deafness in the left ear for the last two years.

History: Three and a half years ago she had a lower left molar extracted, after which she first noticed discharge from the left ear which lasted for four weeks. There is no history of any previous illness.

Present condition: Right ear normal. Left ear completely deaf as regards air conduction. Rinne negative. The meatus is completely closed by a fibrous membrane close to the surface. This case is shown in order to obtain advice as to operative treatment.

Case of Vertigo (simulating "Ménière's Disease") with Anomalous Nystagmus Reactions.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

K. A., FEMALE, aged 35, first seen by the exhibitor in November, 1922, complained of frequent attacks of vertigo with vomiting, the first attack having come on suddenly about a year previously. There was deafness of the right ear, the bone conduction was diminished and the hearing for Galton's whistle did not extend above the mark 3'4. There was spontaneous nystagmus which, instead of being to the side opposite to the apparent lesion, was towards the same side and there was marked falling to the opposite side. A simple labyrinthine lesion would not produce this. The caloric (cold air) and rotation tests produced normal nystagmus, but to a lesser degree on the right than the left side; there was complete absence of past-pointing to either side and the falling was invariably towards the sound side, whichever ear was acted upon. Wassermann reaction negative. Perhaps a neurological examination will suggest a lesion in the superior cerebellar peduncle. The result will be reported at the next meeting.

Section of Otology.

Chairman—Sir CHARLES BALLANCE, K.C.M.G., M.S. (Vice-President of the Section).

Otosclerosis and Osteitis Deformans: A Pathological and Clinical Comparison.

By G. J. JENKINS, F.R.C.S.

(ABSTRACT.)¹

THE subject may be considered under the following headings:—

(1) A comparison of the pathological changes occurring in bone in osteitis deformans and otosclerosis respectively.

(2) A comparison of the clinical aspects of osteitis deformans with those of otosclerosis.

(3) Conclusions.

The microscopic sections demonstrated are selected from serial sections of the temporal bones of three subjects, two from cases of osteitis deformans and one from a case of otosclerosis.

The *first series* of sections is from the temporal bone of a female, aged 84. (Cephalic osteitis deformans.)

The *second series* is from the temporal bone of a male, aged 61. (Osteitis deformans of skull and long bones.)

The *third series* is from the temporal bone of a male, aged 41. (Otosclerosis.) This series of otosclerosis was selected because it showed the general affection of the capsule of the labyrinth.

The most obvious feature in the microscopic appearance of the affected bone in both diseases is the osteoporosis, which affords the main point of similarity.

In these specimens the trabeculae of bone in osteitis deformans are somewhat coarser, and the lamellae more distinct than in otosclerosis. In some parts the bone has almost entirely disappeared in both diseases.

The sharply defined limits of the osteoporosis are readily seen in otosclerosis, but in osteitis deformans they can only be detected by a careful search. The dense bony capsule of the labyrinth seems to afford most resistance to the progress of osteitis deformans, as it is only in this part that I am able to detect this limitation, which is always somewhat indefinite in this disease.

The large osteoclasts, very obvious in all the sections, are more numerous in otosclerosis. The proliferation of fibroblasts is more evident in otosclerosis, but the fibrous tissue is a more prominent feature in osteitis deformans. Here and there the large spaces in the affected part are in intimate relation to the endosteum. In both diseases, the irregular outline and denser staining of the bone in relation to the endosteum indicate new bone-formation and an invasion of the labyrinthine space. In the series of sections of one of the cases of osteitis deformans (second) the space has been encroached upon to a considerable degree.

In both osteitis deformans and otosclerosis, in the specimens imbedded in celloidin there is a deposit of material, probably fibrinous (Gram-positive), in the perilymphatic space.

¹ This paper will be published in full in the *Journal of Laryngology and Otology*.

The stapes is not affected by the disease in either case of osteitis deformans, and is apparently free.

The microscopic appearances are the same in sections of long bones and skull bones.

Clinical Aspects.—Before making a comparison, let me define first what is meant clinically by otosclerosis, and, secondly, what I mean by the expression "otosclerosis group." I need not detail the text-book description of otosclerosis, but I may draw attention to certain features that seem to me important. First among these is the Rinne test¹ in typical otosclerosis. A negative Rinne to low tones (say below 200) is one of the earliest definite signs. As the disease progresses, the "negative Rinne limit" becomes higher, until it may reach the highest tones of the Edelman series of forks. This is also the case in middle-ear deafness, but there is an important difference in that the Rinne is negative, with a much slighter degree of deafness in otosclerosis than in any form of obstruction deafness.

Bone-conduction is usually if not always diminished in otosclerosis. This is more evident with low than with high tones, and may be found with low tones only.

The low tone limit is always raised in typical otosclerosis, whereas the *high tone limit* is only very slightly affected.

In typical otosclerosis cases the patients hear better with electrical aids; this fact often constitutes a useful test.

The study of otosclerosis has led me to believe that the symptoms and signs of typical cases are due to the site of activity of a disease which can produce other forms of deafness, if it occurs in other parts of the labyrinth. Otosclerosis, as is ordinarily understood, is but one of several forms of deafness that may be produced by the same disease.

Although this paper is not primarily concerned with otosclerosis, it is necessary to consider what are the other types of deafness of the otosclerosis group, otherwise it will be impossible to attach a proper value to the various features of the deafness of osteitis deformans.

A few cases of otosclerosis which I have been able to study from the very onset of the disease have begun as cases of pure internal ear deafness.²

These patients consulted me, for the most part, because other members of the family were very deaf, and they wished to know whether or not they were themselves affected. In one instance the patient came on account of tinnitus in herself with a history of deafness in her family.

It is not uncommon to find typical otosclerosis in one ear, and only loss of bone conduction in the other. Moreover, a patient with intimate relatives affected with typical otosclerosis may himself have internal ear deafness.

We are all familiar with that form of deafness in which the symptoms and signs of internal ear deafness and otosclerosis are mixed in varying proportions.

Although this is a subject of vast extent, I hope that I have been able sufficiently to indicate the way in which I have come to look upon otosclerosis as an accident of disease rather than a disease *sui generis*, and to regard it as one only of the many forms of deafness that may be due to a common pathological process.

¹ In testing all patients it is my practice to use Edelman's forks and whistles, and the monochord.

² Mr. Cheate informs me that Laidlaw Purvis held this view.

For the present I propose to speak of these forms of deafness as the deafness of the otosclerosis group.

THE DEAFNESS ASSOCIATED WITH OSTEITIS DEFORMANS.

I have found that in all cases of osteitis deformans in which the skull bones were affected to a marked degree, the patients were invariably deaf. The difficulties of a study of this deafness are obvious; most of the patients were old and often the mental condition was not sufficiently good to allow of a really detailed examination.

The following are notes of nine cases of deafness associated with osteitis deformans; four of the patients were shown at the last meeting of the Section.

(1) A. C., female, aged 62. Operation by Sir Watson Cheyne, May, 1894. Swelling about 3 in. long pushing behind the malar bone; slightly pushing up inferior wall of orbit. The right alveolus greatly affected. Tears overflow. At operation bony tumour of anterior wall of antrum was removed. Deafness: Right gradually, thirty years; left, five years gradually. Paracusis very definite, *nil* in family. Earache: No, in childhood +. Discharge: No, never. Tinnitus: Rumbling at times. Vertigo: No. Headache: No. Lump in cheek began at age of 15; spread along right zygoma. No increase in size of head and no changes in limb or body noticed by patient. Radiograph of the head, shown at the last meeting of the Section, shows bone changes typical of osteitis deformans. Weber: To right side. Rinne: Negative to all tones in both ears. Bone conduction: Diminished 5 to 10 secs. in both ears. High tone limit: Slightly down in both. Low tone limit: Raised to about 190 d.v. in both. Conversational voice: Right, 2 in.; left, 1½ ft. Whisper: Right, 1 in.; left, 1½ ft. After inflation slight improvement in hearing. Hears much better with electric aid. Paracusis test +.

(2) W. P., male, aged 64. Mental condition fairly good. Deafness: One and a half years, gradually worse; thinks he hears better when in a very noisy place; no history of deafness in the family. Earache: No, never. Discharge: No, never. Tinnitus: No. Vertigo: No. Headache: No. Weber: To left side. Bone conduction: Slightly diminished in both. Rinne: Negative to tones below about 190 d.v.; more definitely so in left than right ear. Low tone limit: Raised in both ears to about 190 d.v. High tone limit: Down in both. Conversational voice: Right, 3 ft.; left, 2 ft. Whisper: Right, 2 ft.; left, 1 ft. After inflation—Conversational voice: Right, 4 ft.; left, 6 ft. Whisper: Right, 3 ft.; left, 3 ft. Test for paracusis: In loud noise—Conversational voice: Right, 4 ft.; left, 6 ft. Whisper: Right, 2 ft.; left, 4 ft. (Heard better than normal individual.) Hearing slightly better with electric aid.

(3) J. J., male, aged 62. Mental condition good. Deafness: Five to six years; gradually worse; right equals left. Paracusis: Three to four years; *nil* in family. Earache: In childhood. Discharge: Not recently. In childhood + in both. Tinnitus: "Rushing of water" for years. Vertigo: No. Post-suppurative effects in the middle ears on both sides. Weber: To left. Rinne: Negative to all tones of Edelman series in both ears. Bone conduction: Diminished; right, 15 secs.; left, 19 secs. Low tone limit: Right, 100 d.v.; left, 190 d.v. High tone limit: Very much down in both. Conversational voice: Right, 2 ft.; left, 1 in. Whisper: Right, 1½ ft.; left, 1 in. After inflation—Conversational voice: Right, 4 ft.; left, 3 in. Whisper: Right, 2 ft.; left, 1 in. Paracusis test in engine room—Conversa-

tional voice: Right, 6 ft.; left, 4 ft. Whisper: Right, 3 ft.; left, 6 in. Hears much better with electric aid—Conversational voice: Right, 9 ft. +; left, 9 ft. +.

(4) P. P., female, aged 89. Memory bad, history not reliable. Right tympanic membrane: Opacities. Slight swelling posterior wall of the meatus and also in anterior wall in region of the outer margin of the tympanic plate. Left similar to right. Swellings not so marked. Weber: To right. Rinne: Negative in both to all tones below 512 d.v.; neutral to C3 in both. Bone conduction: Diminished, both 50 to 60 secs.; C3 fork: Right, 20 secs.; left, 12 secs. High tone limit: Very much down in both. Low tone limit: Up to 190 d.v. in both. Conversational voice: Right, 1 in.; left, 2 in. Whisper: Right, touching; left, touching. Paracusis test—Conversational voice: Right, 1 in.; left, 1 ft. Whisper: Right, touching; left, touching. Hearing distinctly better in noise.

(5) A. J., male, aged 58. Not noticed to be deaf. Deafness: No, never. Earache: No, never. Discharge: No, never. Tinnitus: A year ago; buzzing. Vertigo: No, never. Headache: No. Tympanic membranes: Opacities in both. Weber: To right. Rinne: Negative to all tones below C3 in both ears. Neutral to C3 in both ears. Bone conduction: Slightly diminished with Edelman special fork. High tone limit: *Slightly* down. Low tone limit: Good. Conversational voice: Right, 24 ft.; left, 24 ft. Whisper: Right, 21 ft.; left, 21 ft. (after inflation).

(6) H. A., male, aged 69 years. Mental condition fairly good. Irritable. Deafness: Six weeks in both; similarly six years ago; better hearing in quiet; worse with a cold. Earache: No, never. Discharge: Both recently and six years ago. Tinnitus: No. Vertigo: Dizziness. Has had heart disease. Tympanic membranes: Opacities. Weber: Right equals left. Rinne: Right negative to 512 and all tones below; left negative to 190 d.v. Bone conduction: Diminished, right 10 secs.; left, 12 secs. High tone limit: Very much down in both. Low tone limit: About normal in both. After inflation—Conversational voice: Right, 24 ft.; left, 21 ft. Whisper: Right, 18 ft.; left, 12 ft. Paracusis test in engine room—Conversational voice: Right, 12 ft.; left, 12 ft. Whisper: Right, 1 ft.; left, 1 ft. In case of normal ear—Conversational voice: Right, 12 ft.; left, 12 ft. With electric aid—Whisper: Right, 21 ft. +; left, 15 ft.

(7) L. D., female, aged 45. Osteitis deformans and acromegaly. Deafness: Very slight, noticed three years. Earache: In childhood +. Discharge: Never. Tinnitus: No. Vertigo: Three years off and on; ? cardio-vascular. Tympanic membranes: Opacities in both. Weber: To right side. Bone conduction: Diminished in both about 15 secs. Low tone limit: Good in both. High tone limit: Slightly down in both. Rinne: Positive to all tones. Conversational voice: Right, 36 ft. +¹; left, 36 ft. +. Whisper: Right, 36 ft.; left, 24 ft.. Paracusis test, in slight noise—Conversational voice: Right, 15 ft. +; left, 15 ft. +. Whisper: Right, 3 to 4 ft.; left, 3 ft. In loud noise—Conversational voice: Right, 15 ft.; left, 15 ft. Whisper: Right, 1½ ft.; left, 1 ft. Compared with normal ear test at same time about equal. Hears better in left ear with electric aid.

(8) T. H. M., male, aged 63. Has had hemiplegia. Mental condition poor. Deafness: Slight until one and a half years ago, and much worse about a year; hears better in the noise of street traffic; *nil* in family. Earache: No, never. Discharge: No, never. Tinnitus: No. Vertigo: No. Tympanic membranes:

Opacities in both. Weber: Right equals left. Rinne: Positive in both to all tones above 190 d.v.; lower tones not tested. Bone conduction: Diminished; right, 10 secs.; left, 10 secs. Low tone limit: Doubtful. High tone limit: Very slightly down. Conversational voice: Right, 3 ft.; left, 9 ft. Whisper: Right, 2 ft.; left, 9 ft. Does not hear better with electric aid. Paracusis test +. Conversational voice: Right, 6 to 9 ft.; left, 9 ft. +.

(9) G. F. J., male, aged 76. Could not obtain history. Tympanic membranes: Opacities in both. Weber: To right ear, all tones. Rinne: Negative to highest tones in both ears. Bone conduction: Diminished; right, 56 secs.; left, 60 secs. High tone limit: Right, C3 heard, but limit very much lower than normal; left, some tones heard about C2. Conversational voice: Right, 2 in.; left, noise. Whisper: Right and left. Paracusis test (engine room)—Conversational voice: Right, 3 in.; left, hears voices. Seemed to hear better in noise.

An analysis of the cases above described will show that A. C., W. P., J. J., A. J., P. P., might well be described as having the symptoms and signs of otosclerosis with internal ear deafness more prominently marked than usual. The symptoms and signs in H. A., are those of an early case in the otosclerosis group. H. T. M. is difficult to classify, and in G. F. J., the condition is similar to that found in an advanced case of otosclerosis in an old patient. L. O., has internal ear deafness only, such as may be found in some members of the otosclerosis group.

If we include Mr. Mollison's case, we have ten cases of deafness associated with the disease osteitis deformans, of which the greater proportion have some clinical features similar to those of otosclerosis, in some cases obscured by a greater degree of internal ear deafness than is usually found in this condition.

I have examined only nine cases of osteitis deformans from the otological standpoint. They all had obvious affection of the skull bones, and they were all suffering from deafness in some degree and I think it may be accepted that deafness is not obvious until the skull bones are affected.

There was no recorded history of deafness in the family in any of these cases.

Since in osteitis deformans, the disease in the early stages probably involves the labyrinth at some distance from the foramen ovale, the internal ear deafness may be expected to be more prominent than would be the case had the foramen ovale been involved at the onset.

How far age-deafness may be a complication in these cases I am not prepared to say. Perhaps in some cases the overgrowth of bone at the internal auditory meatus (which is common) exerts a pressure on the auditory nerve and so influences the nature of the deafness (? in G. F. J.).

Though ten cases may not seem sufficient to justify a definite statement yet when the above is taken into consideration I think it is very likely that probably these cases really illustrate the character of the deafness found in osteitis deformans and are not accidental.

CONCLUSIONS.

There is a similarity in the microscopical appearance of the diseased bone in osteitis deformans and otosclerosis. There are differences that may be of great importance, or merely accidental, depending on difference of age,

activity of the disease, or other causes. The deafness found in all cases of osteitis deformans, affecting the head in a marked degree, has some characters of typical otosclerosis deafness.

It is necessary to beware of the danger of attaching too much value to mere similarity, but if there is a certain degree of similarity, clinically and pathologically, then it is natural to ask what is the outcome of these observations. Can the two conditions be identical?

Against this possibility is the absence of any hereditary tendency in osteitis deformans. Otosclerosis usually begins in early life and osteitis deformans is usually recognized late in life, though the latter has been found to appear before the age of twenty. The general affection of the bones of the body in osteitis deformans is in contrast with the localized affection in otosclerosis. We do not find any family history of otosclerosis in osteitis deformans.

If these two diseases are due to the same fundamental cause, then the difference in distribution must depend on the individual, and in "otosclerosis" the peculiarity (probably anatomical) that renders the individual liable to the affection must be hereditary.

Lastly, the causes of these bone diseases may be quite distinct, but it is one characteristic common to both that produces the changes in the labyrinth and the consequent similarity of the deafness.

DISCUSSION.

Sir CHARLES BALLANCE (Chairman) said that he was very glad Mr. Jenkins was taking up this subject and hoped he would continue his research into the intimate pathology of these two diseases, especially osteitis deformans. He trusted that when the intimate pathology was known it would be possible to cure, or at least arrest the disease. In the old days he (Sir Charles) had himself operated on this class of case, and had always found that he could not get beyond the disease in various parts of the skull; local recurrences took place.

A feature of interest was the fibrinous deposits. Such deposits usually indicated inflammation, and if that were so in these cases the discovery might lead to some method of elucidating the real nature of these processes.

Dr. ALBERT A. GRAY said he believed this was the first time there had been an actual demonstration of the similarity of the changes in bone in otosclerosis to those in other diseases of bone, though, of course, the similarity had been suspected. He (Dr. Gray) himself had suspected that there might be a similar change in cases of locomotor ataxia in which spontaneous fractures occurred. The specimens and cases showed definite otosclerosis; there was the sharp line of demarcation, the absence of an inflammatory zone. He had been interested in seeing Mr. Jenkins demonstrate the osteoclasts, because Manasse held that these were not present, bone being absorbed by simple pressure. But he (Dr. Gray) had demonstrated osteoclasts, and now Mr. Jenkins had done so also. He (Dr. Gray) did not agree with Mr. Jenkins that otosclerosis began with evidence of nerve deafness, and it would be a very difficult point to prove. The earliest case of otosclerosis in which he had cut sections was one in which the deafness had existed three years, and in that case there had been a very definite change in the bone. He thought that in all otosclerosis cases, and perhaps in osteitis deformans too, there might be a nerve disease, but not a disease of the cochlear branch of the auditory nerve. The views of pathologists as to the causation of osteitis deformans would be interesting; possibly that disease and otosclerosis might be grouped together. They might be nutritional diseases, i.e., the nerves governing the blood supply and the general nutrition of the bone and other structures might be affected in both diseases; he suspected that was the secret in otosclerosis. It would also be interesting to know whether there was any other disease in bone like that in otosclerosis.

Mr. SYDNEY SCOTT said that about twenty years ago he had examined six cases of osteitis deformans, and made many visits to infirmaries looking for cases. One of the male patients whom he had examined had subsequently died and the skeleton had been placed in St. Bartholomew's Hospital museum. In 1907, he (Mr. Scott) had cut some sections of one of this man's temporal bones and they exactly resembled the first one of Mr. Jenkins' present series. The decalcified bone was embedded and cut in paraffin, which yielded thinner sections than those embedded in celloidin, but they were much more difficult to prepare. He (Mr. Scott) could confirm Mr. Jenkins' observation on the stapes; in his own case the stapes was not ankylosed but was absolutely normal, thus differing from what was seen in the specimens of otosclerosis. The osteoporosis of osteitis deformans was diffuse; he had cut sections from the tibia, and one could scarcely tell the difference under the microscope between the tibia and the affected part of the petrous bone. He had seen osteoclasts, which Mr. Jenkins also described. With regard to the hearing tests, in 1903, he had no Bezold forks, but had used ordinary tests and could not distinguish the deafness of these cases from ordinary senile deafness. It was curious that patients with osteitis deformans should so often become affected by malignant disease. He (Mr. Scott) thought that the occurrence of the fibrinous exudate in the cases shown by Mr. Jenkins might prove to be a pure coincidence; he did not think that changes in the labyrinthine fluid were necessarily connected with the osseous changes. In cases of osteitis deformans the patients often died from some intercurrent terminal disease, such as pneumonia, or bronchitis, which might possibly affect fluid tissues.

Mr. RITCHIE RODGER asked whether he rightly understood Mr. Jenkins that there was no history of hereditary disease. Osler had stated that there had been seventy-five cases recorded, and that at least six of these had shown a definite hereditary history. In the article referred to, by Emerson, leontiasis ossium and acromegaly were grouped with the diseases now under discussion. He (Mr. Ritchie Rodger) thought physicians were now inclined to attribute osteitis deformans and leontiasis ossium to a disturbance in the proportions of the secretions of the internal glands. Possibly some elucidation of the pathology of otosclerosis might come along this line.

Sir JAMES DUNDAS-GRANT described a case of a lady in advanced middle life, complaining of deafness and presenting the features of osteitis deformans. Her deafness was a combination of obstructive and nerve deafness, and the hearing was improved to some extent by the Eustachian catheter and bougie (the Eustachian tubes being extremely narrow, suggesting some narrowing of their osseous portions). Bone conduction was diminished, and there was deafness for all tones higher than 7 and 6 of Galton's whistle, pointing to a lesion of the cochlea. The percentage of duration of hearing for a series of nine tuning-forks is shown in the accompanying charts, *à la* Hartmann and Gradenigo (figs. 1, 2, p. 28). In this case an interesting complication was the occurrence of malignant disease, involving an amputation through the thigh.

Another case was that of a man of great intellectual ability, suffering from deafness and well-marked *osteitis deformans*. The right ear was quite deaf to all tones, including those of Galton's whistle. In the left ear hearing was greatly reduced, and Galton's whistle was not heard above the mark 5'1. Rinne was negative for C1, bone-conduction slightly increased. The malleus under Siegel's speculum was fixed on both sides. The left Eustachian tube was only very slightly narrowed, and hearing was improved after inflation (even for Galton's whistle) to a greater degree than the mere inflation would account for, the improvement which he alleged indicating a functional element. There appears to have been a "middle-ear" defect, and, as shown by the complete and rapidly developed deafness in the right ear and the lowered Galton on the left, a diseased condition of the internal ear such as the changes demonstrated by Mr. Jenkins would explain. The chart (fig. 3, p. 28) shows duration of hearing for a series of nine tuning-forks.

He (Sir James Dundas-Grant) had had a case under his care when he was in general practice, of a patient suffering from deafness, vertigo and optic neuritis. He had made a diagnosis of cerebellar tumour, and overlooked the co-existent osteitis deformans. At a hospital the osteitis deformans had been diagnosed and the cerebellar tumour overlooked. The osteitis deformans and the cerebellar tumour were both discovered at the post-mortem examination in an infirmary.

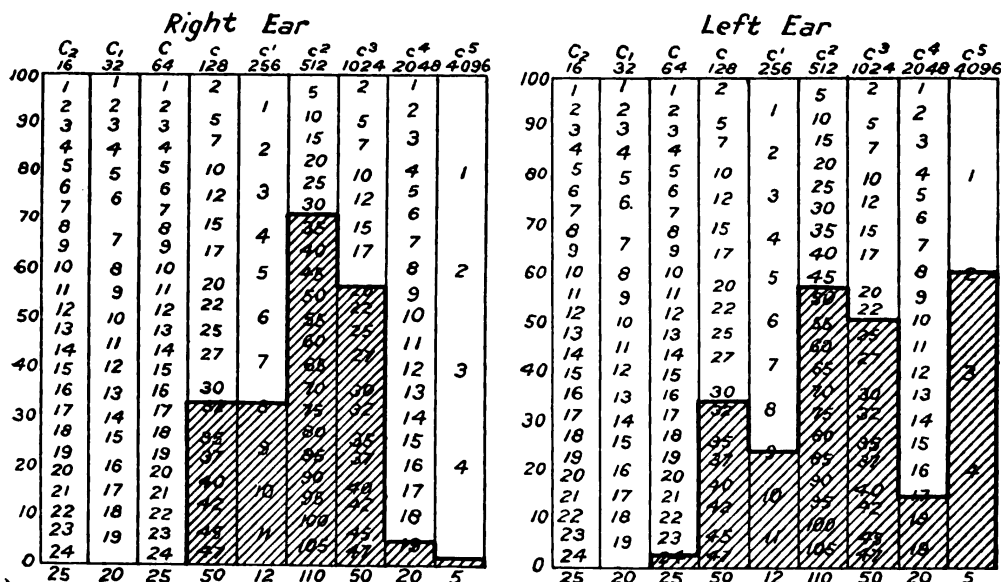


FIG. 1.

FIG. 2.

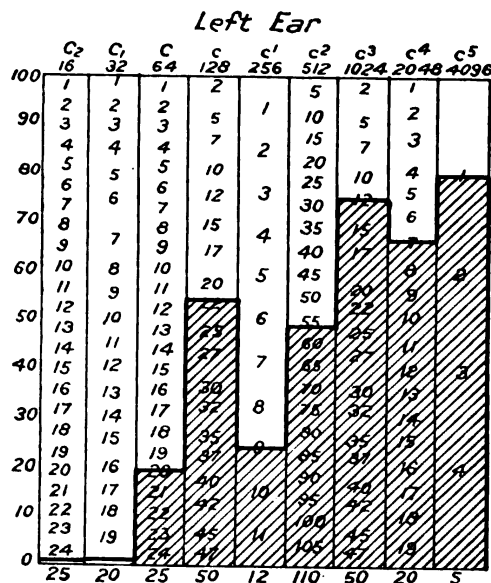


FIG. 3.

Mr. G. J. JENKINS (in reply) said that the question of the ætiology of osteitis deformans was as difficult as that of otosclerosis. A proportion of the cases die of malignant disease, but that in itself was not convincing evidence that osteitis deformans was malignant. The question of its being inflammatory or metabolic was equally difficult. One important point which had arisen out of this study was, that we should distinguish between internal ear deafness and nerve deafness; the present nomenclature was bad, because it was too general. Internal ear deafness should be looked upon as distinct from nerve deafness. He could not be sure about the fibrin in

the labyrinth at the situation indicated, but the deposit was Gram-positive in staining, and this was a selective stain for amorphous fibrin. Whether it was accidental or a part of the disease was a difficult question. It might be because these patients were old subjects that fibrin was deposited in the labyrinths, or the deposit might be peculiar to otosclerosis and osteitis deformans, and might be an important factor in the pathology. In two of the cases he showed at the last meeting the pituitary fossa was distinctly enlarged, and one case was obviously a mixed one, having both osteitis deformans and acromegaly. At a former meeting of the Section Mr. Mollison had shown a case having some of the characters of otosclerosis; this paper was the result of the stimulus derived from that case.

Case of Necrosis of the Left Temporal Bone, involving Facial Nerve and Labyrinth, following Triple Infection of Scarlet Fever, Measles and Diphtheria, in a Child aged 7.

By J. F. O'MALLEY, F.R.C.S.

MARCH 7, 1922: Admitted to Willesden Municipal Hospital with scarlet fever and measles (co-existent). Been ill four days. Temperature normal. (Under care of Dr. W. J. J. Stewart, Medical Superintendent.)

March 14: Temperature 103° F. March 15: Temperature 104° F. March 16: Temperature 105° F. Apparently very severe attack of measles. March 22: Nasal discharge and left otorrhoea; purulent conjunctivitis right eye; trace of albumin in urine. March 23: Throat inflamed; swabs from nose, throat and ear: positive to Klebs-Loeffler bacillus; 8,000 units of anti-toxin given twice on that day. March 24: Redness behind left ear over mastoid; left facial paralysis; incision down to bone under general anæsthesia; no pus; wound packed and fomented; hearing normal (W. J. J. S.). March 25: Temperature down to 100° F. Less nasal discharge. March 28: Left facial paralysis continues; in mastoid wound large area of dead bone is seen; hearing impaired left side. March 29: Antrotomy; no pus in antrum or mastoid cells (W. J. J. S.).

April 1: Case seen by myself. Wound very unhealthy and not granulating; area of dead bone large and apparently affects whole of mastoid process.

May 13: Chicken-pox. May 25: Swelling behind right ear; incision down to bone and pus evacuated under local anæsthesia; swab negative to Klebs-Loeffler bacillus (W. J. J. S.); healed uninterruptedly.

June 1: Floor of left mastoid wound occupied by dead bone.

July 15: Sequestrum of bone separated.

September 15: Large piece of bone separated; more dead bone in deeper parts.

December 7: Operation. There was a depressed irregular wound, scarred at the edges, with pus and dead bone in the centre; under general anæsthesia scarred edges resected and trimmed; dead bone removed; plastic of external meatus done and post-aural wound closed; drained through ear as in radical mastoid.

January 25, 1923 (present state): Wound healed; slight discharge from ear occasionally; facial paralysis less apparent. Vestibular test: Cold caloric, no response; C2 tuning-fork (right ear closed) not heard.

30 Banks-Davis: *Laceration of Meatus and Tympanic Membrane*

DISCUSSION.

Mr. O'MALLEY said that he had made inquiries about symptoms suggesting that the labyrinth was affected before he saw the case. The Medical Superintendent said there were several attacks of retching on the night of March 24-25, twenty-four hours after the onset of the mastoid condition; during the period of the mastoid condition the child was very ill, and could not swallow food, but was nourished with saline and glucose for ten days.

Mr. MARK HOVELL said that for a long time past he had felt it greatly to be regretted that the disinfection of the naso-pharynx was not universally regarded as a routine treatment in cases of measles, scarlet fever and some other infectious diseases. If this were systematically done, he believed it would prevent middle-ear complications in many cases. Disinfection was easily done by spraying collosol argentum through the nostrils.

Sir CHARLES BALLANCE (Chairman) said that he remembered having had diphtheria long ago, before the days of the antitoxin, even before the organism had been isolated; his nose had been sprayed with sulphurous acid, which was very unpleasant, but might have been effectual, as he had no mastoid or ear trouble afterwards and had made a good recovery.

Parotid Fistula in the Scar of an Old Mastoid Wound.

By H. J. BANKS-DAVIS, M.B.

PATIENT, a woman, aged 23; has had operations on both mastoids. Two operations have been performed on the left side.

The secretion exudes from a pin-point depression at the lower end of the scar on the left side. The flow is periodic and "occurs when she is eating."

Mr. Norman Patterson has shown a similar case—the condition is not uncommon.

Laceration of Meatus and Tympanic Membrane produced by a Celluloid Knitting Needle.

By H. J. BANKS-DAVIS, M.B.

PATIENT, a woman, aged 40. Severe hæmorrhage, continued for several hours: the meatus had to be tightly plugged in order to arrest it. It was venous bleeding, and the question is: "What was the source?" Blood came down the Eustachian tube. The patient is well now, except for vertigo. It is unlikely that the jugular bulb was injured.

DISCUSSION.

Mr. J. F. O'MALLEY referred to a similar case of injury to the tympanic membrane, in which there were symptoms of the same kind, though the hæmorrhage was not so severe as in Mr. Banks-Davis's case. There was, however, considerable vertigo. He (Mr. O'Malley) had often wondered what was the pathological lesion which caused the vertigo.

Sir CHARLES BALLANCE (Chairman) said that he remembered the case of a nurse at St. Thomas's Hospital, long ago, whose ear had been syringed by another nurse with a long-pointed syringe, which slipped and went through the tympanum, impinging on the inner wall. The patient fell down as if she had been shot, and had very severe vertigo for a long time; she could not resume duty for eighteen months.

Section of Otology.

President—Sir CHARLES BALLANCE, K.C.M.G., M.S.

Case of Acusticus Tumour (Right) ; Operation by Sir Victor Horsley in 1912 ; Removal of Tumour ; Recovery.

Shown by F. J. CLEMINSON, M.Ch.

(Report by Dr. F. M. R. Walshe.)

DURING the summer of 1912, the patient (a personal friend of the exhibitor) suddenly realized that he had taken to applying the telephone receiver to his left ear, instead of to his right as had been customary. On investigation he found that the right ear was almost completely deaf. In August of that year he began to notice transient amauroses on any exertion, and by November it became apparent that his vision was failing. Examination by Dr. T. R. Elliott at this time revealed, in addition to deafness of the right ear, double optic neuritis. Weakness and unsteadiness of movement in the right arm, and some unsteadiness of gait developed at this time, and a diagnosis of right-sided acusticus tumour having been made, he was operated on by the late Sir Victor Horsley in December, 1912, and a large tumour was removed. Mr. Wilfred Trotter was present at the operation. Following operation, a right-sided facial palsy of peripheral type appeared, and there was for some weeks profound cerebellar ataxy of the right limbs. This slowly regressed during the spring of 1913. From 1913 until 1917 the patient lived an active open-air life in New Zealand, and is now continuously engaged in business.

He has kindly submitted himself to examination, and his present state is as follows: He expresses himself as physically fit, and as conscious of no mental or emotional change from his condition prior to the development of symptoms in 1912. There is an occasional cloudiness of vision in the right eye, and the right ear remains completely deaf. The facial weakness has ceased to improve. He is free from headache or other signs of increased intracranial tension, and the small cerebral hernia now present never varies in size nor becomes tense. In writing he is conscious of some clumsiness of the right hand, and he can only play slow pieces on the piano, as on rapid movement the movements of the right hand and wrist become ataxic. His gait is normal, but he cannot kick a football with his right foot with any degree of precision. His golf has become very uncertain since his operation. He can make a series of strokes with his old facility, but is liable to sudden periods in which he becomes very wild and inaccurate in his play.

Examination reveals a small flaccid swelling over the right cerebellar fossa overlying the bone defect. The optic discs show some post-papillitic atrophy, but there is no swelling and on rough tests vision is good in both eyes. The right ear remains completely deaf. The pupils are normal and ocular movements are of good range and association. On deviation to the right there is a slow wide nystagmus, but no definite nystagmus at rest or on looking to the left. There is loss of sensibility to light cotton-wool touch over the distribution of the right fifth nerve, and also to pinprick and to thermal

sensation. The right corneal reflex is absent. Pressure sense remains. The motor fifth is intact. There is marked paresis of the right face, with occasional fibrillary twitchings of the paretic muscles. The other cranial nerves are normal.

There is slight defect of co-ordination in the right hand, shown as dysmetria and dysdiadochokinesis, and on passive movement there is slight hypotonia of the right arm. These defects are minimal, and do not constitute a grave disability. No demonstrable abnormality in this respect is appreciable in the right leg, and there is no true weakness of the right limbs. The tendon-jerks are brisk and equal on the two sides, and the plantar responses are flexor.

There is a "nystagmus" of the pharyngeal muscles of the right side.

Specimen of Brain and Acusticus Tumour.

Shown by F. M. R. WALSHE, M.D.

(Introduced by Mr. F. J. CLEMINSON.)

THE temporal bone shows a dilatation of the internal auditory meatus.

The case from which this specimen was obtained is alluded to in Dr. Walshe's paper.

Acusticus Tumours.

By F. M. R. WALSHE, M.D.

THE appearance within the past few years of two important contributions to the subject of acusticus tumours (I refer to Cushing's book,¹ and to Mr. Fraser's paper, read before this Section²) has left little to be said, and I have no original information to impart, but it may be of some use to emphasize certain points in the clinical picture, and to make some observations on the functional examination of the labyrinths in the light of the work of Magnus and de Kleijn. To my mind this work has shown the inadequacy of what has hitherto been regarded as a complete investigation of labyrinthine functions, and has revealed in a remarkable manner, and for the first time, the complex character of labyrinthine activities.

In its most characteristic form the clinical course of a case of eighth nerve tumour is well known and easily recognizable. From the fact that the tumour arises on the nerve itself, and on that part of it which lies in the internal auditory meatus, it is not surprising that symptoms referable to the *eighth nerve* should usher in the malady. These symptoms are progressive deafness with or without tinnitus. It is usual to regard tinnitus as an irritative symptom, and, in respect of the cochlear division, we should expect stimulation of the nerve to produce some such symptom. It must be remembered, however, that, in general, neurological experience does not readily favour the idea of "irritation" as an explanation of a symptom which may persist continuously for years, and is associated with a very slowly progressive pathological process. Such symptoms are more commonly found with acute or rapidly progressive lesions, and tend to be intermittent and transient. In this connexion we may note that tinnitus is not a constant symptom, and when it does occur it may be long delayed. While, therefore, I have no alternative explanation to provide, we should not assume as a matter of course that tinnitus indicates what is

¹ Cushing, "Tumours of the Nervus Acusticus," Philadelphia, 1917.

² *Proceedings*, 1920, xiii (Sect. Otol.), p. 109.

loosely called "irritation" of the auditory branch of the eighth nerve. Nor is it always easy to establish deafness as an initial symptom, for deafness, like unilateral blindness, may pass unnoticed by a patient until revealed by examination. With regard to the vestibular division of the nerve, I think vertigo is the only symptom we can definitely attribute to this branch. Only now, in the light of Magnus and de Kleijn's work, are we in a position to differentiate between cerebellar and labyrinthine defect-symptoms with any degree of accuracy, and the evidence indicates that ataxy in movement, nystagmus, and muscular atonia or hypotonia are cerebellar in origin and not labyrinthine. Magnus and de Kleijn have found that throughout the whole animal scale, from guinea-pig to ape, nystagmus is merely an immediate and very transient result of unilateral labyrinth extirpation. The sole permanent system is rotation of the head—so that the face points away from the side of the lesion—with some inclination of the head towards the affected side. In man this amounts to turning of the chin away from the lesion, and a lowering of the occiput towards the side of the lesion. This is a relatively common manifestation in cerebellar lesions, and is spoken of as a cerebellar symptom. When we recall the fact that the study of cases of tumour and gunshot wounds provides us with the bulk of our clinical material, it is apparent that in both instances involvement of the labyrinth or of the eighth nerve is to be anticipated, and therefore it seems probable that the so-called cerebellar position of the head is a sign of unilateral labyrinthine defect. Loss of muscle tone does not appear to be a direct result of labyrinthine extirpation, but when present it is due to the rotation of the head, which sets up what Magnus has called a "tonic neck reflex." This reflex, in turn, produces diminution of tone in the extensor muscles of the limbs on the side of the lesion. I would, therefore, say that deafness, tinnitus, vertigo and inclination of the head are the symptoms definitely indicative of progressive paralysis of the two divisions of the eighth nerve. It may happen that these four symptoms long antedate the appearance of other focal symptoms, and when this is the case we may rightly speak of an initial or "otological stage" of the disease. So far, despite the exhaustive nature of the tests employed for the functional assessment of the eighth nerve, I have not met with a case of eighth nerve tumour recognized as such during this stage. I do not level this as a reproach at otologists or neurologists, for I believe that it would be very difficult to persuade the surgeon to act upon even the fullest information obtainable at this stage.

The next group of symptoms to appear, when the malady follows its usual course, are those referable to progressive paralysis of function in *cranial nerves adjacent to the eighth nerves*, and in the cerebellum. The first of the cranial nerves to show signs of involvement is usually the *fifth*. Here, again, the symptoms may be grouped as irritative and paralytic. Among the symptoms actual pain is not common; the patients complain rather of abnormalities of sensation, such as numbness, creeping sensations in the skin, and so on. In one case under my observation, the patient first sought advice for a distressing feeling of numbness over the right cheek, and, among other diagnoses, that of an infected antrum was made. This may seem a trivial symptom to attract much attention, but anyone who has endured the application of cocaine by a dentist will know how obtrusive and annoying a small patch of anæsthesia on the face may be, and indeed this patient was found to have sensory loss and absent corneal reflex on the right side, physical signs which, had they been looked for, would have betrayed the nervous origin of the symptoms. It is commonly said that diminution or absence of the corneal reflex is the initial

defect-symptom in progressive fifth-nerve lesions, and in the case of which the specimens are shown to-night this was the only objective sign referable to the sensory division of the fifth nerve within a fortnight of death. Cushing records a similar case. The motor division of the fifth nerve is less commonly affected, though in the case just mentioned I was satisfied that there was slight but appreciable weakness of the masseter on the side of the corneal areflexia.

Next in importance, when it is present, is involvement of the *facial nerve*. Facial paresis may be a long delayed sign, and is rarely profound. Generally it consists in slight asymmetry of the lower part of the face on voluntary and expressional movements. Like sixth nerve palsies, which we shall consider next, it is apt to vary from day to day, as was strikingly shown in one case which came under my observation. The patient was a woman with right-sided eighth and fifth nerve lesions and slight right-sided cerebellar symptoms. It was not possible to say definitely whether there was or was not a slight paresis of the right side of the face. However, a diagnosis of acusticus tumour having been made, the question of operative treatment had to be set before the patient, who was extremely upset and wept copiously for the rest of the afternoon. I saw her again just after tea-time, and was surprised to observe a profound weakness of all the facial muscles on the right side. By the next morning this weakness had again disappeared. Undoubtedly, the violent muscular activity entailed in her crying had brought out, by a process of fatigue, a latent weakness of the face. Occasionally, irritative symptoms referable to the seventh nerve may be present in the form of clonic spasms of the face muscles, and Cushing records that a diagnosis of Jacksonian fits has been erroneously made and exploration of the crossed motor cortex undertaken to reveal the cause. The mistake may seem an absurd one, but it is by no means so. The twitching of the facial muscles may be so regular in force and rhythm as closely to resemble a Jacksonian fit. In a fatal case of lethargic encephalitis, which came under my observation some time ago, such a clonic spasm of the lower part of the face on the right side was the sole focal nervous symptom present throughout the course of the malady, and it was quite impossible to decide whether its point of origin was the cerebral cortex or the seventh nerve nucleus. Microscopic examination revealed an intact motor cortex, but showed that the seventh-nerve nucleus was the seat of characteristic and well-marked lesions. It must also be borne in mind that signs of a crossed hemiplegia may be present in eighth-nerve tumours, appearing first as slight paresis of the face of upper neurone type; I shall later describe such a case. It is scarcely surprising, therefore, that twitching of the face should be mistaken for a symptom of similar localizing value in cases where the typical picture of eighth-nerve tumour is not present.

Sixth-nerve Palsy.—Paralysis of the external rectus is relatively common, but, as it occurs in cases of the kind under discussion, it is almost certainly what Collier has called a "a false localizing sign." In other words, sixth-nerve palsy here, as in so many other varieties of cerebral tumour, is not the result of direct involvement of the nerve by the tumour, but is a general pressure effect due, according to Cushing, to strangulation of the nerve between the floor of the skull and the anterior inferior cerebellar artery. Its relatively late appearance, and its tendency to fluctuation and transient disappearance, both indicate this mode of origin.

Symptoms referable to the *ninth, tenth and eleventh nerves* certainly occur, but must be regarded as terminal symptoms, rather than as aids to diagnosis.

They are manifested as dysphagia and dysarthria, and need not be further considered here.

Next in order of importance are symptoms dependent upon *compression of the anterior part of the lateral lobe of the cerebellum*. These vary in date of onset and in intensity from case to case, but they may be said to be a constant part of the typical clinical picture which we are now considering. They are referred to the side on which the tumour is situated and consist of muscular hypotonia, an inability to perform rapidly alternating movements, a tendency to spread of innervation to muscles not normally taking part in a particular movement, and a tendency to error of projection, so that the patient overshoots the mark and may deviate above or below, to right or to left of the object aimed at. In the case of the lower limbs, this ataxy produces a staggering gait and an inability to co-ordinate the component elements in wide movements employing the musculature as a whole, so that the patient loses balance and tends to fall towards the side of the lesion. In the case of the muscles of the head and neck, the same disorder of co-ordination is manifested as nystagmus and defects in articulation. I do not propose to describe cerebellar ataxy in greater detail, or to recite the list of polysyllabic names of Greek derivation by which we have learned to replace a simple descriptive account. I cannot see that such words as "dysdiadochokinesis" have any informative value. On the contrary, they are apt to induce us to suppose that we have really got to the bottom of the nervous disorders which lead to the phenomenon in question, whereas we have not.

In short, the true focal symptoms of an eighth nerve tumour are referable to the eighth, fifth and seventh nerves and to the cerebellum, and these generally usher in the clinical picture and dominate it throughout its course, and are the basis of clinical diagnosis.

We must, however, consider further the general signs of raised intracranial tension, not only because they form a part of the whole clinical picture, but because, as we shall see, they may dominate it so greatly as to obscure the focal symptoms. In this way they may give rise to a clinical picture widely different from that which we have been considering. In cases of eighth-nerve tumours, general pressure symptoms result from compression and distortion of the brain-stem, which blocks the exit of cerebro-spinal fluid from the ventricular system and thus leads to secondary internal hydrocephalus. The symptoms produced in this manner are headache, vomiting and progressive impairment of vision from choked discs. In addition, there may be marked impairment of intelligence, leading to the not unknown error of diagnosing a frontal lobe lesion in cases of cerebellar tumour. As a rule, the true focal symptoms have developed before the effects of general increase of intracranial tension have reached any high degree, but from time to time general pressure symptoms dominate the clinical picture from the beginning and the focal symptoms may never appear to their full extent. Gordon has recently recorded a series of cases of cerebellar tumour, including three of eighth-nerve tumour (as far as can be gathered from his report) in which focal symptoms were conspicuously absent throughout the whole course of the illness, and the impression obtained is that the apparent uniformity of the clinical course of an eighth-nerve tumour as described by Cushing and others depends in part on the fact that cases of this lesion are not recognized as such unless this typical clinical picture is presented. In other words, a somewhat false and deceptive precision in our notions on the subject is apt to develop.

The case from which the specimens exhibited this evening were obtained

was of such a nature, and a brief summary of the patient's history of the symptomatology may serve as a corrective to the simple cut-and-dried symptom-complex we have described. The patient was a young woman who was admitted to hospital for a uterine lesion. On admission she made no complaint of symptoms referable to the nervous system but it was observed that she had considerable headache, appeared to have defective visual acuity and was occasionally sick. In addition, she was apathetic and distinctly stupid. Examination revealed a chronic otitis media with free discharge in the left ear, which was completely deaf. There was bilateral papilloedema and gross impairment of visual acuity. Repeated examination from day to day revealed two other physical signs, the significance of which I freely admit I had not the courage to face, though their possible meaning did occur to me. These were absence of the left corneal reflex and slight paresis of the left masseter muscle. The facial movements were normal on the two sides at first, and there was no defect of cutaneous sensation on the left half of the face. Arm movements, articulation and deglutition were normal. She was not taken from bed to have her gait tested. During the week following admission, there developed a distinct and constant paresis of the lower part of the right side of the face. On appearance of this sign a left supra-tentorial decompression was performed, but it gave no relief to the rapidly increasing general pressure signs, and she died comatose.

At autopsy the tumour which is now before us was found. In situation, relations and appearance, and on microscopic examination it is clearly a typical eighth-nerve tumour. In addition, there is seen to be some dilatation of the ventricles and secondary internal hydrocephalus.

In this case, the presence of middle-ear disease on the left side blinded us as to the significance of the deafness, while the absence of sensory changes in the face induced me to hesitate to place much reliance on the loss of the left corneal reflex, or on the slight paresis of the left masseter. It is now clear that both were true localizing symptoms. Nevertheless, this clinical picture is very different from that described as typical of this lesion and was dominated throughout by the signs of hydrocephalus. The pons is seen to be grossly distorted, and this no doubt was the source of the hemiplegic type of facial paresis observed on the side opposite to the tumour. The distortion of the brain-stem present in this specimen indicates also the high degree of compression that the ascending and descending paths may undergo without giving rise to disturbances of function, and in general we may say that variations in the reflexes, in power and in sensation in the trunk and limbs develop late if they develop at all, and give no reliable information as to the side of the lesion.

There is one other point to be mentioned. I do not propose to describe the characters of the deafness found in these cases. Many of you have had a wide experience of eighth-nerve tumours and can state whatever generalization may be possible in the matter far better than I can, but the vestibular division of the nerve raises some questions of physiological interest which may be worth brief mention. We may consider the eighth nerve as composed of sensory and non-sensory parts, namely, the cochlear and vestibular divisions. The vestibular nerve belongs to the non-sensory afferent proprioceptive system, and, as the work of Magnus and de Kleijn has revealed, is itself physiologically dual. It has two end-organs, subserving separable and distinct functions; these we may speak of as the otolith organs and the semicircular canals. These receptors react to stimuli of different quality and give rise to reflex reactions in the musculature of totally distinct character. The otoliths are stimulated not

by movement, but by variations in the position of the head in relation to the horizontal plane. The reflex reactions they evoke are variations in muscle tone, and therefore in attitude, and these variations persist as long as the posture of the head which gives rise to them is maintained. Thus a tonic reflex arising in the otolith organ may persist for months, as Magnus has shown. The semicircular canals, on the other hand, are stimulated by rotation or by movement in a straight line, either vertically or horizontally. The muscular reactions resulting are not postures, but movements which cease when the stimulus evoking them fails. Mr. Alexander Tweedie has described these different types of reflex reaction before the Section,¹ and I shall not recapitulate his description. The point I wish to make, and, having made it, I shall discreetly leave the subject, is that what we commonly accept as a complete examination of the vestibular division of the eighth nerve is merely an examination of the semicircular canals and cannot be considered as throwing any light on the otolith organs, the functions of which are not less important. In other words, the work of Magnus has revealed that the labyrinth is a dual organ physiologically, and that hitherto we have investigated but one aspect of its functions. It seems possible that a careful investigation of the tonic labyrinthine reflexes might in these cases provide signs of distorted function in the labyrinth at a time when the present repertoire of semi-circular canal tests give us negative results. The elaboration of such an examination-technique is in itself a piece of work which would be well worth doing, if for no other purpose than that it would make otologists familiar with the series of delightfully lucid papers in which Magnus and his collaborators record observations which have rendered obsolete much of the lore still passing muster amongst us as the physiology of the vestibular nerve and the labyrinth.

Surgical Treatment of Eighth Nerve Tumours.

By WILFRED TROTTER, M.S.

(ABSTRACT.)

THE auditory fibroma is a benign slow-growing tumour, which is almost invariably single. Its great seriousness as a pathological condition is therefore due entirely to its situation. Successful operation leads to a certain cure with very little subsequent disability. The problem as to how these tumours should be removed is thus one which justifies the minutest attention to its purely technical side.

It is important that the surgeon should have a clear idea of the anatomical conditions which (a) give rise to the characteristic symptoms of the tumour; (b) lead to certain important complications; and (c) restrict and condition the method by which the tumour is to be reached.

Of these considerations the most important are those which arise out of the situation of the tumour in front of the cerebellum and to the side of the brain-stem. This situation renders it necessary to approach the tumour after dislocating the cerebellar lobe, and gives rise by pressure on the central canal of the nervous system to the most important complication that is met with, viz., secondary hydrocephalus and a general rise of intracranial tension.

It is when this complication is present, as it almost invariably is in the late stages, that the operation becomes most difficult and dangerous. From

¹ *Proceedings*, 1921-22, xv (Sect. Otol.), pp. 15, 19-24.

the surgeon's point of view it is almost impossible to exaggerate the difference in seriousness of operations undertaken before or after the onset of secondary hydrocephalus.

The principal features of operative technique are as follows: The patient is in the prone position with the head flexed and supported on a separate head rest. The intratracheal method is the most convenient for the anæsthetic.

The crossbow incision is used, and in dividing the muscles it must be remembered that the close suture of them at the conclusion of the operation is very important. The bone is removed so as to expose the lower edge of the lateral sinus on each side, and to open up the foramen magnum freely. Throughout the procedure it is extremely important to restrict the loss of blood in every possible way. To ensure this the free use of Horsley's wax is most important. The dura mater is opened in the foramen magnum over the cone of cerebellum, which usually has been displaced into the spinal canal. The freeing of this part of the cerebellum usually results in an escape of fluid, and a reduction of the local tension. If the tension is not reduced in this way the lateral ventricle must be tapped through a separate opening in the bone above the superior curved line, by means of a needle passed into the posterior horn. When the dura has been freely opened and everything has been done to relieve the intracranial tension, the cerebellar lobe is drawn inwards until the tumour is reached.

It is important to remember that during this process cystic collections of fluid are apt to be met with, and may be mistakenly regarded as the source of the symptoms. When the tumour itself is reached, it is to be recognized by its consistence being greater than that of the brain, by its smooth surface, and by its fixation to the outer wall of the posterior fossa.

No attempt is made to remove the tumour intact. The capsule is opened and its contents removed by curetting or by suction, if the latter is possible. In favourable cases, after the substance of the tumour has been got out, an attempt should be made to remove the capsule. This can be safely accomplished in certain cases, and if it is not done a recurrence of the disease is sure to take place sooner or later.

Throughout the intracranial stage of the operation hæmorrhage is often free, but it can usually be controlled by saline packing, and no time should be grudged for this purpose. The dura mater is left freely open, the muscles are very carefully sutured so as to prevent a leakage of cerebro-spinal fluid, and the skin wound is completely closed.

After such an operation, if the patient escapes immediate effects of shock and hæmorrhage, the only serious danger is the development of œdema of the medulla. If this does not occur within the first forty-eight hours the prospects of a satisfactory recovery are very good.

In general it should be the object of the surgeon to complete the operation at one sitting. He will, however, occasionally be compelled to break off on account of technical difficulties in controlling hæmorrhage or reducing intracranial tension. Such a decision should not be founded on any expectation that a mere decompressive operation will be of any permanent benefit to the patient, but should have in view another attempt at removal of the tumour when local conditions become more favourable in the course of a few weeks.

DISCUSSION.

The CHAIRMAN (Sir Charles Ballance) said that he saw his first case of tumour of the auditory nerve as long ago as 1887. Sir Seymour Sharkey asked him to see a patient who had come in with absolute deafness in one ear, and double optic neuritis. The case

was recorded in *Brain* in 1889. The patient was a male, aged 41, and there had been a gradual onset for about two years, with occasional pain in the head, and tinnitus. He was taken into hospital. The headache increased, he had continual giddiness and tinnitus, and ultimately attacks of unconsciousness. Six months after he (Sir Charles) saw him, the patient began to suffer from facial palsy, and three months later he died. The specimen was in St. Thomas's Hospital Museum. Though Sir Seymour Sharkey held the view, shared by others, that it was a case of tumour of the auditory nerve, there was, at that date, no question of operation, and no physician would have listened to a suggestion to remove such a tumour. In doing the post-mortem examination, it was found that the tumour invaded the internal auditory meatus, which was expanded. Radiography, and the comparison of the meatus on the two sides, were great aids in present-day diagnosis. In Politzer's work appeared a picture of a tumour of the auditory nerve, about the second case he (the speaker) knew of. It was that of a woman who had had deafness for ten years. When seen three months before death she had double optic neuritis, soon followed by facial paralysis and dementia. The specimen was obtained by Dr. von Millengen, of Constantinople, and sent by him to Politzer. The internal auditory meatus was expanded by the pressure of the tumour. In dissecting a tumour of this sort, it was a surprise to see how the facial nerve could gradually lengthen and wander over the side of such a tumour, and how late in the case facial palsy might come on. It might be that this palsy supervened when pressure from the growth in the internal auditory meatus occurred. About two years after Sharkey's case he saw another case, that of a woman, and she also died. He saw the post-mortem examination done. The tumour was in much the same position, and facial palsy only supervened three months before the end. As Mr. Trotter pointed out, these patients often died of internal hydrocephalus. He (Sir Charles Ballance) had long ago assisted at some of the first operations in London on encapsuled tumours of the posterior fossa. At first these tumours were avulsed much in the same way as weeds are taken from a garden path, but fortunately a more gentle and dainty method now prevailed, care being taken that no drop of blood should be lost. In one early case at St. Thomas's Hospital, when he had exposed the tumour he felt he had reached the summit of his ambition, for he thought the growth would come away easily. He removed it, but there was severe hæmorrhage from the superior petrosal sinus. He had seen very serious hæmorrhage from the anterior inferior cerebellar artery, but that from the petrosal sinus was more serious, as it was difficult to control. There was a way to prevent hæmorrhage in exposing these tumours which had not been mentioned. It was a method used by Sir David Ferrier. He used marine sponges, perfectly dry. They absorbed the cerebro-spinal fluid and compressed the brain without injuring it, and it was a method he (Sir Charles Ballance) recommended to operators.

He agreed with Mr. Trotter as to the uselessness of a decompression operation in these cases. Decompression seldom relieved serious local pressure. With regard to doing the operation in one stage, as Mr. Trotter advocated, that, of course, was the ideal method, but he (Sir Charles) thought that in the two-stage operation there was this great advantage that, if one opened the dura—and no operation was effectual without opening the dura—the patient in some cases would stand the operation better. Before the war he knew of two cases in which the dura was not opened, and the patient died before a second operation could be done. Many years ago he did some experiments with Sir Charles Sherrington to see what would be the effect of taking away large portions of the skull, and how much more fluid could be introduced into the intradural space after such a large craniotomy. They found that the amount of fluid which could be introduced by removal of the bone alone was infinitesimal. Therefore, it was clear that there was no relief of pressure, however large the craniotomy was; the dura must be opened, or the patient was not safe.

With regard to the great discomfort of having even a small patch of anæsthesia, mentioned by Dr. Walshe. In some of these cases there was involvement of the fifth nerve, and the fact of the discomfort he could corroborate from his experience of a case he saw after the South African war. A colonel had sustained a gunshot wound of the leg, which divided the external saphenous nerve. It rendered the outer side of his little toe and foot anæsthetic, and though previously a good walker, he could not then march more than 200 yards. Dr. Walshe had mentioned a case of mistaken diagnosis,

in which spasm of the facial nerve had led to the diagnosis of cerebellar tumour being changed to that of a tumour above the tentorium, as it was looked upon as a Jacksonian symptom. He (Sir Charles Ballance) had experienced that kind of mistake on more than one occasion. He remembered two such instances which had caused him much distress. The patients in both cases were nurses. The first had symptoms of tumour of the brain, and much pain in the head. Dr. Hughlings Jackson, Sir William Gowers, Sir David Ferrier and Dr. Charles Beevor all saw the patient on several occasions, and all concluded there was a tumour in the left cerebellar fossa. He (the speaker) removed all the bone over the cerebellum, and there was no tumour of the left cerebellar fossa, nor of the right. Much fluid was let out. She became comparatively well for fifteen months, and then was ill again. She came into the hospital, and died eighteen months after the operation he had performed. An encapsuled tumour of the meninges of the opposite frontal lobe was found which could easily have been removed. She had had crossed cerebello-frontal headache. Three years ago a similar event happened, and he exposed the cerebellum without finding a tumour there. In the case of this nurse also a most careful examination was made by several neurologists and she died about two years afterwards. There was an encapsuled tumour of the opposite frontal lobe, and if it had been diagnosed and operated on it would have come out like a pea from its pod.

Mr. Cleminson had just shown a patient from whom the late Sir Victor Horsley had removed an auditory nerve tumour, and he (the speaker) had observed a dropping of the shoulder. He believed that Dr. Gordon Holmes, in his Lectures, had ascribed this dropping of the shoulder to hypertonia due to involvement of the cerebellum. He (Sir Charles) did not remember, in any cases of tumour of the cerebello-pontine angle he had seen, that dropping of the shoulder was noted before the operation. Probably this was due to a failure in observation. Therefore the case shown by Mr. Cleminson was of great interest to him.

Dr. GORDON HOLMES said that experiences differed, and certain symptoms probably attracted the attention of some observers more than others; but on the whole he agreed with the clinical picture which had been put forward by Dr. Walshe and Mr. Wilfred Trotter. He had seen a large number of cases of the kind under discussion, especially during the last few years, and previously he had had a rather extended experience of them as a pathologist. He had been impressed by the variability in the shape of the tumour; in some cases it was a firm, more or less spherical mass, in others a growth of softer structure which moulded itself along the lateral surface of the pons and medulla. He thought this fact was the explanation of the considerable variability of symptoms seen in different cases; in those in which a spherical tumour lay in the region of the internal auditory meatus, the upper cranial nerves alone were affected, but in many cases the early symptoms pointed to a disturbance of the lower cranial nerves, weakness of the palate, disturbance of the movements of the vocal cords, dysarthria and occasionally dysphagia occurring. He had that day looked through notes on thirteen cases which he had seen in the last year or two, in all of which the diagnosis had been confirmed either by autopsy or by operation; he found that few had marked anæsthesia on any part of the face, only a small proportion complained of pain or numbness there, though in a large number, but not in all, the corneal reflex on the same side was absent. Therefore it could not be said that there was always clinical evidence of disturbance of the trigeminal nerve. In his experience the facial nerve was more frequently involved than it was said to be by Dr. Walshe. The extraordinary feature of these cases, to those who had the opportunity of examining the brains after death, was the infrequency of hemiplegic or sensory symptoms on the opposite side of the body. When one saw the side of the pons deeply excavated, one was almost forced to the conclusion that the function of the pyramidal tract and perhaps of the lateral fillet, must have been seriously involved, yet the proportion of cases in which there was weakness, spasticity, changes in the reflex or any form of anæsthesia of the opposite side was very small. He had not seen any cases confirmed by autopsy which did not present sufficient symptoms during life to justify a definite diagnosis. In every case which had come under his observation there had been obvious

symptoms of cerebellar disturbance. Those symptoms differed in some respects from the symptoms due to lesions of the cerebellum itself, probably because they were mainly a result of compression of the middle cerebellar peduncle rather than of involvement of the cerebellum. A striking feature was that there was often in these cases very little disturbance of tone. It was perhaps presumptuous on his part to refer to the surgical treatment, but so many of his cases had passed through the hands of surgeons that he had had some experience in the matter. He had seen one case recover only after gross removal of the tumour, a man upon whom Sir Victor Horsley operated many years ago, but though he lived for several years he was seriously crippled. The danger seemed to be that total removal necessarily meant a disturbance of the vascular supply on the same side of the pons and medulla: the man to whom he referred had, after the operation, the characteristic symptoms of softening in the lateral side of the pons. He saw a few other cases which had survived operation for a week or so after total removal of the tumour, and all showed evidence of acute bulbar involvement. The statement made by Mr. Trotter and Sir Charles Ballance as to the inefficacy of simple decompression seemed to be borne out by every one who had had experience of these cases. But here too there were exceptions. One of the most brilliant results he had seen in the matter of surgical intervention was in the case of a man he saw with Mr. Percy Sargent three years ago. Owing to difficulties during the operation, the scalp was at once sewn up after the tumour had been exposed. The patient had now lost most of his symptoms, and though he was a professional man to whom co-ordination was important, he was going about and leading an active life. He particularly wished to know whether in the experience of otologists there was complete nerve deafness in all cases of auditory nerve tumour which had reached the clinical stage at which a diagnosis could be made.

Mr. SYDNEY SCOTT said this subject interested him from the otological standpoint. He had himself seen forty-five cases in which tumour involved the auditory nerve. He had sections of the labyrinth showing that the growth often invaded the cochlea, and if an attempt had been made to remove the tumour in such cases it would have been necessary to remove the labyrinth to make sure of getting rid of the whole tumour.

Dr. Gordon Holmes had suggested that there was a period in which the diagnosis could be made before deafness was complete: but in investigating these cases he (Mr. Scott) had never felt justified in diagnosing such a tumour until there was almost absolute deafness. The difficulty was to make sure of this, because the good ear was so liable to hear the loud tones used in testing the deaf ear. The use of the Bárány noise apparatus was limited: it excluded tones of low pitch, but it did not exclude high tones; in cases of intracranial tumour, when optic neuritis of high degree was present, there was practically always diminished bone conduction, and some loss for the highest tones. Great loss to low tones was much more consistent with the presence of auditory nerve tumours than was taught in many text-books on otology. Loss to low tones was more important as a sign of auditory nerve tumour than loss to high tones alone, of which latter there were many causes. As a rule, the typical signs were great and progressing loss of low tone appreciation, with diminished bone conduction, and some bilateral loss to high tones. Preservation of low tone appreciation was unusual, but he found this in a man who had a tumour originating not in the auditory nerve, but as a pre-pontine intradural cholesteatoma. The mass spread to one side pressing the pons and the auditory nerve backwards. The patient, an intelligent young man, appeared to be very deaf for conversation; he said he could hear the sound of the voice but could not analyse the sounds. Mr. Scott ascertained that the patient could hear, on both sides, a fork of sixteen double vibrations per second, but tones above 440 vibrations were to him an utter blank on one side, while on the other he could hear to about 5,000 vibrations per second. As the result of Magnus's investigations, Dr. Walshe foresaw that our customary vestibular tests were incomplete. Still, the caloric rotation and galvanic tests, together with the hearing tests, sometimes took three hours for a single patient; the testing could not be hurried or completed at one sitting, and great deliberation had to be exercised or the results were unreliable. Sir Charles Ballance had referred to the possible confusion of frontal and cerebellar lesions. This should be impossible. One

patient, on whom it was proposed to operate for frontal tumour, on the left side, was next morning sent to Mr. Scott to be tested, owing to deafness in the right ear. He concluded there was a lesion of the right auditory nerve, because he found the vestibular tests were negative, as in cases of labyrinthine ablation, and there was complete absence of the galvanic test on the affected side. The physician was informed, and the diagnosis altered. A right extracerebellar tumour was found. Another patient with bilateral auditory nerve tumours bore a transephalic galvanic current of 20 ma. without any sign of vertigo or forced movements of eyes, head, &c. A 10 or 15 ma. current in the ordinary person caused forced movements when applied to the normal ear. He had described elsewhere the method suitable for the galvanic test. One electrode was placed on the wrist of the side to be examined, the anode being placed on the ear on the same side, so that the current passed through the vestibular nerve on the side being examined. Whenever possible it was preferable that the patient be tested standing, as weaker currents were required. The galvanic reaction had been positive on the normal side, negative on the affected side in all cases in which the patient had been proved to have a lesion affecting the nerve trunk. There should no longer be any such mistake as diagnosing a frontal tumour for an extracerebellar tumour or vice versa. Mr. Scott owed his experience of auditory nerve tumours to his colleagues, Dr. Gordon Holmes and the physicians at the National Hospital, who had referred so many of their cases to him for special examination. He had used the Bezold-Edelmann tone series and the Edelmann-Galton whistle or monochord for all hearing tests.

Sir JAMES DUNDAS-GRANT asked whether members had been able to confirm the diagnostic point described by Jones in his work on equilibrium and vertigo as an indication of auditory nerve tumour. Jones said that on the same side as the tumour all the caloric tests were negative, i.e., for the vertical as well as for the horizontal canals, and that on the sound side the reflex from the horizontal canal was positive, but for the vertical canals negative. He (Sir James) had shown before this Section a specimen from a case in which these signs had been quite distinct: a tumour was found distending the internal auditory meatus. The supposition was that the strands from the vertical canal ran upwards near the middle line for some distance farther than those which crossed to the horizontal canal; therefore the tumour transmitted the pressure through the brain to the strands going to the vertical canals of the sound side; these were so close to the middle line as to be involved, while the strands to the horizontal canal escaped. Much of the testing could be carried out by means of the "cold air" apparatus with little disturbance of the patient; he could rest in bed with his head on a low pillow for the horizontal canals, and could sit up for the vertical canals. The galvanic tests were of enormous value, especially when those for the labyrinth were negative. In the case of an auditory nerve tumour, the galvanic test became negative as well as the caloric.

Dr. WALSHE (in reply) said he had expressed the opinion that we were apt to have unduly precise notions as to the typical clinical picture of eighth nerve tumours, and it was not surprising, therefore, that in the course of his exceptional experience Dr. Holmes had observed cases not presenting the usual clinical picture. Nevertheless, he believed that the symptom-complex he had described was the most typical, though there was really no disagreement between Dr. Holmes and himself on the general question.

Section of Otology.

President—Sir CHARLES A. BALLANCE, K.C.M.G., C.B., M.V.O.

The Morbid Anatomy and Drainage of Otitic Meningitis.

By E. D. D. DAVIS, F.R.C.S.

THESE observations which I shall describe were limited to otitic meningitis of the base of the skull and were made from thirteen autopsies, of which there are full notes, and from a number of specimens seen in the museums of the London medical schools. Otitic meningitis of the middle fossa and cortex of the brain was much less frequent than that of the posterior fossa and was usually secondary to a temporo-sphenoidal abscess, or a localized meningitis occurred. In the autopsies mentioned above, thick gelatinous pus was invariably found in the cisterna interpeduncularis from which it extended into the subarachnoid space taking the path of the large cerebral vessels. In front, the pus spread over the optic chiasma and along the anterior cerebral artery to the longitudinal fissure, forming a collection of pus above the corpus callosum. In the later stages the pus travelled on each side along the middle cerebral arteries and Sylvian fissures to creep up the cerebral cortex. More frequently suppuration extended backwards by the posterior cerebral arteries and around the crura to form an abscess between the tentorium and the superior surface of the cerebellum.

The cisterna magna, or cisterna cerebello-medullaris, was free from pus in some cases, but in the late stages and when suppuration was advanced, pus extended backwards, surrounding the medulla and reaching the cisterna magna by that route. Collections of pus were present (1) at the internal auditory meatus and on the posterior surface of the petrous bone immediately in front of the lateral sinus; (2) in the cisterna interpeduncularis; (3) between the tentorium and the superior surface of the cerebellum; (4) in the longitudinal fissure above the corpus callosum.

The path of infection in seven cases was traced through the fenestra ovalis to the labyrinth and to the internal auditory meatus, then to the under surface of the pons and the cisterna interpeduncularis.

In one case there was thrombosis of the lateral sinus, with a collection of pus round the sinus and covering the under surface and posterior aspect of the cerebellar hemisphere.

In one other case, a collection of pus was found in the aqueduct of the vestibule and it appeared to reach that position along the saccus endolymphaticus. Fistulae of the external semicircular canal or of the promontory were not discovered. In the remaining four cases the path of infection was untraceable and may have been part of a blood infection or septicæmia.

In order to demonstrate the paths of infection, a series of experiments were carried out on the cadaver, in which the subarachnoid space was injected

[April 20, 1923.]

with a solution of methylene blue run by a funnel and tube through the internal ear and internal auditory meatus, and at a subsequent examination, when the skull cap and brain were removed, the methylene blue was in every case found in the cisterna interpeduncularis extending to the optic chiasma and backwards around the crura to the interval between the tentorium and the cerebellum, but no methylene blue was present in the cisterna magna or in the ventricles. Similarly when the injection was made through the dura mater immediately in front of the lateral sinus (to resemble infection by sinus phlebitis) the pigment was limited to the under surface and posterior aspect of the cerebellum extending to the incisura posterior and to the cisterna magna; none was seen in the other cisternæ.

These experiments were adapted to test the various routes of drainage and 30 c.c. of methylene blue were injected through the internal auditory meatus to the cisterna interpeduncularis; practically all the 30 c.c. of pigment were recovered by aspiration through the same channel with a coarse needle and syringe. When a trocar and cannula were inserted through the atlanto-occipital ligament and foramen magnum, to tap the cisterna magna, no pigment drained away.

Again, when 30 c.c. of blue were injected in front of the lateral sinus, only 2 c.c. were recovered by aspiration, but occipito-atlantal puncture of the cisterna magna produced drops of the blue fluid.

Lastly, during and after the injection of methylene blue through the internal auditory meatus, a cannula was inserted in the spinal theca in the lumbar region and it was surprising to notice that no methylene blue came through, not even when 10 c.c. of cerebro-spinal fluid were drawn off and considerable suction was employed by a syringe. Also, later examination of the brain showed no appreciable diffusion of the pigment beyond the cisterna interpeduncularis. These facts point to two conclusions; first, that repeated lumbar puncture during life does not increase the area of suppuration, and, secondly, that lumbar puncture is inefficient as a method of drainage.

Although these experiments are artificial and it cannot be claimed that they reproduce exactly the phenomena of meningitis during life, yet when they are considered in conjunction with the morbid anatomy, the following conclusions may be justified:—

(1) When meningitis is so far advanced that the cisterna interpeduncularis contains pus, or in the still later stage when there is pus in the cisterna magna, efficient drainage is obviously difficult. Therefore the prospect of saving patients in cases of meningitis is very much improved by the earliest possible diagnosis, when the suppuration is limited to the labyrinth or lateral sinus area, or, at the latest, when the suppuration is localized to the area around the internal auditory meatus and posterior surface of the petrous bone. When the infection arises from sinus phlebitis the prognosis is probably better than when it spreads through the internal ear, because in the first case the suppuration is more usually localized to the posterior fossa and may not extend to the cisterna interpeduncularis.

(2) When meningitis arises from infection of the labyrinth, drainage and suction by a syringe through the internal auditory meatus are least inefficient and most likely to be successful.

(3) If meningitis arises from sinus phlebitis, drainage and suction should be effected both behind and in front of the lateral sinus, and in this type of case occipito-atlantal puncture may be useful.

(4) Lumbar puncture is not a satisfactory method of drainage for pus, and

though it is recognized as a valuable aid to treatment, Weed, Wegeforth, Ayer, and Felton, of the Rockefeller Institute, have conclusively shown by a series of experiments and controls that if a bacteræmia or septicæmia is produced by the intravenous inoculation of the *Bacillus lactis aerogenes* or other organisms, in animals, a fatal meningitis is established by lumbar puncture or by the withdrawal of cerebro-spinal fluid, whereas meningitis does not occur in those septicæmic animals in which cerebro-spinal fluid is not withdrawn. The importance of these experiments is accentuated by the fact that meningitis is sometimes due to a blood infection and the above observers have proved that animals dying of meningitis seldom fail to show organisms in the blood-vessels either microscopically or culturally. Blood infection is almost invariably present if meningitis has existed for eighteen hours and septicæmia probably plays an important part in the death of the animal suffering from meningitis. Further experiments strongly indicated that facilitation of the involvement of the meninges from the blood stream after removal of cerebro-spinal fluid is dependent upon the reduction of the pressure of the fluid or on other general intracranial reaction, even if such reduction is of very short duration, and is not related to the injury produced by the needle. The immediate replacement of the cerebro-spinal fluid by Ringer's solution does not prevent the onset of meningitis, and it is thought that fluid leaks into the tissues through the puncture; this leakage could be diminished by using a fine needle, and incidentally prevent "lumbar puncture headaches."

This excellent work from the Rockefeller Institute supports Jenkins' statement that only the smallest quantity of cerebro-spinal fluid—sufficient, i.e., for diagnosis—should be withdrawn by lumbar puncture.

REFERENCE.

WEED, WEGEFORTH, AYER, and FELTON, "A Study of Experimental Meningitis," *Monographs of the Rockefeller Institute for Medical Research*, No. 12, March 25, 1920.

DISCUSSION.

Sir CHARLES BALLANCE (President) said that Mr. Davis's paper was the kind of communication from which much could be gleaned. There was always an inclination to publish cases which were successful, but much more could be learned from a post-mortem examination of failures.

Mr. SOMERVILLE HASTINGS asked what Mr. Davis considered to be the clinical indications for occipito-atlantal puncture. He, himself, had never done it, and he would be glad to hear what was its technique.

Mr. J. F. O'MALLEY asked what symptoms would induce Mr. Davis to open directly through the labyrinth, or to put in a drain in the wall of the antrum in front of the lateral sinus. Or did he think it an advantage to do both at the same time?

Mr. T. B. LAYTON said he was interested to hear that there was some danger in doing lumbar puncture. He used to imagine that it was a small operation which could not do any harm, and therefore in any case of doubt should be carried out to help the diagnosis. Mr. Davis's paper seemed to show that although it might be undertaken in order to obtain further evidence when necessary, there were cases in which the question should be carefully considered. He asked whether, in either of Mr. Davis's cases which were successful, organisms were found in the cerebro-spinal fluid. Information was needed as to what was the kind of case in which drainage of the cerebro-spinal cavity in the hope of saving the patient's life was advisable, as distinct from cases in which removal of the infected bone in relation with the dura was all that was necessary.

Sir CHARLES BALLANCE (President) said this was an extremely important subject, as the treatment of these cases was a matter of life and death. Mr. Layton had referred to lumbar puncture; he (the President) had known sudden death occur from lumbar puncture, which should never be done without careful consideration in certain cases, because it produced a change in the position of the cerebellum, and affected the vital centres in the medulla. Another important question mentioned by Mr. Layton was as to whether organisms were present or not. Frequently, in past years, he (the President) had done lumbar puncture and had found turbid fluid; and he thought there was no question that organisms had been present, but the reports frequently stated that the milky condition of the fluid was due simply to the cells, since no organisms could be cultivated from it. If no organisms were in the fluid, and the proper treatment were carried out, the patient would, almost certainly, recover. But if organisms were present, the difficulty of saving the patient was likely to be very great indeed. In the fulminating cases of meningitis the patient might die within thirty hours of the onset. In such cases the outlook was at present hopeless, though the attempt to save life should not be abandoned. He had been interested in Mr. Davis's remark about methylene-blue injections into the subarachnoid space. During the war, he (the President) had found that washing out the subarachnoid space in cases of meningitis from the lateral ventricle, putting the puncture-needle into the lumbar theca, and colouring the salt solution with methylene-blue caused the blue fluid to come out of the lumbar puncture cannula in twenty-five seconds. It was therefore extraordinary to hear now that in a dead body the fluid obtained by lumbar puncture was not coloured by the methylene-blue, which had been injected definitely into the subarachnoid space of the cranial cavity. There was no doubt, as Mr. Davis had said, that the only possible treatment of these cases was by operation, and that the surgeon should be able to operate within a very short time of what he believed to be the onset of meningitis. That, however, was very difficult to ensure, because the usual experience was that patients in these cases were not seen until they were practically moribund. At first the symptoms were slight, but later unconsciousness supervened, and the surgeon was called in in the hope of saving life when that had become impossible. The course of the cerebro-spinal fluid in health was well known, and in the living body the fluid carried coloured material rapidly in various directions. In Golla's experiments on the cat, injections into the lumbar theca spread rapidly almost everywhere. It was known, also, that the fluid was secreted by the choroid plexuses, and came out of the ventricles through the openings in the roof of the fourth ventricle, spreading into the great cisterna at the back of the under surface of the cerebellum, and then travelling forward into the interpeduncular space and various cisternæ at the base of the brain. It also flowed down the spinal canal. It then spread, as Mr. Davis described, over the surface of the hemispheres in the direction of the superior longitudinal sinus, especially in the region of the Sylvian fissure. Probably some members had seen the spread of this rapidly fulminating subarachnoid meningitis; he had watched it on the operating table; the greenish pus underneath the subarachnoid membrane spreading from sulcus to sulcus between the convolutions. Everyone wished to know how this process could be arrested. He (the President) disagreed with Mr. Davis on one point—namely, that occipito-atlantal puncture was a surgical operation which should replace drainage. To his (Sir Charles Ballance's) mind, such a view was reactionary, in the sense that none of them would now think of treating an acute abscess in any other position by means of puncture with trochar and cannula. Occipito-atlantal puncture was easy to do, and there was no danger in it, unless the operator was in doubt of his ability to perform it. But he felt strongly, from what he had seen post mortem in meningitis cases, that no puncture was capable of arresting the process which was spreading in the subarachnoid space. If the condition was due to infective organisms, he (Sir Charles) thought the only possible surgical way of dealing with it was by Haynes' method. If the stream of fluid could only be directed out through a surgical opening in the great cerebellar cistern, its spread in the other directions might possibly be arrested. He had always been much impressed with the fact that meningitis and other infections in the skull were greatly enforced by increased pressure. If the pressure of the fluid in the subarachnoid space were reduced, he believed there would be a good

chance of preventing the further spread of the infection. If, then, by opening the great cerebellar cistern, one could diminish the pressure of the fluid in the subarachnoid space, and start the current of fluid in a new direction, the hope of stopping the spread of inflammation over the brain surface might be entertained. But as matters stood at present one could not expect to cure all these cases. He thought, however, that the condition should be treated as an acute suppuration in any other part of the body was treated.

Mr. E. D. D. DAVIS (in reply) said that the method adopted for occipito-atlantal puncture was as follows: The head must be slightly flexed and the needle inserted at a point in the mid-line of the neck immediately above the spine of the axis and directed in the mid-line to the fronto-nasal suture. The needle would be felt to go through the occipito-atlantal ligament with a jerk; then it was necessary to move carefully and to see that the direction to the fronto-nasal suture was correct. The operation could be done without an anæsthetic. It was now frequently performed without mishap. He (Mr. Davis) thought that occipito-atlantal puncture might be sometimes more useful than Haynes' operation. He considered that the cisterna magna contained pus only in the very late stages of meningitis, hence Haynes' operation was generally unsuccessful at so late a stage of meningitis. Occipito-atlantal puncture was valuable in cases in which the patients were more or less moribund, and in which the surgeon felt impelled to do something. In reply to Mr. O'Malley's question as to when to drain through the labyrinth, Mr. Davis said that if it was decided that the meningitis had arisen from labyrinthitis—that is, when the signs of labyrinthitis had preceded the meningitis—drainage should be established through the internal auditory meatus and the labyrinth. He admitted that it was sometimes difficult to decide on labyrinthine drainage in suspected meningitis when the diagnosis was uncertain, as it required some courage to go through the labyrinth and destroy the internal ear. But if there were indications that meningitis had reached the skull through the labyrinth, then drainage through the internal auditory meatus must be done. When meningitis arose from lateral sinus thrombosis or infection, he (the speaker) left the labyrinth alone and drained in front and behind the lateral sinus. One of the two cases he had mentioned, was that of a patient whom he had shown at a former meeting of this Section.¹ She had all the symptoms of meningitis—headache, vomiting, drowsiness, and the cerebro-spinal fluid gave pneumococcus on culture. He had operated practically as soon as he saw her. There was a large extradural abscess between the posterior surface of the petrous bone and the dura, arising from a perisinus abscess which tracked forward between the dura mater and the petrous bone; he had drained this and had left the wound open so that later if there was no improvement, he could drain through the dura mater. The patient had done well for a few days, but then she had become more drowsy and vomiting had recommenced. He (Mr. Davis) therefore drained the subarachnoid space in front and behind the lateral sinus and the patient recovered. He thought it was a localized meningitis of the posterior fossa. In the other case, the patient had had an acute attack of suppuration sometime after a radical mastoid operation performed elsewhere. There had been a temperature of 103° F. and signs of meningitis, therefore the mastoid had been opened up again and the posterior fossa drained behind the lateral sinus. The cerebro-spinal fluid had contained pneumococci. The patient had done well.

The cases recorded by Mr. Lawson Whale² and Mr. Martyn³ appeared to have been similar to these two cases.

Case of Complete Deafness dating from a Fall.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

PATIENT, male, aged 48, became completely deaf about three and a half years ago, after a fall of 6 ft. on to his feet, not striking his head but suffering pain in his neck several days later. He felt ill and had to be helped

¹ *Proceedings*, 1922, xv (Sect. Otol.), p. 44.

² *Brit. Med. Journ.*, February 24, 1923, p. 323.

³ *Lancet*, March 10, 1923, p. 485.

home. During the night he had diarrhoea and vomiting and next morning was quite deaf. He was told that he shouted when speaking. Dr. Dan McKenzie referred him to me at the West End Hospital for Nervous Diseases, for consideration of neurological possibilities. My colleague, Dr. Carlill, kindly took the greatest interest in the case and gave it the benefit of treatment by gross suggestion. The result was negative and in favour of an organic lesion. The anomalous character of the data has rendered the diagnosis difficult.

On examination in May, 1922, when he appeared to be totally deaf for sounds of any kind, and there being then no voice-raising with noise-machines in the ears, the click of the large "distinette" produced a palpebral reflex. There was marked spontaneous nystagmus to the left and slight past-pointing to the right; the voice was monotonous. Lip-reading had not been spontaneously acquired. Rotation to the right with external canals horizontal produced no increase of nystagmus to the left. Rotation to the left produced active nystagmus to the right but no past-pointing. Cold air to the right external canal caused nystagmus in thirty-five seconds. Cold air to the left external canal produced no nystagmus after sixty seconds. Cold air to the right vertical canal caused slight rotary nystagmus to the left. Cold air to the left vertical canal obtained no response. Past-pointing and giddiness were not induced by any of these tests. Galvanism (tested in November) on either side with 15 ma. excited nystagmus in the direction of the cathode.

These tests suggest a lesion—concussion—of both labyrinths, the auditory nerve trunk being unimpaired. The left labyrinth appeared to be the most affected, yet the spontaneous nystagmus was to the left side.

The symptoms varied slightly from time to time, so as to suggest a functional element in the case, as also did the diminution of the pharyngeal reflex.

Case of Deafness greatly increased after a Fall.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

PATIENT, female, aged 29, had been dull of hearing for nine or ten years, but became extremely deaf ten months ago after a fall of 25 ft., in which her head was knocked and after which she was unconscious for seven or eight days. When first seen in September, 1922, she heard whispered voice at only 12 in. on the right side and 2 in. on the left, reduced by inflation to 6 in. and $\frac{1}{2}$ in. respectively. On the other hand the ordinary voice heard at 14 in. and 4 in. was after inflation heard at 3 ft. and 2 ft. respectively. The hearing for Gradenigo's tuning-fork (64 d.v.) was reduced considerably, viz., to 3 and 2 instead of 6. Bonnier's test (tuning-fork 128 d.v. heard through condyles of femur) was positive. Paracusis Willisii was present. Bone-conduction on the mastoid was normal and Rinne was markedly negative. Weber positive. Galton's whistle normal (1'1). The Eustachian tubes were slightly narrow. The features were essentially those of otosclerosis, but there was no family tendency and there was no initial tinnitus.

She was treated by occasional inflation and frequent self-inflation while at the same time she took, with small doses of *ignatia amara*, 5 minims of liq. hydrarg. perchlor., thrice daily. This last remedy was given because of Erichsen's use of it in the treatment of "railway spine" concussion, to which the traumatic condition in the present case seemed analogous. In October she reported herself as feeling better in herself and freer from tinnitus. At present she seems to have reached a stationary condition and states that she hears as well as she did before the accident.

The nature of the change induced by the accident is to some extent a matter of conjecture. It may have been either functional or organic. The history supplies sufficient ground for either or both. Opinions will be welcome.

Case of Long-standing Deafness attributable to Falls on the Head; Improvement.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

PATIENT, male, aged 25, with deafness of fifteen years' duration, gave a history of a fall when 10 years old, which was followed by giddiness and commencing deafness.

First seen, October, 1921. Whispered voice heard at 5 in. right and 10 in. left. Improvement on inflation to 12 in. right, but none on the left. C 64 d.v. not heard. Tuning-fork by bone-conduction on vertex equal both sides and on mastoids slightly increased; Rinne negative. C 128 d.v. heard by femur condyle (Bonnier). Galton's whistle reduced to mark 4 on the right and 3·2 on the left. Eustachian tubes narrow. Gellé negative right, positive left. Rombergism to right, nystagmus to left. Cold air labyrinth test, nystagmus delayed; past-pointing normal.

The features are those of a nerve-deafness, probably cochlear, with concomitant chronic Eustachian catarrh.

The patient is well-built (up till recently played football), cheerful in disposition, but with a somewhat monotonous voice. There is a slight asymmetry of the face. His deafness was so considerable that he was unable to use the telephone, and his livelihood was in jeopardy. He was also so unsteady and apprehensive that he had to be accompanied by his brother.

He was first treated by means of inflation (catheter, Politzer, &c.), and in November reported improvement. His Eustachian tubes appeared to be more pervious. Glycerophosphates and paraffin were ordered.

In January, 1922, whisper was on the right side 2 ft., improved by inflation to 5 ft., and on the left 2½ ft. improved to 7 ft. Galton (left) 4·6. Induction of nystagmus still delayed.

In view of the traumatic factor small doses of liq. hydrarg. perchlor. were ordered tentatively. In March his hearing for the whisper was on the right side 3 ft. improved to 5 ft., and on the left 5 ft. improved to 20 ft. In December the whisper on the right side was at 2 ft., improved to 4 ft., and on the left 18 ft., improved to 18½ ft.

In February of this year he had an attack of influenza accompanied by retrogression in his hearing-power, but I hope to find it is only temporary. The improvement in general alertness and usefulness is unmistakable in what at first appeared a most discouraging case.

He is to be examined by an ophthalmologist on account of defect of vision in his right eye; the result may throw fresh light on his case.

DISCUSSION.

Sir CHARLES BALLANCE (President) said that no surgeon objected to perchloride of mercury being given to any patient, and in these particular instances the drug appeared to have been useful.

Mr. F. J. CLEMINSON said he had recently seen a man aged 29 who, while crossing the Channel in a gale, was quietly sleeping in a saloon when a slab of marble,

50 Dundas-Grant : *Long-standing Deafness attributable to Falls*

weighing 1 cwt., fell on to his head, causing not only profound traumatic neurasthenia, but considerable deafness in both ears, especially in the right. That had occurred twelve months ago, and the patient had had treatment for some time, and had eventually brought a suit against the railway company: it was in that connexion that he (Mr. Cleminson) had seen him. The condition he had found was a marked deafness to both the voice and acoumeter on the right side, and to a less degree on the left. A 30-in. watch had been heard at 8 in. by the right ear, and at 16 in. by the left. When the ears were syringed with cold water, there had been a delayed response on the right, and the tests with the tuning-fork, &c., had pointed to internal ear deafness on that side. He (Mr. Cleminson) was unable to visualize the lesion which occurred in such cases; perhaps Sir James Dundas-Grant could suggest what happened. He had only seen the patient twice, at an interval, and he did not know what had happened to him since the last occasion.

Mr. J. F. O'MALLEY said these cases often presented difficult problems, e.g., as to why a patient should be seized with sudden deafness, apart from injury. A fortnight ago, a woman engaged in selling newspapers in the street came to see him at the clinic, accompanied by a friend who stated that the patient had been well until four days previously, then deafness had suddenly occurred, and she had been unable to hear anything since. There was no history of an accident or of vertigo which would suggest a sudden lesion of the labyrinth, rupture of a blood-vessel, or an invasion of a syphilitic nature. He (Mr. O'Malley) had had the patient's vestibular reaction tested, and there was a good response. In the case of a deaf and aphonic soldier whom he had seen during the war, when, after producing a vigorous vestibular reaction with a cold application, he had applied a speaking-tube, and shouted down it, he had obtained a prompt answer. But he had obtained no answer from the patient in the case to which he was now referring. Sir James Dundas-Grant's second case seemed to him (Mr. O'Malley) to be one of otosclerosis; but it would be difficult to explain how the fall had increased the deafness, as the tests showed that bone conduction was good all the time. He thought there was a large element of functional disturbance in that case. In the third case he had noticed the peculiar voice—a manner of speaking associated with deafness in early childhood; the patient learning to speak better as he grew older. He (Mr. O'Malley) had found that kind of voice in cases in which the patient had had an attack of cerebro-spinal meningitis in early life, but in which the hearing had not been completely obliterated, and later difficulty had occurred in controlling the pitch of the voice.

Mr. E. D. D. DAVIS said that during the war he had seen several cases of injury, in the region of the mastoid process, in which the deafness was permanent. In the commoner concussion case, such as that from a motor accident, the hearing really did improve. If the hearing did not improve within six weeks after the accident, he (Mr. Davis) considered that the deafness was likely to be permanent. He asked whether one was justified in that conclusion. It was his experience that by deafness occurring after a shell or bullet wound in the region of the mastoid process or posterior fossa of the skull, without any direct injury to the ear itself, the loss of hearing was usually permanent.

Sir JAMES DUNDAS-GRANT (in reply) said it was useful to remember the old observation that when the handle of a broom was struck on the ground, the handle was driven more deeply into the broom-head. A fall on the feet sometimes caused fracture of the base of the skull, and there might be real damage to the delicate structures in the internal ear even in the absence of skull fracture. He (Sir James Dundas-Grant) believed that these injuries were often accompanied by fractures which healed and left no trace, even on post-mortem examination years afterwards. He thought that in some cases of injury of the head, at some distance from the ear, there was a fracture through the internal ear, or at all events such a shaking up that the internal ear was rendered insensitive, and it might be completely disorganized by hæmorrhage either into the labyrinth or the internal auditory meatus. He agreed with Mr. Davis that if no improvement took place within six weeks, there had been something more than a mere concussion and that destruction had occurred. But his own experience in the third case showed that it was unwise to despair too soon.

Section of Otology.

President—Sir CHARLES A. BALLANCE, K.C.M.G., C.B., M.V.O., M.S.

An Instrument for Assisting the Deaf.

Exhibited by W. M. MOLLISON, M.Ch.

THE instrument now exhibited is known as the Marconi Otophone. It consists of electrical amplifying circuits in a polished cabinet to which are connected a sensitive microphone and a pair of head telephones.

By means of three switches the amount of current supplied to the valves can be regulated and the degree of speech amplification can be adjusted to five different values.

The Marconi Company does not intend to market this instrument through the usual trade channels, but proposes to supply it only through the recommendation of the medical profession. A full description can be obtained from the Marconi Research Department.

DISCUSSION.

Mr. MOLLISON said that he had only had a week's experience of the instrument, but already he had found it useful in helping really deaf people to hear.

Sir CHARLES BALLANCE (President) said that this was a remarkable instrument. In course of time it would no doubt be greatly improved; it would be a great advantage if outside noises could be suppressed. A great obstacle to its use at present was its weight (16 lb.).

Mr. SOMERVILLE HASTINGS asked whether the instrument had been tried in the case of congenital deaf-mutes. When he (the speaker) heard the confusing extraneous sounds, he wondered whether it would be possible to attach a funnel to the microphone which could be directed towards the sound one desired to hear.

Epidemic Cerebro-spinal Meningitis associated with Acute Suppuration of the Middle Ear.

By FREDERICK SYDENHAM, F.R.C.S., and DAN MCKENZIE, M.D.

THE patient, a girl, aged 16, was admitted to hospital under Mr. Sydenham's care in February, 1922. She was semi-comatose and, to all appearance, totally deaf; the temperature was 103'5° F.; there was discharge from both ears with pain in both mastoids, and œdema on one side. The Heath operation was performed on both sides. A little pus was found in the antrum, but nothing further, and not enough in Mr. Sydenham's opinion to account for the grave

symptoms. Both lateral sinuses were exposed and explored. The patient made a good recovery but remained deaf.

On December 7, 1922, she came under the care of Dr. McKenzie, on account of the deafness. On examination she was found to hear all the tuning-forks, but only for a few seconds, both by air and bone. A shout was heard as a loud noise only. The caloric tests were negative.

The case is reported as one of (probable) epidemic meningitis associated with suppuration of the middle ear, to draw attention to the possibility of the meningeal infection being due to that of the middle ear (see *Journal of Laryngology*, 1922).

Epileptiform Seizures subsequent to Operation for Temporo-Sphenoidal Abscess.

By DAN MCKENZIE, M.D.

THE patient, a woman aged 35, was operated on for temporo-sphenoidal abscess six years ago, the pus being successfully evacuated through the antro-tympanic roof. The abscess was chronic, symptoms having existed for about six months, and the walls of the cavity were thick and tough. The first suspicious seizure occurred a year after the operation, and they are becoming more frequent as time goes on. But they have never amounted to more than one in two or three months. The diagnosis of probable epilepsy has been made by Dr. C. O. Hawthorne.

DISCUSSION.

Sir CHARLES BALLANCE (President) said that the second case seemed to be an unusual one, because the temporo-sphenoidal abscess was followed—a long time afterwards—by epilepsy; he (Sir Charles) did not remember that sequence in any of his own cases, though epilepsy following brain operations was not infrequent. In the first case, he supposed there must have been, as suggested, some meningitis in the posterior fossa, which involved the nerve, and consequently when the patient recovered she was deaf. He remembered one case of pterygo-maxillary abscess in a case which was complicated by some intracranial infection; he believed it was lateral sinus infection. In his experience successive operations were always unsatisfactory, and often ended fatally. Whenever it was possible, all that was needed should be done at one operation. In this case the cavernous sinus was thrombosed, and there had been no attempt to deal with that infective process. He asked whether there had been a recent operation on infective cavernous thrombosis, and if so, what had been the result. Had any member had a successful operation recently?

Sir JAMES DUNDAS-GRANT asked whether in the second case there was any localizing Jacksonian sign or any warning aura in connexion with the epilepsy.

Mr. E. MUSGRAVE WOODMAN said he had asked the patient whether she had any such warnings and she had told him that she suddenly fell down wherever she happened to be. This seemed to him significant, and suggested that the condition might be hysterical. It would be interesting to hear from members their views as to what treatment, operative or non-operative, would suit such a case, especially if the seizures were caused by a brain scar.

Dr. DAN MCKENZIE (in reply) said that he had since seen another case, in a patient who had undergone an operation twenty years ago. There had first been a brain abscess, which was operated upon by Mr. Marsh, of Birmingham; then a mastoid operation had been performed by Sir Charles Ballance. He (Dr. McKenzie) had not

been able to persuade the man to come to the meeting. The attacks seemed to be those of *petit mal* or automatism. The patient had never had regular epileptic convulsions. He (the speaker) did not know whether in the present patient it would be worth while to try and reopen the old area. Mr. Tilley had told him of a case—in his own practice—of epilepsy following brain abscess in which operation had been successful, i.e., there were no fits following it. The epilepsy in these cases might be idiopathic, and then operation would be useless. If the fits were due to a scar, conceivably operation would bring relief.

Sir CHARLES BALLANCE (President) referred to a patient who, before suffering from a left temporo-sphenoidal abscess, was very musical, but afterwards lost his musical sense and was unable to play the piano. With regard to operation in cases of *petit mal*, or so-called idiopathic epilepsy, he (the President) had seen many of these cases during and after the War; he thought the scars in the brain produced a definite vascular change in the cortex. When exposing the cortex, he had seen a blush come round the scar and the area then became white again. If the scar was adherent to the dura, and the dura adherent to the bone, the normal excursions of the brain were prevented, and in such a case, he thought, some benefit would be produced by freeing the dura and thus allowing the brain to move freely again. In some cases in which he had carried out this plan the epilepsy had ceased, but he did not think he had done so after operations on brain abscess caused by ear disease. A drug which he had used in war cases with remarkable effect was luminal; if a patient takes this drug continuously in small doses, the epilepsy may not return. To prevent the parts becoming adherent, Mr. Sargent used celluloid, which served the purpose very well as, like pure platinum, it was not acted on by the tissues and fluids of the body.

Otitic Pterygo-maxillary Abscess induced by Thrombophlebitis of the Jugular Bulb.

By DAN MCKENZIE, M.D.

A BOY, aged 8, was admitted to the Central London Throat and Ear Hospital with chronic suppuration of the left middle ear and a temperature of 102° F. Immediate radical mastoid. No relief to temperature. Two days later wound re-opened, and a small drop of pus on the floor of the external meatus was found to lead to an abscess in front of the bony meatus. This was opened and drained, and a counter opening made into the pharynx. Lateral sinus explored, no clot found. No relief to temperature. Two days later, internal jugular vein exposed and resected. Thrombosed down to lower border of thyroid cartilage. Vein cleared of clot with curette as high as jugular bulb. Next day signs of cavernous sinus thrombosis, followed two days later by death.

Post-mortem.—Specimen (on view) shows erosion of outer wall of jugular bulb leading to the site of abscess in the pterygo-maxillary fossa. No evidence of bone disease adjacent to the abscess.

This was evidently a case of thrombosis of the jugular bulb, which had extended down the vein into the neck, but not upwards into the lateral sinus. The thrombus in the bulb had broken down, and the pus had made its way through the wall of the bulb into the pterygo-maxillary fossa, tracking down towards the pharynx, and back over the floor of the external meatus. This development is rare. Going over the literature of pterygo-maxillary abscess in 1915, I could find only three cases in which the abscess was (doubtfully) traceable to thrombosis in the bulb.

Brain Abscess due to Otitic Infection ; Right Temporo-sphenoidal Abscess without Clinical Signs.

By T. H. JUST, F.R.C.S.

E. A. M., AGED 27, had had otorrhœa and deafness ; right side, since childhood. She had had no other symptoms until a few days before admission to St. Bartholomew's Hospital early last year, when she had had constant headache for three days, increasing in severity, and each morning she had vomited.

Condition on admission : Was able to walk slowly, but kept her head still, and there was some rigidity of the neck. The temperature was 101'2 F., pulse 120. The right tympanic membrane was obscured by epithelial débris, purulent discharge, and granulations. No abnormal signs were discovered during the examination of the central nervous system. The patient was right handed. There was no amnesia.

I decided to explore the right mastoid and expose the dura mater. Accordingly the radical operation was performed. The mastoid was acellular, but the roof of the antrum was carious and an extradural abscess was found immediately above the tegmen.

The dura mater beneath the temporo-sphenoidal lobe was covered with granulations ; no pulsation of the brain could be felt ; in the centre of the exposed dura mater a sinus was found leading into an abscess within the brain. This was opened up and drained by tubes. In draining the abscess I used the procedure which Harvey Cushing suggested in the treatment of war wounds. With a No. 10 catheter I washed out the cavity with saline solution and sucked out the material used in washing-out, repeating the process several times in the effort to get away all the débris from the abscess cavity ; I did that until the saline returned clear. The tube was kept in ten days ; at the end of which time there was no pus coming from the abscess cavity, therefore I left the tube out. In a recent case in which the wound is not yet healed, I used the same technique. In this case there was 1½ oz. of offensive pus in the temporo-sphenoidal lobe ; I took away the granulating dura which was covering the abscess, and washed and sucked it out with a syringe for ten minutes, I then put in a tube, and shifted the tube on the second day when no pus came out, but only a little straw-coloured fluid. On the third day I removed the tube altogether. In war wounds of the brain it was found better to keep the tract clean and not to use a tube, as the tube seemed to act as an irritant. Recovery was progressive and uneventful, and she left the hospital five weeks afterwards.

It is now nearly eighteen months since this operation, and nothing further has developed, though she occasionally attends the out-patient department complaining of vague "sensations down the right side of the body," which my neurological colleagues regard as "functional."

DISCUSSION.

Mr. G. J. JENKINS said that it was difficult to generalize as no two cases of these abscesses quite followed the same course. In one of his own cases he had drained with corrugated rubber tissue ; this tissue was much softer than a drainage tube, and as much or as little as required could be packed into the cavity. Sometimes a tube was

rendered useless by becoming blocked. His (the speaker's) inclination was to make the drainage from the abscess-cavity as free as possible, cutting away a good deal of the wall, according to the extent of the surrounding adhesions. This plan was likely to be much more effective than that of making a small hole and passing a tube through it.

Dr. W. KELSON asked whether the abscess was chronic or acute.

Sir JAMES DUNDAS-GRANT asked whether there was any objection to exploring the abscess cavity with the protected little finger, so as to ascertain whether the abscess wall was rigid, and whether there was, possibly, a second abscess feeling like an oyster through the wall. If the wall was not dense, the case would be suitable for removal of the tube, whereas if the wall was dense, it would be most unsuitable.

Sir CHARLES BALLANCE (President) said that, except in a few cases, he had never dealt with a brain abscess without drainage, the exceptions being those cases in which he could enucleate the abscess. In his experience, a brain abscess was very difficult to drain. The liquid tissue of the brain was the difficulty. If the abscess had no wall, then as soon as pus came out, the brain flowed around and filled up the cavity. If one took out the drainage tube to see how the case was getting on, it might be very difficult to replace it accurately. His (the speaker's) best cases had been those in which he had inserted the drainage tube immediately after opening the abscess, and left it there. The suggestion to use rubber tissue might be a good one. It was difficult to wash out brain abscesses; the size of them was not known, so that sometimes the attempt to wash them out was dangerous as well as difficult. Moreover, sometimes a brain abscess had diverticula, and when the main abscess was drained, the pus in these diverticula was untouched. Brain abscess cases certainly required much caution in their management.

Mr. JUST (in reply) said that he realized the difficulties and limitations in dealing with brain abscess, but he thought what he had done was worth trying. In reply to Dr. Kelson, he said that the abscess was sub-acute and that the walls were not very thick. He had used rubber tissue instead of a tube, and, on the whole, he preferred it.

Left Temporo-sphenoidal Abscess; Amnesia for Names of Objects.

By SYDNEY SCOTT, M.S.

F. N., AGED 10, was said to have had deafness in the left ear, for fully two years. She had been treated for otorrhœa until nine months ago, when the discharge ceased, after removal of tonsils and adenoids. There had been no other symptoms until about a month before admission. She attended the out-patient department complaining of pain in her left ear, and some otorrhœa, but her condition was not regarded as serious until she had some kind of convulsion, followed by another two days later, and was then admitted as an emergency case into St. Bartholomew's Hospital. The convulsions were said to have involved the right side of the face and right upper and lower extremities.

Condition on admission: Conscious; no hemiplegia. Temperature 101.6° F., pulse 96; tongue thickly furred. Some pus and débris in left external auditory meatus. No mastoid signs, but slight tenderness left side. The knee-jerks were unobtainable. The superficial abdominal reflexes were weakened on the right side. The child said she had no headache, but she liked to be left alone. On being questioned to test her memory for names of objects she soon made repeated mistakes, became confused, and exclaimed she "could not be bothered

56 Scott: *Left Temporo-sphenoidal Abscess; Cerebellar Abscess*

to think." It seemed probable that the child had localized encephalitis of the left temporo-sphenoidal lobe and I decided to open the mastoid at once and expose the brain. The antrum and aditus contained cholesteatoma and pus, and there was an extradural abscess in the middle cranial fossa, as in the preceding case. No pulsation of the exposed dura could be felt. When the dura was incised unmistakable adhesions to the arachnoid and pia mater were found, extending to the lateral surface of the temporo-sphenoidal lobe. No pus escaped through the dura, though the incision was carried into the cortex, nor did the brain tissue protrude.

The child's general condition did not become quite satisfactory, though neurological examinations revealed no fresh signs: she was allowed up and walked in the ward. The nurses said she seemed peevish and irritable and behaved like a spoilt child, and her mother said she was unlike her real self. In the third week she vomited unexpectedly two or three times. The "name amnesia" persisted, and it seemed justifiable to make a more determined exploration of the brain. I turned down a flap of the scalp and again exposed the dura mater. On making an incision through the adherent membranes into the brain I at once found the abscess. This was drained with rubber tubes, and the child's condition, temperament and memory rapidly improved forthwith.

It is now one and a quarter years since this operation, and the child is now particularly bright and well, though the period of convalescence cannot be said to have been free from anxiety. Last summer she had more convulsions after a minor plastic operation, and terrifying dreams frequently disturbed her, night after night. No further operation on the brain has been necessary. The operation cavity closed down and the meatus became stenosed. At present a post-auricular sinus persists. Attempts to re-establish a meatus and to close the sinus have been deferred until the child's condition justifies the procedure.

The observation of amnesia for names of objects, the occurrence of vomiting and absence of headache, were the leading clinical features of her case. The presence of the extradural abscess and of adhesions between the dura and the brain indicated the path of infection. Recovery was, no doubt, greatly favoured by the dural adhesions.

Cerebellar Abscess; Sudden Coma and Apnoea; Recovery after Operation during Artificial Respiration.

By SYDNEY SCOTT, M.S.

W. C. J., A YOUNG man, aged 17, was sent to London by his doctor, suffering from severe headache and chronic otorrhoea. He was admitted to St. Bartholomew's Hospital about 2 a.m., and in spite of his pain he could then walk and stand, and showed no discernible physical signs apart from discharge from the right ear. The house-surgeon withdrew some clear cerebro-spinal fluid, obviously under pressure, and arrangements were made to operate on the ear at 2 p.m. the same day. The patient became drowsy shortly before the time for operation, and had become quite unconscious when I first saw him in the operating theatre. Apart from confirming the evidence of middle-ear suppuration, and finding that the patient's upper extremities remained elevated when raised and left free (cataleptic state), no other observations could be made. An

anæsthetic was given, though this hardly seemed necessary, and the operation on the mastoid was begun, but before the antrum had been opened the patient had ceased to breathe. Artificial respiration was applied, and the operation stopped, but breathing remained suspended while the dura mater of middle and posterior cranial fossæ were exposed. The tension of the dura mater was much higher in the posterior fossa, and no pulsation could be felt. No extradural abscess was seen. It was not possible, under the circumstances, to see if any path of infection could be followed up, and the dura mater was incised posteriorly to the sigmoid sinus, several radiating incisions through the dura mater covering the lateral and inferior surface of the cerebellum. Here no adhesions were encountered, the cerebellum prolapsed freely, expanding over the edges of the cut dura mater. In the cerebellum a large abscess was found, and on the escape of an ounce or two of pus spontaneous respiration was resumed. It was, however, necessary to apply artificial respiration for some little time, and the patient remained comatose for about two days, then he began to recover, and it was possible by the usual tests to recognize the signs of a cerebellar lesion, e.g., coarse nystagmus to the side of the lesion—asynergia and unusual signs of co-ordination in limbs on same side as the lesion.

When he left the hospital after three months, he appeared to be perfectly well.

It is remarked that the drainage in this case was *not* by the route of infection for reasons which are obvious.

Cerebellar Abscess Five Weeks after Onset of Acute Otitis Media, Right Side.

By SYDNEY SCOTT, M.S.

A BOY, aged 7, was admitted with otorrhœa and deafness on the right side. During the previous week drowsiness had been noticed and he had vomited a few times.

On admission: Temperature, 98° F.; pulse 130; nystagmus to the right. Deviation to the right when pointing with right upper extremity. No asynergia (no dysidiadokokinesia, and finger-to-nose test was correct). Reflexes normal, except doubtful plantar response.

Schwartz operation: Very small extradural abscess, mesial side of sigmoid sinus. Exploration of cerebellum through this area, and about $\frac{1}{4}$ oz. of pus evacuated. Tube drainage.

The boy is still in St. Bartholomew's Hospital, and appears to be progressing favourably at the time of writing.

I ventured to bring forward these few cases of brain abscess as examples, on the one hand, of the comparatively scant clinical evidence which would have justified a clinical diagnosis of abscess in each case, and on the other hand, the fortuitous circumstances on which recovery depends. These cases do not comprise all we have met with during the last eighteen months, for some others which appeared as likely to recover have succumbed. Of the fatal cases, two cerebellar abscesses developed meningitis later, and in one case of temporo-sphenoidal abscess which had been diagnosed and drained within three weeks of the onset of acute otitis media, and had almost recovered,

there developed a very slowly spreading œdema of the brain, which terminated fatally about two months later. The autopsy in the last case proved that the abscess was efficiently drained, and there was no meningitis or intraventricular effusion, only œdema of the left cerebral hemisphere—a diffuse form of infective encephalitis.

DISCUSSION.

Mr. LAWSON WHALE asked whether Mr. Scott considered that amnesia was still present. This girl, when a watch was shown to her, called it a clock; whereas any girl aged 11 might be expected to know the difference between a watch and a clock.

Mr. G. J. JENKINS asked whether Mr. Scott found "Cushing's defect" in the perimeter tracing in the first of these cases. He thought that for amnesia to be present the abscess would be further forward than in cases in which this defect was likely to be obtained.

Sir CHARLES BALLANCE (President) said he thought that the presence of amnesia was a sufficient ground for diagnosis, and in this case Mr. Scott had diagnosed the condition at once, and had been able to save the patient's life. A little before the war he (the speaker) had as patient an officer who had undergone a mastoid operation for long-continued otorrhœa. The nurse who was looking after him one day reported that he had forgotten her name; and when shown a watch he had been unable to name it. He (Sir Charles) had gently removed the tegmen and incised the dura above it. The dura was not pulsating, and was discoloured. There was no abscess, but the part of the brain above the tegmen was in a condition of cellulitis. Drainage through the dura was continued for ten days, and the patient had recovered. Shortly before his complete recovery he had been able to distinguish and name objects, and afterwards he had remained well.

Mr. A. J. HUTCHISON remarked that the patient in the second case could use his hand well, except for buttoning and unbuttoning, and the disability in this respect was due to his having lost sensation in the tips of fingers and thumb.

Mr. E. MUSGRAVE WOODMAN asked what would be the best method of draining cerebellar abscess in contradistinction to abscess of the temporo-sphenoidal lobe. If cerebellar abscess were approached from the aural region, pus welled up from the bottom, and everything looked satisfactory, but it was upward drainage, and in most of the cases which he had seen the patients died. In approaching the abscess from below, if there was a dural space which was not shut off by adhesions, there was a risk of infection and the production of meningitis.

Mr. W. M. MOLLISON suggested that the numbness of the finger tips might be due to congestion of the cerebral cortex, in consequence of the temporary cessation of respiration.

Mr. G. J. JENKINS, referring to the danger of meningitis in cases of abscess, said that recently, in two cases, he had put B.I.P. round the edges, getting between dura and brain. It was a special preparation of B.I.P., without paraffin, and the components sterilized separately. He thought it acted as a wall to prevent the extension of sepsis. He believed that the statistics of the results of treatment of brain abscess would give a gloomier picture than was generally thought.

Mr. MUSGRAVE WOODMAN said he understood that at Queen Square Hospital intratracheal ether was being used as the anæsthetic in brain cases. At Birmingham they were using oxygen and chloroform intratracheally. With this method of anæsthesia it was not material if the patient breathed voluntarily or not.

Mr. H. J. MARRIAGE said that he had once had to operate at a cottage hospital on a child under artificial respiration in a case of acute mastoid disease. It had been obvious that a brain abscess was also present, but there had been no localizing

symptoms to show where it was situated. Just after the administration of the anæsthetic had begun, the child ceased to breathe, and this circumstance pointed to the cerebellum as the site of the abscess. The Silvester method of inducing artificial respiration had been carried out for ten minutes, without any return of breathing, although the pulse had remained quite good. The assistant had then employed a modified Silvester's method, bringing the arms up to the level of the shoulder and then pressing them forcibly against the chest. While this was being done he (Mr. Marriage) had trephined over the cerebellum. As soon as the trochar had been inserted, pus had escaped, and the breathing had immediately recommenced. For the moment he (the speaker) had been satisfied with putting in a drainage tube and leaving it. Next day he had opened up the mastoid and found a track running beneath the lateral sinus into the abscess. He had put drainage tubes into both wounds, and for a week the child had done very well. Then it had suddenly rolled over and died. Post mortem, not only a cerebellar abscess had been found but also a temporo-sphenoidal abscess, of which there had not been any symptoms.

Mr. CLEMINSON said that he had had an opposite experience in connexion with cerebellar abscess (in a girl aged 15). While the pus was draining out, the patient had stopped breathing, and though artificial respiration had been carried on for six hours, she had never breathed again. At that time there had been no intratracheal apparatus in the hospital for the administration of the anæsthetic, but he felt that if a tube could have been introduced through the larynx and air insufflated, possibly the œdema which caused pressure on the edge of the foramen magnum might have subsided. If he had to operate again in a case of cerebellar abscess he would have an intratracheal anæsthetic, because the tube could be left in position, and air could be blown into the lungs indefinitely.

Sir CHARLES BALLANCE (President) said that in his experience as soon as the dura was opened respiration was resumed. In one case he had had at Queen Square artificial respiration had been carried on for five hours; he (Sir Charles) had performed craniectomy, and as soon as the dura had been opened, the patient had begun to breathe again. This had also been the case during his operations on cerebellar tumours and abscesses, breathing had begun again as soon as pressure had been removed from the medulla. In the cases with which he had had to deal the Silvester method of artificial respiration had been employed. The most important point was that of the prevention of meningitis in cases of brain abscess in which there was no matting of meninges. That was a problem which, he thought, had not yet been solved. Some members had suggested putting various materials underneath the dura before opening the abscess; but it was not easy—owing to the pressure—to introduce anything between the dura and the arachnoid, before opening the abscess, and even if that was done, the sub-arachnoid space was not shut off. He (the speaker) had tried putting pieces of gauze which had been soaked in some solution between the dura and the arachnoid, simply closing the subdural space in that way, or putting in B.I.P. But that was not all that was necessary; the subarachnoid space must also be closed, for it was in that space that one desired to avoid the possibility of meningitis. In some cases, when he had opened the brain abscess, he had been unable to prevent the extension of infection to the subarachnoid space, through healthy brain tissue without matting of the meninges. If this problem could be solved the lives of the patients in these cases could be saved. Then we should not have to search for the pathological track through which the infection had entered the cranial cavity.

Mr. SYDNEY SCOTT (in reply) said that artificial respiration had been performed by the anæsthetist, Dr. R. Wade and the house surgeon, Mr. Prance, the sister of the ward acting as chief assistant while he exposed and opened the cerebellum.

Case of Vertigo cured by Opening the External Semicircular Canal.

By W. M. MOLLISON, M.Ch.

H. E., AGED 42, was seen at Guy's Hospital in September, 1922; for two months he had suffered from giddiness and tinnitus in the left ear. The giddiness came on suddenly while he was driving a van, objects rushed to the left and he "felt queer"; the attack gradually passed off. The attacks recurred, sometimes he had as many as three a day; on one occasion he fell in the fire and burnt himself severely. Examination of the ears showed normal membranes. Hearing in the right ear was good, in the left ear almost absent.

No spontaneous nystagmus was seen. Caloric response on both sides was sluggish; indeed it was doubtful whether any response was obtained on the left side. Past pointing normal on the good side (right) and absent on the deaf side. Dr. Symonds examined the patient and found no sign of intracranial lesion. It was decided to open the left external semicircular canal. This was done in January, 1923. Within four weeks of the operation the patient was entirely free from vertigo and could hear a whisper at a distance of 8 ft. from the left ear.

I have not seen the man for six weeks, but I have tested his hearing to-day, and find that he is deaf in the affected ear.

DISCUSSION.

Dr. DAN MCKENZIE asked whether Mr. Mollison could say what the lesion might have been. He (Dr. Mackenzie) remembered the suggestion that in some cases of vertigo there was a state similar to that in the eyeball in cases of glaucoma; that is to say the condition was due to pressure. He thought this theory would explain the symptoms in the present case; first the attacks of deafness, secondly the relief following the opening of the external canal.

Dr. KELSON said that he had had a similar case in 1913. The patient, a house painter, had been so giddy that he could not go up a ladder in the course of his work. He (Dr. Kelson) had operated on the right external semicircular canal, in a similar way to that described, and had shown the patient at a meeting of the Section in 1914. At that date he had practically lost the giddiness and the tinnitus. Since half the benefit from recording such cases was lost if they were not followed up, he (Dr. Kelson) had written to the patient's doctor, who had reported, in 1916, that the man was well, could again climb ladders and do his work and no longer had tinnitus. In 1921, eight years after operation, the report was that the man had kept well until the previous September, when he had again begun to have attacks of giddiness, and some degree of tinnitus.

Mr. ARCHER RYLAND said Mr. Mollison had certainly achieved a good result in this case, for not only had there been a total abolition of the vertigo, but the hearing had very substantially improved. He (Mr. Ryland) did not know if Mr. Mollison would strongly advocate this operation for the relief of vertigo. He (the speaker) would feel very hesitant about opening the external semicircular canal for that purpose. He had on one occasion opened this canal inadvertently in the course of an ordinary radical mastoid operation, and a prolonged and troublesome vertigo had resulted.

Sir JAMES DUNDAS-GRANT said he supposed the idea was that in this case there had been an increased tension in the fluids of the internal ear. He asked what had

been the steps in the operation. The middle ear had been preserved intact. He (Sir James) suggested that if the operation on the external semi-circular canal was to entail loss of hearing, it would be safer, and to many of us easier, to remove the tympanic membrane and ossicles completely, thereby allowing more room for the expansion of the internal liquids, through the freer play of the annular ligament in the fenestra ovalis. That greater play might suffice to relieve the pressure.

Mr. LAWSON WHALE said that he had done this operation deliberately in two cases. He had to do the radical mastoid operation first, otherwise he would not have obtained access to the mastoid horizontal canal. He made a $\frac{1}{4}$ in. opening immediately above and parallel to the horizontal part of the facial canal, and he would like to hear how large an opening Mr. Mollison had made, and how much exposure of the semi-circular canal he had secured. He would also like to know how long after the operation compensation for the loss of labyrinthine function had occurred. In his own cases the patients had been giddy for three weeks, and had then recovered.

Mr. MOLLISON (in reply) said that beyond accepting the suggestion that these cases might be compared to those of glaucoma, he had no theories. He approached such cases, from an operative point of view, with considerable hesitation. In the cases (four in number) in which he had operated, the hearing in the affected ear had been much diminished; in two it had been practically absent. Three of the patients had been taken into hospital and carefully examined by physicians before it had been decided to operate. In two of the cases he had operated at the request of the physician, as the patients' lives were a burden as a result of vertigo. In one case, that of a clergyman, there had been attacks for two years; the second case was the one shown to-day. In the third case the patient was a publican whose vertigo had been so severe that he had had to give up all his work. The fourth case was that of a private patient, who had consulted Dr. Hurst. He (Mr. Mollison) had found the operation so easy that he feared he might be tempted to perform it without due consideration. He opened the mastoid exactly as for ordinary mastoidotomy, exposing the aditus region as fully as for a mastoid, so as to get a good view of the external semi-circular canal; he then chipped open the canal. He had not formed a conclusion as to the best course to pursue after that. In the first case, remembering Mr. Sydney Scott's experiments on pigeons, he had injected absolute alcohol into the canal, in order to destroy the function of the labyrinth. The result had been fortunate; it was more than two years ago since the operation, and the patient had resumed work and had had no more vertigo. In the second case alcohol had not been injected, and all vertigo had ceased. In the third case pure carbolic acid had been applied, and the attacks had ceased absolutely. The fourth case was so recent that the result was not yet known. The operative treatment of cases of non-suppurative labyrinthine vertigo had been suggested many years ago by Mr. Cheatle, and carried out by Mr. Lake, Mr. Jenkins, Mr. Hugh E. Jones, Mr. Sydney Scott, and others.

The difficulty consisted in selecting cases in which operation was indicated, and in excluding extra-labyrinthine causes for the vertigo.

Sequestra removed from the Region of the Eustachian Tube during a Radical Mastoid Operation.

By T. H. JUST, F.R.C.S.

THE patient, a female, aged 23, had suffered from suppuration in the right ear since childhood. For six weeks before being seen by exhibitor she had had a right-side facial palsy. She died suddenly, five days after a radical mastoid operation. An autopsy showed extensive osteitis, practically the whole of the petrous portion of the temporal bone being involved.

Section of Ependymal Glioma growing from the Floor of the Fourth Ventricle, simulating a Cerebellar Abscess, in a Case of Bilateral Chronic Suppurative Otitis Media.

By T. H. JUST, F.R.C.S.

THE patient, a male, aged 26, complained of double otorrhœa of long standing, and of symptoms indicating that the left cerebellar fossa was involved. On exploration, the cerebellum bulged under pressure but no abscess was found. The patient became worse, and a more thorough exploration was made but the result was negative. The patient suddenly died of respiratory failure two days later. Post mortem, an ependymal glioma was discovered, arising from the floor of the fourth ventricle. The tumour would have been absolutely irremovable.

PROCEEDINGS
OF THE
ROYAL SOCIETY OF MEDICINE

EDITED BY
SIR JOHN Y. W. MACALISTER
UNDER THE DIRECTION OF
THE EDITORIAL COMMITTEE

VOLUME THE SIXTEENTH

SESSION 1922-23

SECTION OF PATHOLOGY



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1923

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SECTION OF PATHOLOGY.

CONTENTS.

October 17, 1922.

Professor J. C. G. LEDINGHAM, C.M.G., M.B., F.R.S.	PAGE
President's Address: Natural Resistance and the Study of Normal Defence Mechanisms	1

November 21, 1922.

J. C. MOTTRAM.	
Some Effects of Exposure to Radium on the Blood Platelets	9
CUTHBERT DUKES, O.B.E., M.D., D.P.H.	
A New Fermentation Tube, in which Carbohydrates may be separated from Proteins during Sterilization	18

February 20, 1923.

S. G. SHATTOCK, F.R.C.S., F.R.S.	
The Disruptive Phenomena in Gunshot Injuries: their Physics	17
S. C. DYKE, M.B., B.Ch., D.P.H.Oxon., and C. H. BUDGE, M.R.C.S., L.R.C.P.	
On the Inheritance of the Specific Isoagglutinable Substances of Human Red Cells. With a Note on the Possible Existence of a Lethal Factor by S. C. DYKE	35

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LONDON :
JOHN BALE, SONS AND DANIELSSON, LTD.,
OXFORD HOUSE,
83-91, GREAT TITCHFIELD STREET, OXFORD STREET, W. 1.

Section of Pathology.

President—Professor J. C. G. LEDINGHAM, C.M.G., M.B., F.R.S.

Natural Resistance and the Study of Normal Defence Mechanisms:

PRESIDENT'S ADDRESS.

By Professor J. C. G. LEDINGHAM, C.M.G., M.B., F.R.S.

SOME form of address from newly-elected presidents has been customary in the past, and I would desire your forbearance while I review certain aspects of immunity which have long baffled the experimental pathologist and which are certain to receive in the future more adequate consideration when the fundamentals of the science of immunity like those of all experimental sciences come to be relaid.

The phenomena to which I direct your attention come in the category of what is known as natural immunity or natural resistance—a subject vast and many sided—and I propose to consider simply what amount of light has been thrown on the elucidation of certain well-known instances of natural immunity to bacterial infection, by the study of the bactericidal functions of body cells and fluids. The infection I choose for illustrative purposes is that of anthrax, largely because it has been in connexion with the peculiar and fascinating divergencies of susceptibility exhibited by animal species towards this infection, that defence mechanisms have been tested with a view to their elucidation.

Some preliminary considerations need ventilation before I proceed to the main theme. When we consider the enormous output of literature on immunity which, since the beginning of the century has followed regularly the discovery of some new defence mechanism, we have reason to feel that some sufficient explanation might have been vouchsafed for the existence of these peculiar resistances, but as I hope to show you to-night, there is no subject in immunity which has been so persistently and yet so inadequately explored. The discovery of a new immunity mechanism has led in the first instance, as a rule, to its intensive exploitation for diagnostic or therapeutic purposes, and rightly so in the main. Some mechanisms have lent themselves more readily than others to such exploitation. Many again have failed to attract anything but a passing fancy and they have been promptly forgotten or ignored, while the great flood of freshly gathered facts and fictions has continued to roll on uninterrupted. And yet if, as I believe, knowledge is best grasped in its historical setting, then surely these half-forgotten theses must claim the attention of the serious investigator. I am no lover of the archives for their purely bibliographic interest, but I firmly believe that were more attention paid to the archives by the average investigator, not only would

there be a welcome reduction of much unnecessary literature, but a keener vision of our science as a whole and a greater ambition to build truly. Even in my very brief working experience I have seen many an instance of failure, innocent usually, sometimes possibly deliberate, to fit some apparently novel fact into its proper setting in the great edifice of natural knowledge. None the less, with the colossal mass of literature on pathology, bacteriology, and immunity on our shelves, it is no easy task to comply with the historical method. To take the subject of immunity alone, it may be remembered that no fewer than twenty volumes of the *Zeitschrift für Immunitätsforschung* appeared in the five years preceding the war—a truly vast encyclopædia of alleged fact—and this single effort represented only a fraction perhaps of the total output for the period in question. In face of this, how is the junior investigator to be directed to lay his bricks in an edifice the alignment of which he cannot possibly know? The task, I admit, is hard, but I maintain that the ambition should ever be to build truly on the historical past so that when the time comes for synthesis the old bricks may simply require relaying. The real expert must aim to be a man of vision with a working knowledge of, and a pride in, a glorious historical accomplishment. A mastery of technique is often, in my opinion, of much less relative value.

THE PROBLEM OF NATURAL IMMUNITY.

Natural immunity remains a dark corner in our edifice. Immunology as an essentially experimental science has undoubtedly gained its chief triumphs in the domain of acquired immunity. It has sought with marked success not only to imitate the immunity that is seen to follow successful combat with the actual disease naturally contracted, but also to transfer the chief bearer of that immunity from the immune subject, be it recovered human or immunized horse, to the acute case. In some notable instances we seem to know with certainty what we are doing in so acting, that, for instance, the passive fluid injected represents simply so many units of an accurately titrated substance, suspended, we shall say, in a vehicle of serum. So far as we are able to judge experimentally, the vehicle itself might be indifferent. In any case, if it has effects of its own apart from its known content of titratable substance, and there would appear to be some clinical evidence of their presence, these effects are not such as can be readily repeated or experimentally controlled. In other cases in which the passive transference of immune serum is followed by undoubted success as, for example, in anthrax, it has so far been impossible to determine precisely what particular principle in the serum so injected is responsible for the success. In other infections again, such as the coccal septicæmias, the success achieved has been but partial and fortuitous. Either the systems of titration on an *in vitro* basis have been unsatisfactory or, when biological titration has been partially possible, the existing great variety of coccal types both in man and animals and their contrary affinities for various animal species will doubtless long militate against the elaboration of any rational and stereotyped scheme of serotherapy in these infections. We may learn, however, from our difficulties. We can see that nature, specifically unaided, can successfully circumscribe the sphere of operation of a coccal or even an anthrax infection while she may fail to control a general invasion. We note also that nature not infrequently appears to derive much assistance in the control of infection from the inoculation, for example, of a normal serum or from the inoculation of some type of colloid fluid circumspectly administered. Possibly the not infrequently observed phenomenon of

the incompatibility of double infections may be placed in the same category of facts. In any case there would appear to be abundant justification at the present stage of immunological research for the closest study of the normal defence mechanisms.

THE MECHANISMS OF DEFENCE.

It is a strange circumstance that those curious instances of normal resistance which are referred to in all the text-books, should rest on such an insecure basis of fact from what might be called the quantitative standpoint. They, and the alleged explanatory mechanisms, appeared to fascinate the earlier workers intensely, but the experimental work they devoted to their solution cannot now be regarded as authoritative in the light of present knowledge. It seems that as each new mechanism of defence was discovered it was immediately tested and generally found to explain the observed resistance to the satisfaction of the discoverer. In what follows I shall illustrate what has happened in the case of anthrax and draw certain inferences as to future lines of progress. Put succinctly, the problem before us is simply this: Is the mechanism of a certain case of natural resistance capable of full and satisfactory expression in terms of test-tube analysis? Or must mechanisms other than those with which we are familiar be called in to explain the phenomenon?

Earlier Contributions to the Subject.

The mechanisms are not many and it would appear advisable to summarize them briefly before discussing their application to the problem in question. What contributions to the mechanism of defence were made by the great masters of general pathology and cytology of the past half-century? I need not discuss, even if I possessed the necessary knowledge, the various doctrines and conceptions of inflammation that formed the basis of pathological teaching of possibly most of us, but it is very obvious from even cursory analysis of the works of the great masters that the phenomena of inflammation gradually but surely came to be regarded in the light of natural defence mechanisms. That this was so is abundantly evident at the commencement of the present century and in illustration I may cite the inaugural address of Marchand, a valued teacher of my own, on assuming the chair of Cohnheim at Leipzig in 1900. The title of his address was "Ueber die natürlichen Schützmittel des Organismus," and it was an attempt to summarize, in the sense of defence mechanisms, the various changes produced in the course of the inflammatory process—changes in the circulation of the part, in the circulating blood elements and in the fixed tissue cells, the local leucocytic response, the clearing or scavenging of the infected area presumably brought about by ferments from living or broken-down leucocytes, the restoration of the circulatory mechanism by newly-formed vessels growing on a scaffolding of fibrin and perhaps gaining the necessary energy for their development from the discharged effusion, the consolidation of the whole by newly-formed tissue derived from the fixed tissue cells of the part—these and many other processes he regarded as essentially defence mechanisms depending on the reactivity of the local tissues.

On the whole I receive the impression from reading the works of these masters that their methods of work were too local and circumscribed to render the results capable of general applicability to the phenomena of bacterial invasion. They had little conception then of the vast potentialities for defence residing not only in the fluids circulating in the inflamed part but also in the

emigrated leucocytes and possibly also in the fixed tissue cells. Since those days the immunologist has had his innings, but I am of opinion that we shall return to the consideration on ampler lines of local condition and function in the widest sense if we are to understand thoroughly the rationale of natural immunity. Already one sees a tendency towards the combined histological and serological attack on these problems.

Metchnikoff's Conception of Phagocytic Action.

I pass to Metchnikoff, whose attempt to extend the sphere of phagocytic action from the physiological to the pathological field and to read into it the idea of a protective mechanism with an application to all higher animals possessing circulating amœboid elements, constituted the first large-scale conception calculated to raise the lore of inflammation from one of purely local to one of the most general application. It was, in fact, the commencement of immunity as a general science. To Metchnikoff the leucocyte came to be endowed with particular qualities and properties according to the reactivity of the host. It was, moreover, the source *par excellence* of any and all bactericidal substances that might be present in cell-free fluids of the body. The constant polemics into which his rigid adherence to the conception of the all-sufficiency of the phagocyte led him are now matters of history, but it has to be remembered that these very polemics with the rising school of humoralists led by Nuttall, Buchner, and a host of others, gave the stimulus to uncounted researches on the properties and sources of growth-inhibitory and bactericidal bodies in tissues and fluids. Metchnikoff sought to retrieve the position of the phagocyte by many ingeniously contrived experiments, but it was obvious that opinion was definitely ranged alongside the newer humoral ideas while the ultimate source of the alexin and the intermediary body or *substance sensibilisatrice*, the co-operative action of which with a thermolabile alexin was later demonstrated, were left more or less open questions. The final demonstration by Denys and Leclef, Mennes, and others showing the dependence of phagocytic action in immune serum on the presence of a *substance sensibilisatrice* and the extension of the principle to normal serum, by Wright and Douglas, constituted a reasonable enough reconciliation between the opposing views. We know, however, that absolutely independent phagocytic action cannot be excluded as a defence factor, especially when organisms of low virulence are in question, and researches on spontaneous phagocytosis have demonstrated that in a given collection of leucocytes exposed to organisms, some individuals undoubtedly appear to possess much higher phagocytic powers than others. We have not reached the end of this particular problem.

Properties of the Normal Alexin.

After the phagocyte came the alexin of the cell-free fluids. The complex nature of the normal alexin and its presence both in plasma and in cell-free serum are now fairly generally accepted facts. It should be noted, however, that the complex nature of the normal alexin is much more difficult to demonstrate than that of the so-called bacteriolysin in immune serum, and, as we shall see, there is now evidence that certain normal sera possess considerable bactericidal and growth-inhibitory effects which are not destroyed by the usual inactivation temperatures. In fact, the test-organism in all these matters is of prime importance. Here it is sufficient to note that the normal alexin can kill or dissolve certain organisms while others are unaffected or at most suffer growth inhibition.

Properties of Leukins.

I pass to the leukins or the bactericidal substances present in extracts of leucocytes. The study of these arose largely out of the views expressed by Buchner and Metchnikoff that the source of the alexin might possibly be found in such substances. The chief work on this defence mechanism, which has not perhaps attracted the attention it deserves, has been that of Hahn, Schattenfroh, Petterson, Kling, Manwaring, Schneider, and Petrie in this country. I need not recapitulate the various methods employed for securing potent extracts. These will be found in all text-books, but it is quite clear that the methods of extraction influence greatly the potency of the resulting product. These extracts do not lose their power of killing certain test organisms after heating, say, at 60° F. They can resist very much higher temperatures, even up to 80° F. The constitution of these leukins or endolysins is still uncertain. Some workers have attempted to show that they possess complementing powers in the presence of inactivated sera, but others have entirely failed to confirm such action. Petterson would say that these extracts contain both an alcohol-soluble and an alcohol-insoluble fraction, and that the one can inhibit the action of the other. These effects, however, are almost certainly to be reckoned in the category of inhibition phenomena explicable on colloidal principles. The chief interest of the leukins lies in the effects they produce on different groups of organisms, and in the similarity of such effects to those produced by very analogous extracts prepared from tissues, and which were demonstrated twenty years ago by Conradi, Korschun and Morgenroth, Tarassewitsch, and others. These leukins have, as a rule, been tested against organisms of the typhoid-coli group and organisms of the subtilis group, to which anthrax belongs. Curious differences have been shown by extracts of leucocytes of various animal species in their action on bacterial types. Thus guinea-pig leucocytic extracts are said to possess little or no bactericidal action on *Bacillus typhosus*, while those from the rabbit are distinctly potent. Petrie, however, using extracts prepared from leucocytes triturated at a temperature of liquid air failed to demonstrate bactericidal bodies for *Bacillus typhosus* in rabbit leucocytes. The leucocytic extracts of the hen have, according to Schneider, no action on *Bacillus typhosus*, but a very considerable action on *Bacillus anthracis*. On the other hand, the serum of the hen can kill *Bacillus typhosus*, but has little action on *Bacillus anthracis*, so that it would seem that absence of bactericidal property in the extract of a cell might be compensated by its presence in the surrounding fluid, and vice versa. The study of bactericidal bodies in tissue extracts and body secretions is again being actively pursued in connexion with bacteriophage problems. In the so-called bacteriophage, from whatever source it may be obtained, there is exhibited the same thermostability and the same limitation of action to certain bacterial groups. Rapidity of action of these leucocytic extracts on organisms of the subtilis group and slowness of action on organisms like *Bacillus typhosus*, with subsequent overgrowth of presumably resistant organisms, are features which recall those noted in investigations connected with the bacteriophage and with the bactericidal bodies present in egg-white, as demonstrated by Rettger and Fleming.

I close this subject by noting the existence of the thermostable bactericidal body in rat serum. This body has been carefully tested by Pirenne against organisms of the subtilis group, and also organisms like *Bacillus coli* and *Bacillus pyocyaneus*. Plating experiments have shown that organisms like

Bacillus mycoides, *Bacillus megatherium*, *Bacillus subtilis*, are rapidly killed off, while *Bacillus proteus*, *Bacillus coli*, and *Bacillus pyocyaneus* multiply freely. The cholera vibrio is also killed off, but this action was found to be due to the ordinary thermolabile alexin in the rat serum, and it disappeared after inactivation of the serum.

Proteolytic Bodies in Leucocytes.

There remain only the proteolytic bodies contained in leucocytes; these have been studied by many workers chiefly in connexion with the so-called antitryptic action of serum. We know little or nothing of their action on bacteria, and indeed it would be difficult to separate any such action exhibited from that due to the more generally studied endolysins. I may just mention the alleged existence of bactericidal bodies in platelets, a subject introduced by Gruber and Futaki in 1907, and but little studied since. These authors came to the conclusion that the bactericidal action on anthrax of normal rabbit serum (a highly susceptible animal) depended on substances derived from the platelets. Barreau, who continued this work, found that the serum of the dog (a highly resistant animal to anthrax) had no action on anthrax nor had its platelets. He concluded, however, that the platelet bactericidal bodies or plakins probably did not play much of a part in natural resistance, as the rat, for instance, a resistant animal, was rich in plakins, while the rabbit, a susceptible animal, was equally so. It is possible that the recent work on the purely mechanical function of blood platelets in removing suspended organisms by virtue of their adhesive properties may throw a different light on these alleged bactericidal substances in platelets.

ANIMAL EXPERIMENTS.

The application of these defence mechanisms to the elucidation of natural resistance to anthrax can now be very shortly considered. The resistant animals chiefly studied have been the frog, the fowl (especially the hen and pigeon), the rat, and the dog, but, as I have indicated before, we have no accurate data of a quantitative kind as to the extent of this resistance in most cases. There is no doubt that the frog presents an extraordinary resistance to anthrax infection—a resistance which in the early days was attributed to its low body temperature. Attempts were made to infect frogs kept at 37° C., and in these circumstances the animals readily succumbed. Metchnikoff attributed the deaths in these cases to diminished phagocytic action, whereas in the frog, the temperature of which was not interfered with, exuberant phagocytosis at the seat of inoculation afforded sufficient explanation of the immunity. The humoralists, however, maintained that the immunity was due to the bactericidal properties of the local lymph (Nuttall, Baumgarten, Petruschky, &c.). Metchnikoff countered this by showing that *Bacillus anthracis* could grow readily in frog plasma. Galli-Valerio favoured the combined action of phagocytosis and bactericidal property of lymph as the most likely explanation. The matter remains quite obscure, and a more recent worker, Dithorn, simply states that anthrax rods inoculated in any way into frogs show degenerative changes in a few days and lose their contours. The test organisms may, of course, play a decisive rôle in view of the fact that Dieudonné, for example, cultivated a race of anthrax growing abundantly at 12° C., and with it succeeded in killing frogs readily. These experiments require confirmation.

With regard to fowls, the hen and pigeon, and particularly the hen, are known to possess high resistance, and in the classical experiments of Pasteur and Joubert, in 1878, the immunity was attributed to the high body temperature of the fowl. By immersing the fowl in cold water infection took place. The death under such circumstances has been attributed by later workers to a general lowering of resistance, and not to an inability on the part of *Bacillus anthracis* to grow at the high temperature of the fowl. Metchnikoff maintained that phagocytosis in the normal hen was rapid and complete, and in the cooled hen very poor. Later, Thiltges stated that phagocytosis was not in evidence, and that the immunity was due to the bactericidal action of the plasma, a property which Gengou denied. Thiltges agreed, however, with Metchnikoff in the matter of the pigeon. Bail and Petterson and Schneider ascribe the resistance to the action of the hen leukins, which act very powerfully on *Bacillus anthracis*, while the serum has relatively little action. Donati in a more recent communication ascribes the immunity of the fowl simply to a local invasion of leucocytes, which hinder capsule formation, and by virtue of bactericidal substances secreted by them, and not by phagocytosis, secure the removal of the invaders.

It is notorious that the adult dog can tolerate without inconvenience the inoculation of large quantities of bacilli, and, as one might expect, this immunity has been attributed by Metchnikoff to phagocytosis at the site of inoculation. Hektoen later showed that in the presence of dog serum dog leucocytes readily took up *Bacillus anthracis*. It would appear that the serum of the dog has but little or no anthracidal action as compared, for instance, with that of the rabbit, which is, on the contrary, a fairly highly susceptible animal. While without action on *Bacillus anthracis*, dog serum, according to Petrie, has a powerful action on *Bacillus typhosus*. Hektoen attributes some importance also to the leukins of the dog. Petrie, however, found none.

The rat presents a more interesting problem, though it has to be remembered that there is no absolute immunity in this species. Behring, in 1888, showed that rat serum was anthracidal, while Metchnikoff found that the main defence was the phagocytic response. The thermostability of the bactericidal body in rat serum, as shown by Pirenne and Horton, is a most interesting feature. It acts equally well at 18° C. as at 0° C., and remains active for fairly long periods in the cold.

SUMMARY.

To summarize, it must be confessed that the curiously contradictory and yet perhaps genuinely reasonable explanatory theses give us little that is solid to hang on to. No one instance of normal immunity has yet been investigated as a complete problem. Partial mechanisms only have been studied. The hearer might well conclude that dogs are immune because dogs are dogs, and so for rats, fowls and frogs, but that would not be quite the impression I should like to make. If a certain animal is immune to a particular experimental infection, such as anthrax, one ought to be able to explain fully what local phenomena have occurred to prevent a general invasion by the organism. To do so effectively must involve the testing of each possible mechanism separately and in conjunction, and it must involve a return to the cytological study of the changes which the invading organism undergoes *in situ*. The problem must be attacked not only by methods which derive their authority from long experience with the bactericidal properties of cells and fluids, but also by methods which reflect the trend of present-day studies on general

metabolism both of parasite and host. With regard to the former much has been made of the capsule, but the data on the point are contradictory. In every set of experiments strict attention must be paid to the maintenance of virulence. It may, indeed, be found that by experimental selection a test organism which has once proved virulent for one individual of a resistant species may prove equally so for all individuals of the species. Strains of *Bacillus anthracis* have been thus selected which are alleged to have killed fowls, rats, and frogs, but the experiments lack confirmation.

Another important aspect of the subject which has recently been brought to the forefront by Besredka relates to the site of inoculation of the test organism. In the course of his researches on the production of immunity by vaccinating that portion only of the body which is most susceptible Besredka has turned his attention to anthrax infection in the guinea-pig, an animal notoriously difficult to protect by any method of vaccination. He shows experimentally what, by the way, had been amply demonstrated twenty years ago by Noetzel, that animals like the rabbit and guinea-pig can tolerate easily doses of virulent anthrax if introduced directly into the circulation or into the peritoneal cavity without the cutaneous tissues being contaminated. This contamination can be avoided by a special and careful technique. According to Besredka the skin of the guinea-pig is the only susceptible portion of the guinea-pig's anatomy, and if it had no skin it would be a highly refractory animal instead of being, as it is, one of the most susceptible. He further demonstrated the possibility of securing solid immunity to anthrax, by whatever route inoculated, by vaccination of the skin with the attenuated Pasteurian vaccines. I do not wholly accept much of the evidence adduced so far in support of the conception of partial or local immunities or susceptibilities, but I believe the matter is worth the fullest investigation. In any case it is obvious that future work on natural resistance must take count of the possibility of very diverse immunities or susceptibilities apparently combined in one immune whole.

I have dealt in this address with species resistance solely, but it has to be remembered that there are racial variations of resistance within the species. For this reason the study of the mechanism of normal immunity will doubtless demand the services of the geneticist, who will be responsible for securing pedigreed stock for experimental purposes. This is no fanciful suggestion. In connexion with these most promising developments in experimental epidemiology which are being carried out in this country and in America the services of the geneticist must be invaluable. The dietetic factor, too, may prove of supreme importance in experiments on natural resistance, and there is already a body of evidence pointing in this direction. It is possible also that we may learn something from comparative observations on the rationale of natural immunity in plants to fungal infections. In a recent address by Blackman some of these mechanisms reveal extraordinarily interesting relationships between the attacking fungus and the cells of the immune host. In conclusion, I feel that I have touched but the fringe of this vast subject, but if my exposure of the little we really know may stimulate further research I shall be content.

Section of Pathology.

President—Professor J. C. G. LEDINGHAM, C.M.G., M.B., F.R.S.

Some Effects of Exposure to Radium on the Blood Platelets.

By J. C. MOTTRAM.

(From the Research Department of the Radium Institute.)

IN a previous paper [1] it was shown that exposure to radium is followed by a great diminution in the number of platelets in the blood. This has been recently confirmed by Möller [2]. An exposure of rats to 0'46 rads gave rise to a fall in platelets as shown on fig. 1, rats Nos. 1 to 5, where it is seen that a return to the normal level occurs on about the twenty-second day.

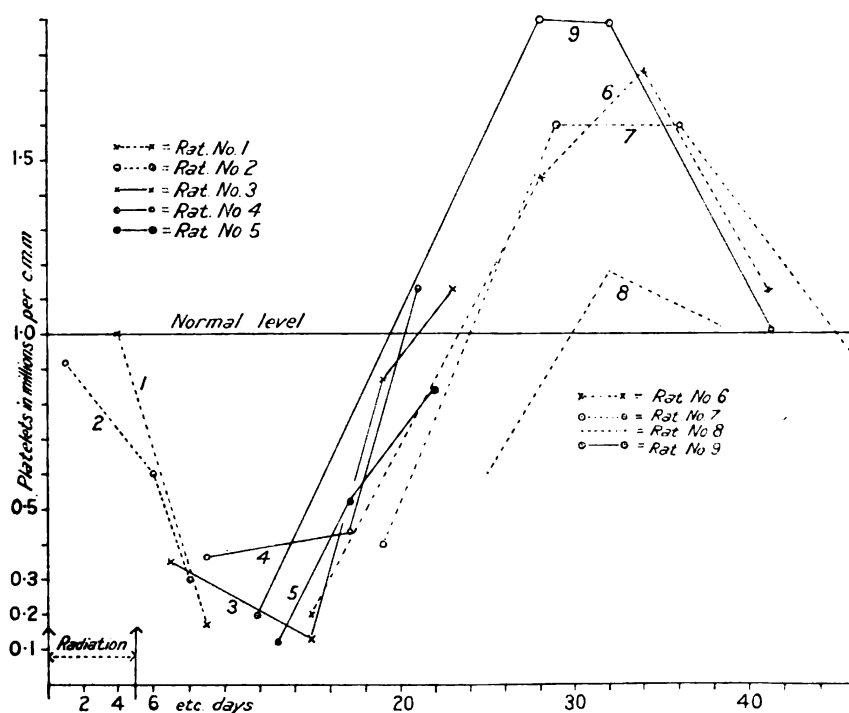


FIG. 1.

Continuing these investigations, observations have been made in order to discover what is the behaviour of the platelets after this return to the normal. The results are shown in fig. 1, rats Nos. 6 to 9, and in Protocol I. They show that after reaching the normal level a rise in platelet numbers occurs, having its peak about the thirtieth day. This is followed by a fall, a second return to the normal occurring in the neighbourhood of the fortieth day. The red-cell counts present no equivalent changes, and the control observations

show that repeated blood examinations do not alter the platelet content in a similar manner.

Experiments were next carried out in order to discover whether an increase in platelet numbers could be brought about by repeated small exposures to radium, similar to the lymphocytosis which follows small doses of radiation frequently repeated [3]. Four rats were exposed daily to radiation for one hour, and another set of four animals for an hour every other day. The conditions of radiation were as tabulated in Protocol I. Eight unirradiated control animals were at the same time examined. After a few days the radiated animals presented a fall similar to that shown in fig. 1, but less steep. During the first few days, however, a number of counts much above the normal were irregularly observed. The distribution of these counts is compared with the controls in fig. 2, where it is seen that high counts occur among the experimental animals; whereas in the controls no counts above 1.4 millions per millimetre were observed, in the experimental, five counts of 1.8 and one of 2.0 were found.

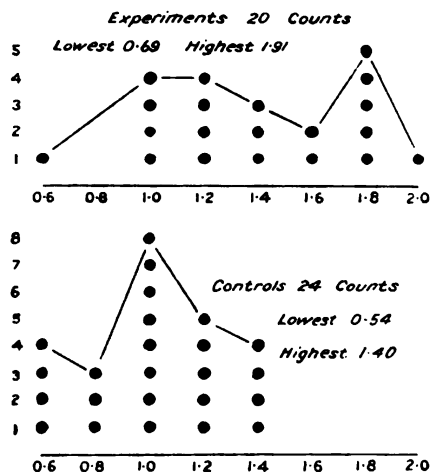


FIG. 2.

A more intense study of the early effects was therefore made with the use of animals exposed for one hour twice a week. The results are given in fig. 3, and Protocol II, and show a decided rise in the platelet counts, having a maximum about the end of the second week. This was repeated on two more animals (*see* Protocol III), one of which showed a very decided rise on the tenth and twelfth days, and the other a less pronounced rise on the twelfth day. In all these animals a rapid return to the normal occurred, although exposure to radium was not discontinued. An exposure of one hour per week was not followed by a change in platelet numbers, although the observations extended over a period of eight weeks (*see* Protocol IV).

Observations were next made to see whether a similar transitory rise preceded the profound fall produced by continuous radiation, as in fig. 1. The findings are given in Protocol V, where it is seen that under an intensity of radiation sufficient to produce a diminution in platelet numbers beginning at about the fifth day, no preliminary rise occurred during the first four days.

The morphological characters of the platelets were not observed to vary under these conditions of radiation. In some cases an increased density at

the centre of the platelet occurred, simulating a nucleus, but when fixed and stained for chromatin it was seen not to be of the nature of a nucleus. The stains which most strongly coloured the platelets were found to be those

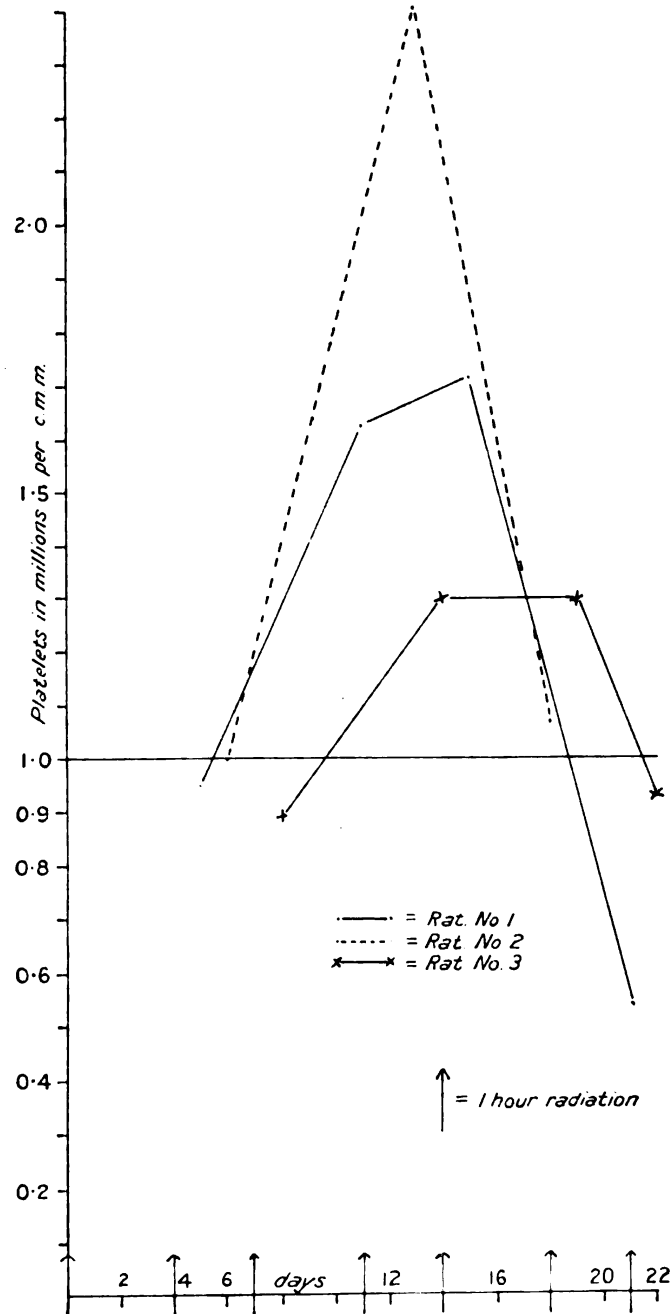


FIG. 3.

especially powerful in staining protoplasm, such as eosin, acid fuchsin and prolonged exposure to iron hæmatoxylin, and these also stained the central opacity above mentioned.

*

12 Mottram: *Some Effects of Exposure to Radium*

The origin of platelets is still a matter of controversy [4] so that a comparison of their behaviour under radiation with the behaviour of the other blood cells may perhaps give a clue as to their source. Under conditions of continuous radiation, similar to those used in the above experiments, a lymphopænia occurs within a few hours, a polynuclear leucopænia in about seven days, and an anæmia in about fourteen days, so that the reaction of the platelets is much more nearly comparable with the red-cell behaviour than with the lymphocytic or polynuclear. This suggests that the platelets are likely to be related in origin and morphology to red cells rather than to leucocytes.

The possibility of being able either to increase or to diminish the platelets offers a field of research into the function of these bodies, which is being followed up, more especially with regard to their supposed action on micro-organisms [5]. Möller [2] is of opinion that the anæmia following radiation is the result of hæmorrhages into the tissues conditioned by the diminished platelet numbers.

SUMMARY.

Prolonged exposure to radium causes a profound fall in the number of blood platelets, which is followed by a return to the normal, then by a rise above the normal, and finally by a second return to the normal level.

Repeated small doses of radiation give rise to a primary rise in platelet numbers, which is, however, maintained only for a few days, when the normal level is again reached.

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[1] CRAMER, DREW, and MOTTRAM, *Proc. Roy. Soc., B*, 1922, xciii, p. 449. [2] MÖLLER, *Comptes Rend. Soc. de Biol.*, 1922, lxxxvii, p. 759. [3] RUSS, CHAMBERS, SCOTT, MOTTRAM, *Lancet*, 1919, i, p. 692. [4] BEDSON, *Journ. Path. and Bact.*, 1922, xxv, pp. 400, 401. [5] COVAERTS, *Comptes Rend. Soc. de Biol.*, 1922, lxxxv, p. 745.

PROTOCOL I.

Four rats, weights 120, 105, 115, 105 gm., exposed continuously for five days to 220 mgr. RaBr_2 , $2\text{H}_2\text{O}$, distance 8 in., screen 0.1 mm. lead plus 0.12 mm. silver; equivalent to 0.46 rads. Red-cell counts in millions corresponding to platelet counts shown in fig. 1:—

Rat No. 6	7.7	...	8.6	...	7.5	...	7.3
Rat No. 7	9.4	...	10.7	...	9.5	...	9.7
Rat No. 8	7.8	...	8.9	...	8.9
Rat No. 9	8.0	...	8.0	...	8.1	...	9.8

Three control rats, weights 60, 70, 90 gm. Red-cell counts in millions corresponding to platelet counts:—

Rat No. 1	8.2	...	9.1	...	8.4	...	10.6
Rat No. 2	8.1	...	10.8	...	10.4
Rat No. 3	8.1	...	9.2	...	9.7	...	11.1

Platelet counts in millions:—

Days	5	7	13	20
Rat No. 1
Days	0	11	23	...
Rat No. 2
Days	5	11	18	24
Rat No. 3

PROTOCOL II.

Three rats, weights 60, 70, and 90 grm., exposed for one hour twice a week, 220 mgr. $\text{RaBr}_2 \cdot 2\text{H}_2\text{O}$, distance 8 in., screen 0.12 mm. silver. Red-cell counts in millions, corresponding to platelet counts given in fig. 3.

Rat No. 1	9.5	...	10.3	...	9.8	...	7.8
Rat No. 2	9.1	...	8.7	...	8.5	...	
Rat No. 3	7.8	...	10.5	...	8.2	...	9.0

PROTOCOL III.

Two rats, weights 75, 95 grm., exposed as on Protocol II. Red-cell and platelet counts in millions per millimetre:—

				Day		Reds		Platelets
Rat 75	7	...	11.3	...	0.59
"	10	...	13.2	...	2.02
"	12	...	12.1	...	1.65
"	15	...	10.8	...	0.63
				Day		Reds		Platelets
Rat 95	7	...	9.8	...	0.62
"	10	...	9.7	...	0.61
"	12	...	8.8	...	0.90
"	14	...	7.1	...	0.64

PROTOCOL IV.

Two rats, weights 70 and 60 grm. exposed as in Protocol II for one hour per week. Platelet counts extended over eight weeks. The following figures obtained in millions per millimetre. Time not given.

Rat 70	...	0.7	...	1.0	...	0.9	...	0.8	...	0.9	...	1.3	...	1.1	...	1.2
Rat 60	...	0.6	...	0.8	...	1.0	...	0.7	...	0.7	...	0.7	...	1.0	...	0.7

PROTOCOL V.

Four rats, weights 95, 85, 125, 120 grm., subjected to continuous radiation. Conditions as in Protocol I. Platelet counts in millions per millimetre.

Hours after radiation			0		2		4		24		29		48
Rat 95	0.7	...	0.8	...	0.9	...	0.6	...	0.6	...	0.5
Hours after radiation			0		14		18		39				
Rat 85	1.1	...	1.0	...	1.4	...	0.8				
Hours after radiation			52		76		98						
Rat 125	0.9	...	1.3	...	1.1						
Hours after radiation			53		73		97						
Rat 120	0.5	...	0.5	...	0.5						

A New Fermentation Tube, in which Carbohydrates may be separated from Proteins during Sterilization.

By CUTHBERT DUKES, O.B.E., M.D., D.P.H.

(I) OBJECT.

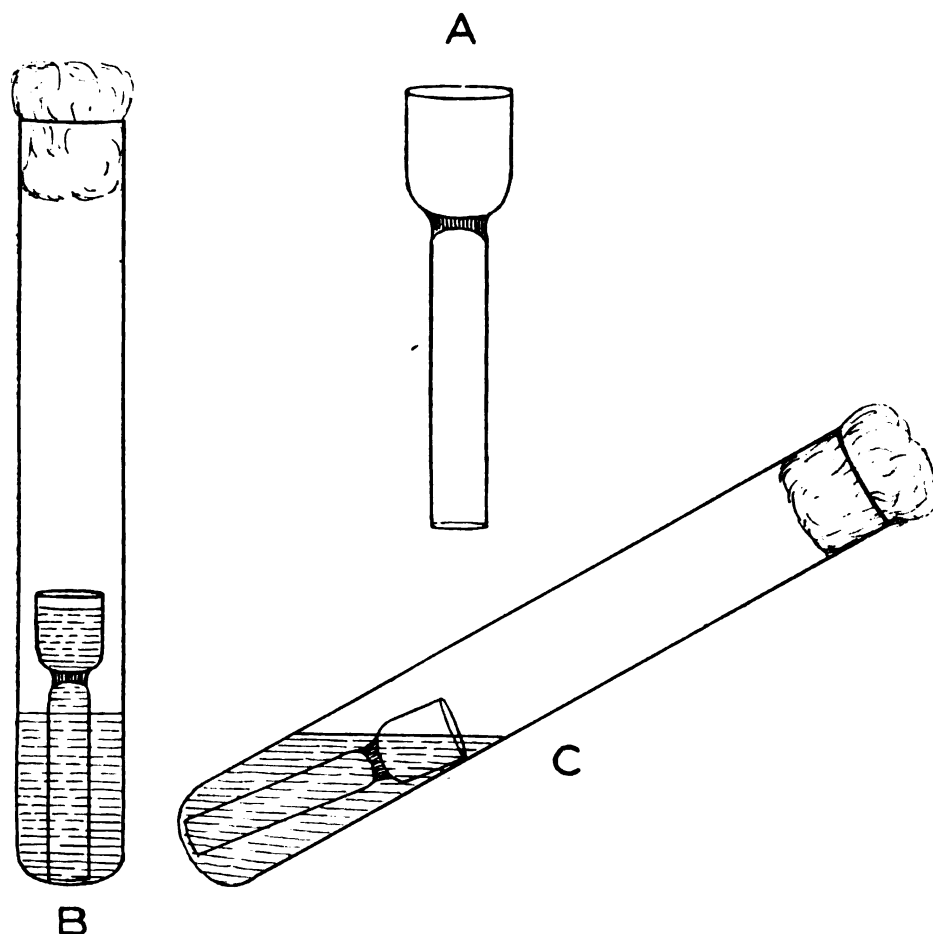
THIS fermentation tube is designed so that carbohydrates may be separated from proteins during sterilization, preparatory to the treatment of the fermentation of carbohydrates by bacteria in the presence of peptone.

(II) DESCRIPTION.

The tube consists of two parts, a stem and a cup, the cup resting on the stem. The stem is made of medium strength, hollow glass tubing $\frac{1}{4}$ in. in external diameter: the stem is $1\frac{1}{2}$ in. long, open at the lower end and closed at the top. The cup rests on the closed upper end of the stem, is $\frac{1}{2}$ in. in diameter, $\frac{1}{2}$ in. in depth and holds just over $\frac{1}{2}$ c.c. The cup and the stem are

[November 21, 1922.]

separated by an impermeable glass diaphragm. About $4\frac{1}{2}$ c.c. of a 1 per cent. peptone water solution containing an indicator is placed in test tubes $\frac{3}{8}$ in. in diameter and 6 in. long, and the fermentation tube dropped in. $\frac{1}{2}$ c.c. of a 10 per cent. solution of the sugar to be tested is then delivered to the cup by means of a pipette; the mouth of the test tube is plugged and the tubes are autoclaved in the upright position. As with Durham's tube, the air is driven out of the stem of the fermentation tube during sterilization and as the temperature cools peptone water rises to take its place so that the stem fills with fluid. Before inoculation the tube is tilted on its side and the sterilized sugar solution runs out and mixes with the sterilized peptone water, giving a



1 per cent. concentration of the sugar in approximately 1 per cent. peptone water. Tilting to an angle of 30 degrees is usually sufficient to ensure this mixing, and with 6 or 8 in. test tubes there is no danger of the contained fluids reaching the cotton-wool plug. After mixing, the tube is inoculated and incubated; acid production is determined from the change in the indicator; gas production, by the displacement of the peptone water from the stem.

[A, natural size; B, C, reduced.]

(III) REASONS FOR THE EMPLOYMENT OF THIS TUBE.

I designed this tube in an attempt to overcome certain difficulties encountered in a study of the fermentation reactions of some unclassified

intestinal bacilli producing alkaline colonies on bile-salt lactose-agar plates. I had been in the habit of sterilizing my carbohydrate and peptone solutions together in the steamer on three successive days and used Andrade's acid fuchsin indicator. Three reactions noted with these organisms—and, I may add, since observed with well-defined members of the paratyphoid group—gave rise to perplexity, namely, a transient early acidity, a transient late acidity and the production of a bubble of gas without acidity. By a study of the hydrogen-ion concentration of the sugar-peptone solutions in which these organisms were growing and by periodic quantitative estimations of the sugar by Benedict's method I have determined that these reactions are due, not to the fermentation of the sugar intended to be studied, but to the fermentation of the products to which the sugar is changed when heated in the presence of peptone. It is common knowledge that sugars must not be autoclaved in the presence of peptone; the very considerable change which takes place merely on heating in the steamer is not, in my opinion, generally appreciated. When a peptone water solution containing utilizable carbohydrate is inoculated with an organism and incubated, two reactions proceed concurrently—an acid fermentation from the hydrolysis of the sugar and an alkaline fermentation from the digestion of the protein. If lactose be the sugar to be tested and this has been steamed in the presence of peptone, a fraction of the lactose will have been hydrolysed to glucose and other simpler substances; if the organism under examination is a non-lactose fermenter, but capable of fermenting glucose, it may produce sufficient acid from the glucose (appearing as the result of steaming) to turn the indicator acid and a small bubble of gas may form, in spite of the fact that the lactose has not been fermented. This simple explanation was found by chemical tests to be responsible for the troublesome uncertainties referred to above. The particulars of these chemical tests will be published in detail at a later date; it is sufficient to remark at present that when the sugar solution was sterilized separately these difficulties at once disappeared.

(IV) ADVANTAGES CLAIMED FOR NEW FERMENTATION TUBE.

The danger of heating proteins and carbohydrates together has been well-known in bacteriological work but it is doubtful whether the extent of the alteration in the sugar has been recognized. This alteration appears to have been responsible for the doubtful reactions sometimes recorded for well-defined species and for the discrepancies in the results of different investigators. Although the experienced bacteriologist may not be deceived by these defects when working with familiar bacteria, they constitute a real difficulty in research work on unclassified organisms. The procedure of sterilizing the sugar solution in a separate vessel either by heat or filtration is open to the objections that it is time-consuming, it increases the danger of contamination and necessitates the subsequent incubation of the tubes to make sure they are sterile. The advantages of the new fermentation tube are the following:—

(1) *That no Alteration takes place in the Sugar during Sterilization.*—Miss Masters, B.Sc., has kindly examined for me, by the polarimeter method, some 10 per cent. lactose solutions that had stood at room temperature, as well as others that had been steamed and also others autoclaved. She found that no appreciable change took place as the result of autoclaving at 120° C. for twenty minutes. On the other hand she confirmed the pronounced deterioration suffered by the sugar when heated in the presence of peptone. Practical experience in the use of these fermentation tubes—

seventeen different carbohydrates having been employed in testing the large group of organisms with which I have been working—has shown that all uncertain reactions have been eliminated.

(2) *A more delicate Indicator can be employed.*—I use Andrade's acid fuchsin. It is customary to decolorize this with normal caustic soda until just straw coloured, but it sometimes happens with the old method that the tubes containing peptone, sugar and indicator turn red on sterilization and have to be discarded: to obviate this the indicator is often over-decolorized. Since this misfortune never occurs with the tube I am describing I have been able to use the acid fuchsin only just decolorized and thereby obtain a more delicate indicator.

(3) *The Feasibility of Autoclaving Sugar Solution.*—This is a great advantage. The less commonly employed carbohydrates are often wanted in a hurry and by this method they are available in an hour, whereas when sterilized in the steamer an interval of three days has to elapse. Autoclaving also is a more certain method of sterilization than steaming.

(4) *Economy.*—This fermentation tube is economical when dealing with the rare and expensive carbohydrates and by this means only the exact number of tubes required need be prepared. The current price of xylose, inosite and dulcitol make this a most useful attribute.

(V) THE MAKING OF THE TUBES.

These fermentation tubes are not difficult to make for anyone who has had a little experience of glass-blowing. They may be made either in one piece by closing the glass tubing which is to form the top of the stem and blowing a bulb for the cup, or two pieces of glass of different diameter may be joined end to end. In the former method medium-thickness glass tubing with an external diameter of $\frac{1}{4}$ in. is taken and cut into pieces 9 in. long, this providing sufficient glass for two tubes. Each piece is drawn out in the middle into a spear and then divided. The blow pipe is turned on to the glass tubing which is to form the stem 2 in. from the end and the tube revolved in the flame until the lumen is closed. The glass immediately beyond this diaphragm towards the spear is then heated and blown out through the open spear into a bulb $\frac{1}{2}$ in. in diameter and about $\frac{3}{4}$ in. long. When cool this elongated bulb is filed $\frac{1}{2}$ in. above the diaphragm and separated by applying a red hot bead of glass.

In the second method the same sized tubing is used for the stem but the bulb is made from medium-thickness glass tubing $\frac{1}{2}$ in. in diameter. The bulb piece is drawn out in the flame and the end sealed: the end of the stem piece is heated until closed and whilst both are red hot they are joined together. The joint is consolidated by heating and blowing at each end in turn. When cool the cup section is cut off $\frac{1}{2}$ in. above the diaphragm. This method ensures that all the cups are exactly the same size. With a little experience these tubes can be made at the rate of one every minute and a half.¹

(VI) SUMMARY.

The foregoing describes a new fermentation tube by means of which carbohydrates may be separated from proteins during sterilization and for the employment of which many advantages are claimed.

¹ They can be purchased from Baird and Tatlock, Hatton Garden, E.C.

Section of Pathology.

President—Professor J. C. G. LEDINGHAM, C.M.G., M.B., F.R.S.

The Disruptive Phenomena in Gunshot Injuries : their Physics.

By S. G. SHATTOCK, F.R.C.S., F.R.S.

IN the *Proceedings* of this Section, published in 1918,¹ I described at length and illustrated the chief effects produced in soft parts by gunshot, and pointed out that, whatever peculiarities such wounds present clinically, their proper pathological interest is limited to the fact that the velocity of the missiles produces special effects known as explosive, or better, as disruptive. In the present communication I propose to draw attention to the physics of the disruptive phenomena themselves.

In considering the subject it will be simplest to commence by way of exclusion.

(1) THE CONE OF COMPRESSED AIR ACCOMPANYING THE BULLET.

Professor Vernon Boys some years ago, as is generally known, succeeded in photographing bullets in full flight.¹ The bullets were mostly Lee-Metford, and the photographs were taken within a few feet of the muzzle of the rifle, when the velocity was 2,000 ft. per second. The bullet was fired so as to strike two leaden wires arranged almost in contact; on striking these the missile completed the circuit of a previously arranged electric system, and the spark so produced illuminated the photographic plate already exposed, the rifle being discharged, of course, in the dark. The photographs (of which a selection lately has been placed in the Royal College of Surgeons' Museum) show that the missile is accompanied with a conoidal wave of air, the difference in the density of which makes its outline susceptible of photography. The summit of the cone is rounded in correspondence with the nose of the Lee-Metford bullet but the compressed air immediately in front of the latter does not exceed 2.5 mm. ($\frac{1}{8}$ in.). From this the cone spreads out somewhat obtusely around and behind the projectile, its limit being ultimately too wide to appear in the photographs. Furthermore, in the immediate wake of the flat base of the bullet, and being about the same diameter as the latter, there are a series of irregular markings indicating the disturbance in aerial density due to the filling of the potential vacuum created by its flight.

¹ *Proceedings*, 1918, xi (Sect. Path.), pp. 47-118.

² *Nature*, March 2 and 9, 1893.

How far is the cone of compressed air responsible for the explosive effect produced upon a solid organ? It is tempting, at first, to attribute some importance to this, but in reality it is an absolutely negligible factor. In the past the displacement of air by a grazing bullet and, *par excellence*, a common ball was thought capable of producing considerable damage and was known as "windage." The results of one single method (devised by myself) of proving how innocuous the factor is, is shown in the accompanying figure. Screens of tissue paper, strained upon square frames of wood, 10 in. across, were shot through at a distance of 20 ft. with a service rifle and pointed bullet. The diameter of the hole in the paper is but little larger than that of the bullet—it measures 1.5 cm. as against 8 mm. Its margin is radially split in a remarkably regular way, and when the small triangular flaps, which were driven backwards, are replaced, the aperture becomes almost closed showing how little loss of substance has taken place owing to the pointed form of the bullet. Were the air-wave of any import, the paper would, obviously, have been as widely torn as the diameter of the cone of air. When the bullet is allowed to drop through

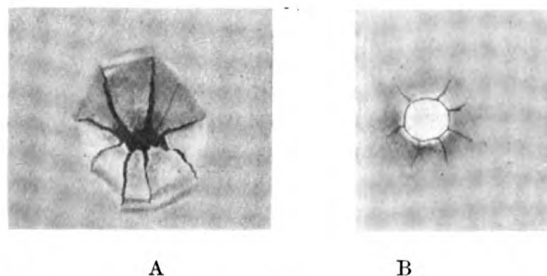


FIG. 1.—A (on the left), showing the hole produced in a tense sheet of tissue paper, by a pointed bullet fired from a service rifle at a distance of 20 ft. The perforation is but little larger than the diameter of the missile; proving that the air wave produced by the bullet is a negligible factor in the explosive phenomenon. The distance chosen, 20 ft., excludes any action of the gas of explosion. Natural size.

B, showing the aperture in the paper (after it has been soaked off the canvas backing) produced by the pointed bullet and service rifle when fired through the ordinary target. Natural size.

Figurae explicatio.—A, papyri charta tenuissima super frameam tensa et deinde perforata. Apertura vix major est quam missile perforans, quo demonstratur aeris undulam missili projectam negligi posse ut causam diruptionis quae in visceribus compectis reperitur. Magnitudinis naturalis.

B, papyri charta crassior quam illam praecedentem. Apertura magnitudine non excedit missile perforans. Magnitudinis naturalis.

the tissue paper, the latter is radially split and retroverted, the hole being exactly the diameter of the bullet. An examination of the paper targets used at rifle ranges shows the same thing. The paper is considerably stouter than tissue paper, and is moreover backed with canvas. The actual loss of substance produced by the present British bullet as measured in the paper after the latter has been soaked off the canvas is 0.4 cm. in diameter, the diameter of the bullet being 0.8 cm.; around this hole the paper is radially split and slightly retroverted, but only for a distance that will just allow of the passage of the bullet itself. The smaller extent of the radial splitting is of course due to the support afforded by the canvas backing.

The lightness of the air displacement was shown in another way by Professor Boys, by firing the bullet half an inch below a suspended sheet of gold leaf, without the latter being deflected.

(2) THE SPIN OF THE BULLET.

The late Sir Victor Horsley, in commenting upon the effects produced by firing unmantled cylindro-conoidal bullets (mostly miniature) at close range, directly into soft modelling clay, attributed the resulting cavitation of the clay chiefly to the rotation of the missile on its long axis, and in a secondary degree only to its velocity or rate of forward movement.¹ A series of specimens showing the result of the experiments was presented to the College museum by this distinguished neurologist and surgeon. The types of 0.22 ammunition made by different manufacturers vary in power. The long ammunition, as generally used, contains 4 to 7 gr. of powder, and the bullet weighs from 40 to 50 gr.; under these conditions the muzzle-velocity varies from 900 to 1,300 ft. per second. And there is a further series in the museum of University College. Of the cavities in the clay, plaster casts were made, and with one exception, where the clay mould is also preserved, these constitute the material for study.² A careful inspection of these different casts fails, in my opinion, to confirm



FIG 3.—A, British bullet fired into sand at a distance of not less than 100 yds. It shows fine parallel grooving on the apex due to the sand. The grooving presents no appreciable obliquity or spiral.

B, British bullet flattened and split open on the side from ricochet. The fine parallel striae on the apex run longitudinally as in the other specimen.

Figurae explicatio.—A, Missile Britannicum in arenae acervum projectum, super apicem monstrans strias minutas arenae impressas particulis. Hae striae nusquam nisi in longitudinem directae sunt.

B, Missile Britannicum. Super apicem monstrantur striae in longitudinem dispositae ut in figurâ alterâ.

the evidence of any such degree of rotation as would account for the result. Taking the results produced both by the miniature and the larger unmantled bullets the casts show the formation of a capacious tubular cavity for about the first half of the course of the missile. The distal portion of the cast does not exceed in diameter that of the bullet: unlike the rest it is smooth on the surface, and it represents the non-disruptive effects resulting when the resistance has reduced the speed of the projectile. Not infrequently the bullet turns in this part of its course in such a way as to travel side on, but without somersaulting, and is so found *in situ* at the end of the track. Under these circumstances the corresponding part of the track is of a flattened tape-like form. Without loading the matter with needless minutiae it will be enough to select one of these casts as typical of all.

¹ The miniature bullet rotates once in 16 in., in 0.22-bore rifles as used in 1918.

² *Brit. Med. Journ.*, November 21, 1914; and *Proc. R. Inst.*, iii, 1895, p. 228.

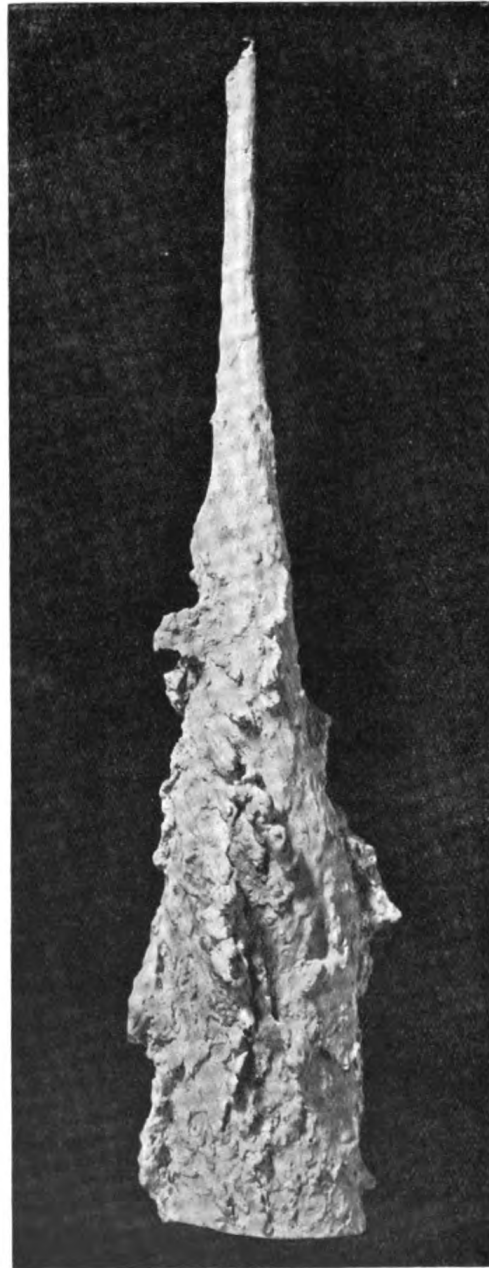


FIG. 2.—A photograph of a cast of one of the cavities produced in soft modelling clay by firing a miniature leaden bullet into it at close range. The ridges (which correspond, of course, with fissures in the clay) exhibit only small traces of a spiral disposition. The twist of the rifling is one in 14 in. The bullet can be recognized lying obliquely at the end of the narrow segment of the cast. The specimen (College of Surgeons) is reduced to half the natural size.

Figurae explicatio.—E cavitate in argillâ molli causatâ missili proxime projecto. Projiciuntur striae crassiores quarum dispositione probatur missilis rotationem negligi posse ut diruptionis causam.

On my putting the question to Professor Boys whether the *ratio* of forward movement and spin would be changed on impact he thought that if the missile proceeded side on the rotation might be relatively increased. But this complication of the question may be ignored. The rate of both movements is increased when the missile leaves the rifle, by reason of the sudden reduction in friction, seeing that the gas of explosion is under pressure throughout, otherwise it would have little propulsive effect, but it may be assumed that the ratio between the two remains the same.

Another means of estimating the rotation of the bullet after it has left the rifle is afforded by the examination of pointed missiles fired from the service rifle into *sand*. In many cases the British bullet breaks up under such circumstances, but even then the mantle over the intact apex may give a similar proof. A close inspection of the apex in the intact bullet will disclose fine parallel markings or scratches due to the particles of sand. On the apex the grooving cannot be confused with that due to the rifling, which is limited to the broader body of the missile and coarser in kind. If there is any obliquity in the sand-marking over the apex it is too slight to be appreciable.

(3) VELOCITY OR FORCE OF IMPACT.

The disruptive factor is thus reduced to that of forward velocity or force of impact acting upon a mobile but practically incompressible medium. A *lateral* movement is imparted to the medium from its intense compression in front of the rapidly advancing missile,¹ while compression is also exerted laterally since the conoidal point simultaneously acts as a wedge.

If we apply the foregoing considerations to the perforating injuries, say, of the liver, no admission of air behind the missile takes place in the body, there is no actual displacement of the surrounding substance as there is in the plastic clay; and this being so the wave of force arising from the compression of the parts in front of and at the sides of the advancing bullet results in radial splitting. That the element of spin (once in 10 in. in the older Lee-Metford) is a very subsidiary one, appears, also, from the short distance in which it may have to act. In traversing the thickest part of the right lobe of the liver from before backwards (9.7 cm., 4 in.) the bullet would make less than half a turn; the greatest oblique diameter of the right lobe from the lower margin upwards does not exceed 16.7 cm. (6½ in.). Nevertheless, disruptive effects are well pronounced in this viscus. In the experimental perforation of the right lobe of the isolated and suspended sheep's liver the thickness traversed was 1½ in. This would give a rotation of the bullet during its passage, of 0.15 of a turn; yet the explosive result was well pronounced.² In gutter wounds, if caused by missiles at high velocity, similar rending effects ensue, a gutter being practically a hemiperforation.

The Disruptive Phenomenon in the Skull.

It is in the skull that disruptive effects are witnessed in the highest grade. They are limited to cases where the perforation is bilateral, and where the missile is at high velocity. In the case of shrapnel balls the missile may perforate the skull on one side and be retained within the cranial cavity or become

¹ The barrel of the larger present rifle is 26 in. in length, the muzzle velocity being 2,380 ft. per second. The barrel of the shorter is 25¼ in., with a muzzle velocity of 2,440 ft.

² *Proceedings*, 1917-18, xi (Sect. Path.), pp. 55, 56.

embedded in the brain. But the velocity may be sufficiently high for a shrapnel bullet to cause bilateral perforation in a thin skull without disruptive effect. There are in the College collection thin-walled skulls from Omdurman (Sudan war) showing these things. In gunshot perforations of the cranium of the unilateral, low-speed kind, the perforation is accompanied with a certain amount of fissuring around, though this may be of very limited or even insignificant extent, and at the entry the inner plate is, of course, damaged in excess of the outer. A still lesser degree of injury, viz., the impaction of the missile at the stricken spot, is strikingly shown in a skull in the College collection, where a spherical musket ball projects from the back of the occipital bone; and where by the subsequent upgrowth of osseous tissue around its base it has become firmly and permanently fixed in position.

Whilst, finally, the minimal form of gunshot damage to the calvaria must be assigned to a graze which produces a fracture of the internal table without any of the outer, of which well-known condition there are examples from the late war contained in the collection at the College. It is also established by observation that the calvaria may be so lightly grazed as to escape any fracture whatever, but be nevertheless so concussed that the subjacent cerebral substance may be contused. As a curiosity there is (in the Museum of St. Thomas's Hospital) a femur (with a leaden bullet) which was struck at so low a velocity that the shaft of the bone is undamaged, whilst the bullet is flattened and fractured. When the skull is bilaterally perforated at close range, the extent of the fracture is surprising, and has not infrequently been observed in civil practice; for a pistol shot, if suicidal and so at close range, will produce similar effects. When the rifle or pistol is discharged quite close to the outside of the skull or through the mouth a second factor of damage is introduced, viz., the gas derived from the explosion. The potency of this is shown in cases where blank cartridge only has been used—the back of the skull may be blown away from within the mouth. In all the experiments superintended by myself the shooting was done at a distance of 20 ft., and this disturbing factor eliminated.

The disruptive results in through-and-through perforations of the skull depend upon:—

- (1) The high velocity of the missile.
- (2) The practical incompressibility of the semifluid cerebral substance.
- (3) The rigidity of the containing envelope.

The phenomenon, though still thought by some to be hydrostatic, is really a hydrodynamic one. It is not the result of a local compression caused by the advancing bullet acting equally in all directions upon an incompressible medium, confined in a completely closed and rigid space. Although the cranium is rigid, yet owing to the ready exit of blood from the brain by the sinuses, the actual cubic space occupied by the bullet during its perforation of the bone and brain would be easily equalled by the displacement of blood from the cranial cavity, and the occurrence of a proper hydrostatic effect forestalled.

That the disruptive effect is due to a wave of displacement is brought out by the following experiment, devised by myself, of firing at flat-sided tins, some in the empty state, and others filled with water, but all of them *freely open at the top*. The tins were 11 in. high and 7 in. across the front, and $5\frac{1}{2}$ in. across the side; the lids, which were hinged upon a removable straight rod of iron wire, being removed. They were shot through at a distance of 20 ft. with a service rifle and the ordinary pointed and mantled bullet.



FIG. 4.—Photograph of a cocoa tin 11 in. high and 7 in. by $5\frac{1}{2}$ in. which was filled with water and shot through at a distance of 20 ft. with a service rifle and pointed bullet, the lid of the tin having been first removed. The aperture of entry is clean and circular, the sides are bulged outwards, the corners being drawn in. The exit in the back is a great rent with radially torn borders, split in large triangular flaps which are widely everted; a result due to the propulsion of the water against the back accompanying the impingement of the bullet. Reduced to $\frac{3}{4}$.

Figurae explicatio.—Vas quadrilaterale ferro tenui constructum quod perforatum est postquam aqua impletum est, operculo prius ablato. Apertura introitus in pariete anteriori parva, rotunda; illa exitus, magna et dirupta, aqua contra parietem illum propulsa cum missili. Deminuitur ad $\frac{3}{4}$.

That which was empty showed a clean circular aperture of entry, and a precisely similar one of exit through the back. There is no splitting beyond either, and the burr around the posterior margin of each hole is so little that the result is only explicable by an equivalent amount of substance having been shot away. The other tin was filled with water, the lid, as before mentioned, being removed, and shot through, also at a distance of 20 ft. The aperture of entry is clear cut, without any surrounding splitting, exactly as in the case of the empty tin. The four sides are bulged out, the corners being drawn inwards, so that the shape approaches that of a cylinder; the bottom is likewise bulged downwards. The alteration in the top which (without the lid) is furnished with a broad flat rim, is of interest: the distal strip of the rim is bulged upwards, whilst that in front remains flat. The exit in the back of the tin is a great rent with radially torn borders, the metal being split in large triangular flaps, which are widely retroverted. A true hydrostatic effect is here out of question, since the vessel was freely open at the top. The wide rent in the back is explained by the propulsion of the water, practically inelastic and incompressible,¹ against it accompanying the exit of the bullet. The rate of the wave of displacement is equal to that of the missile, and as soon as the apex of the latter impinges against the tin the pressure of the water splits the latter radially from the weakened spot. The bulging upwards of the posterior part only of the rim around the top and not of the anterior, indicates also the main direction of the displacement. That the bulging outwards of the sides is not due to the negative pressure brought about by the sudden displacement and escape of water upwards is shown by the fact that it is the four flat sides and the bottom which are bulged, whilst the more resistant angles or corners are drawn in; the contrary would have been the case under the opposite condition of atmospheric pressure from without.

Corresponding results, *mutatis mutandis*, are obtained by firing at crania inverted and filled with water, *the foramen magnum being unclosed*—an observation which I have fully confirmed. The skulls (human) used by myself were soaked many days in water, and then made watertight by filling the fissures and sutures with plasticine; and in order to block the finer vascular canals the interior was thinly coated with plaster of Paris by swirling round a small amount of this inside, and pouring out the excess. They were then inverted and completely filled with water, the foramen magnum being left open. When shot through at a distance of 20 ft. with a service rifle and pointed bullet, they were literally blown to pieces, the fragments being widely scattered in the neighbourhood. On replacing the pieces so as to reconstruct the skull, much of the disruption was found to have taken place along the lines of the sutures. In human skulls similarly prepared, inverted and shot through in the empty state, there results a clean circular aperture of entry and a similar one of exit.

This experiment I carried out also on sheeps' skulls which had been partially macerated and cleaned externally, but with the *dura mater* left intact, so as to close the different apertures; the zygomatic arch of one side was sawn away so as fully to expose the side of the cranium, the part to be struck being marked with a circle of black chalk. In those turned upside down and shot through in the empty condition, two clean perforations, of entry and exit, resulted; there was extremely little fissuring of the thin bone around either. In those inverted and filled with water, *the foramen magnum being left*

¹ The slight elasticity of water connotes a certain amount of compressibility, but this is so little that for practical purposes it may be ignored.

open, the cranium was extensively comminuted, the connexion of many of the fragments with the rest of the skull being retained by the dura. First, a sheep skull was shot through under similar conditions, with the brain *in situ* and foramen magnum fully open. A remarkable amount of comminution ensued.

Punctured Fractures of the Skull.

The difference in design in the outer and inner tables of the calvaria in punctured fractures is too well known to need any description. The damage of the table last perforated exceeds that of the table perforated first. The greater compactness or rigidity of the internal table accentuates the difference in perforation from without inwards as against that from within outwards. The factor which chiefly leads to the more extensive fracture on the distal aspect and which is so obvious that any other is often overlooked, is the want of support on the side perforated last combined with the rigidity of the material concerned. The result of driving a nail through a panel of wood is familiar enough. But the following experiment upon the skull itself will be to the



FIG. 5.—Portion of an adult calvaria from which after death, a disc comprising the external table and diploë was removed as shown in A. In (B), the internal table was afterwards fractured from the outer aspect. The internal table was broken in excess of the circular defect, as shown in (C), where the parts are viewed from the inner aspect, proving that the excess is to be ascribed to want of support, the factor of displacement of material in front of the perforating object being eliminated. Natural size.

Figurae explicatio.—A, calvariae portio ab qua, post mortem resectus est discus e tabula externa cum diploë constans, tabula interna conservata. B, tabula interna fracta est ab extra fractura magnitudine aperturam circularem excedit quo probatur excessum illum ascribi insuper debere diminutione resistentiae apud tabulam postremo perforatam. C, easdem ut ab intra apparet.

point: On portion of a normal calvaria from a young adult, and which had at no time been allowed to dry, I trephined through the outer table and diploë; the bone so cut through was carefully gouged away so as to leave the inner table intact, and smoothly exposed. The flat head of a screw just fitting the trephine hole was placed in the latter against the inner table, and sharply struck through. The inner table, as a result, was split in excess of the circular aperture, into two fragments which remained hinged on as shown in the accompanying photograph, the lateral limit of each being determined by the presence of a meningeal groove.

The other factor can be studied separately only by experimental device: this is the displacement of material in front of the penetrating body. In the

following experiments, a four-sided flat-faced punch was used, the tabular bone being perforated gradatim (with strokes of a hammer) upon a flat plate of steel, so that the second factor—that of reduced support on the distal aspect—was eliminated. The effect of such displacement is shown in its simplest form when a sheet of malleable metal, such as lead, is punched upon a steel surface. The metal, without becoming fractured, is displaced so as to rise in a low circular eminence on the under side; the centre of the area is closed with a thin film of the metal corresponding in area with the flat end of the punch. If the procedure is continued still further, the quadrilateral film becomes detached at its edges and broken into further fragments. The displacement of the metal results in the production of a "burr" around the aperture.

In the case of the calvaria, if the perforation is carried out in the way first described, upon a resisting surface of steel, a burr is produced; but seeing that the material is not malleable, the burr consists of displaced and tightly compressed débris of bone. The following will explain the mechanism of its production, as carried out upon the calvaria of a child, 3 mm. in thickness,



FIG. 6.—Showing the nearly complete perforation of a sheet of lead by means of a four-sided solid punch used against a flat surface of steel. The displacement of the malleable metal has led to the formation of a circular "burr"; a thin quadrilateral film of metal still remains intact. A, as seen from the lower aspect. B, in vertical section. Twice the natural size.

Figurae explicatio.—Plumbi lamina quae clavo perforata est, tabulam super ferream. Monstratur eminentia circularis aspectu inferiori, plumbi expressioni sine fracturâ causata. In mediâ eminentiâ lamella quadrata aperturam adhuc obturat. A, Ut apparet ab infra. B, in sectione verticali. Figurae bis magnificantur.

and which had at no time been allowed to dry. The four-sided, flat-faced punch was driven from the inner aspect outwards, so that the table last perforated should be fully supported against the flat surface of steel underneath. In the process of penetration there is first produced on the lower side, a circular elevation, the outer table over which is intact. If the parts are examined at this stage by means of a vertical section, after the bone has been dehydrated in alcohol, passed through xylol, and embedded in paraffin, a quadrilateral segment of the outer table is found driven forwards, and the subjacent diploë crushed. As the penetration proceeds the elevation increases, and the external table over it becomes radially fissured. Finally the eminence becomes broken into small fragments, the "burr" so produced consisting superficially of minute pieces of the outer table, some of them still hinged on, and more deeply, of displaced fragments of the diploë and internal table. But

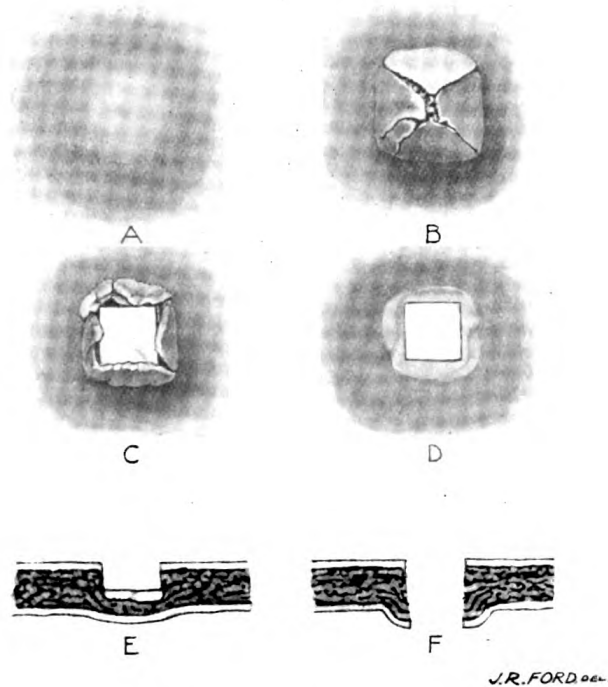


FIG. 7.—Showing stages in the perforation of the calvaria of a child, by means of a solid four-sided flat-ended punch, the bone being supported upon a flat plate of steel. A, B, C, D, are viewed from the lower aspect; E, F, in vertical section. The punch was driven from the inner table outwards.

A.—The formation of a low hemispherical swelling due to the displacement and compression of the inner table and diploë.

B.—The outer table radially split over the eminence.

C.—Perforation complete, showing the local eminence or burr, formed superficially of everted fragments of the outer table, the debris closing the hole being displaced by the forward movement of the punch.

D.—The burr removed by pressing a scalpel against the four sides of the punch; the circle marks the amount of loss due to the removal of the burr. This loss is quite local, there being no further fracture due to the want of support which obtains under natural conditions, as the punch was used against a flat surface of steel.

E.—Vertical section; the punch about halfway through; showing the displacement of a piece of the internal table, and compression of the diploë.

F.—Vertical section; complete perforation; showing fracture of the outer table and its eversion, with displacement of the debris above; so as to form a burr.

All the figures are enlarged twice.

Figurae explicatio.—Monstrantur gradus in calvaria pueri post mortem perforanda. Perforata est calvaria tabulam super ferream, clavo percusso contra laminam cranii internam.

A.—Gradus primus. Eminentia circularis apud laminam ossis externam, compressione laminae internae atque diploëdis causata.

B.—Gradus secundus. Eminentia eadem, lamina externa in radios fissa.

C.—Gradus tertius. Perforatio laminae completa; laminae externae fragmenta eversa juxta aperturam.

D.—Perforatio completa; ossis fragmentorum eversorum, circulo absciso.

E.—Sectio verticalis. Perforatio incompleta. Monstrans eminentiam circulearem compressione laminae internae atque diploëdis causatam.

F.—Sectio verticalis; annulum fragmentorum laminae externae atque diploëdis circum aperturam monstrans.

In his omnibus casibus, calvariâ tabulam super ferream resistentem perforatâ, fractura ad marginem perforationis limitatur.

Bis magnificantur omnes figurae.

there is no splitting of the external table beyond the burr, owing to the support afforded by the steel surface beneath the bone. When the burr is removed by pressing a scalpel horizontally against the sides of the punch, the osseous débris around the latter is found compactly pressed together, the loss of substance in the external table being circumscribed by a subcircular line corresponding with the limit of the burr. On pushing the punch completely through, the central part of the débris is displaced and the opening cleared. The aperture in the internal table, i.e., of entry, remains perfectly sharp, and without surrounding fissure: no escape of material takes place backwards, i.e., behind the punch. As applied to punctured fractures of the calvaria occurring under natural conditions, however, the displacement of material in front of the penetrating instrument is a factor of minor importance in producing the disparity of damage on the two aspects of the bone: for the puncture cannot take place against a resisting surface like that furnished under the conditions of the experiment. The factor is overshadowed by that of the want of support on the aspect last perforated.

Gunshot Fractures of Long Bones.

The pathological interest attaching to gunshot fractures of the long bones is so small that it is not worth taking up. It will be enough to point out one thing, viz., that a new element is introduced, which is the rigidity of the structure damaged; though the same rule holds good elsewhere—the greater the velocity of the missile, the greater the damage. In the accompanying photograph is shown the splitting produced in a tibia by *slowly* compressing a limited part of its shaft in a parallel vice: the bone used had not been at any time allowed to dry, and its brittleness in this way increased. The longitudinal direction of the main fissures is determined by the “grain” of the bone due to the direction of the Haversian canals: the fissures tend to run in the planes of least resistance.

In compact tissue, the local comminution, apart from the distant fissuring, is very pronounced if the shaft is fairly struck at close range. This is well shown in some of Sir Victor Horsley's experiments (now in University College Museum), where the shafts of the tibia and of the femur of a dead pony were shot point blank. In the tibia the bone at the stricken spot is broken into fragments so numerous and small that it looks as if roughly powdered, and from this fissures extend for considerable distances in both directions; and much the same has happened in the femur. The result is somewhat comparable to that obtained by Professor Boys on firing through a sheet of plate glass, where the photograph shows a column of glass dust accompanying and concealing the Lee-Metford bullet after its perforation. The lateral and forward displacement of fragments around the advancing missile necessarily results in an increased amount of damage to the soft parts of a limb. Short of a stereoscopic skiagram, a good method of displaying the true state of things is that of laying open the limb through the seat of fracture, or after being hardened in Kaiserling fluid, the soft parts may be cut away from one side and the fragments embedded in the muscles displayed individually.¹ But for displaying the actual damage and secondary fissures, many of which would otherwise escape detection, the time-honoured method of maceration retains its value. The forcible displacement of such fragments into the muscles is an important factor in producing the protrusion of the latter through the cutaneous exit, which under such circumstances may be extensively split.

¹ See figure 12 at end of paper.



FIG. 8.—The upper half of a tibia which was slowly compressed in a vice, over the area indicated by the V line. The shaft is extensively split, some of the fissures reaching far from the spot compressed. Reduced.

Figurae explicatio.—Tibiae pars superior quae in machinâ compressa est apud locum lineis indicatum: ossis diaphysis late comminuitur, fissuris extendentibus longe a loco compresso. Deminuitur.

The British Rifle Bullet.

It has been stated¹ that the British bullet, in consequence of the pointed end of the cone being of aluminium, and the rest of lead, has an explosive action; that on striking (bone at least) the momentum of the softer metal would carry this forwards upon the aluminium, and split up the mantle, seeing that the two segments of the core are discontinuous. It is next to impossible to estimate the relative vulnerability of the British and German bullets with accuracy from their examination when taken from the wounded, since ricochet, the striking of the missile against accoutrement, gas masks, &c., furnish such abundant sources of damage to them.

The following experiments show that it is only on meeting with a high

¹ Professor Dr. K. Stargardt, *Münch. med. Wochenschr.*, 1914, lxi, p. 2448.

resistance that the British bullet undergoes fragmentation.¹ They were made by firing through bone with a service rifle and pointed bullet at a distance of 20 ft., into cotton waste. In order to get a direct hit I selected well-developed adult sterna, which had not been allowed to dry after removal from the body; and for denser bone the calvaria. A sternum was screwed by the ends, over an aperture in the front of a long box: behind the aperture was placed a cylindrical sack about 2 ft. in length, tightly packed with cotton-waste, beyond which it was first ascertained that the bullet would penetrate; behind the sack the box was filled with a mass of loose waste, in which the missile was effectively stopped without striking any second object.

(1) A sternum was shot through exactly in the middle of the gladiolus at 20 ft. The bullet recovered from the loose cotton waste was quite undamaged.

(2) Two other sterna were then closely spliced together, and shot through exactly in the mid-line. The bullet on recovery was, as in the first case, absolutely intact.

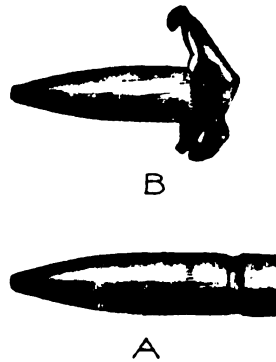


FIG. 9.—A, British bullet fired at a distance of 20 ft., through a parietal bone of medium thickness (0·6 cm.): the missile is intact.

B, British bullet fired through a thicker and denser parietal bone (0·9 c.m.), and received, like the foregoing, into cotton waste. The mantle at the base is longitudinally split and everted; the lead and aluminium cone have been evacuated. Natural size.

Figurae explicatio.—A, missile Britannicum, quod os parietale, in crassitudine (0·6 cm.) perforaverat missile intactus est.

B, missile Britannicum quod os parietale crassius atque densius quam praecedens perforaverat (0·9 c.m.). Apud basem missilis cortex in longitudinem scissus et eversus est, plumbo ab intra expanso. Magnitudinis naturalis.

In order to test the effect upon more resistant bone, a similar experiment was carried out upon the parietal bones of two human calvariae. One of these from a male, aged 56, measured, where perforated, 0·6 cm. in thickness, and was a fair sample of an ordinary male skull; the other measured 0·9 cm., and, without being diseased, was of high density, the tables being thick and the diploë close. They had at no time been allowed to dry after removal from

¹ The arrangements for carrying out these and other experiments were kindly made for me at the Acton range; and at the Polytechnic Institution (21st Battalion, County of London Volunteers Regiment: Officer Commanding, Major J. E. K. Studd): Staff Sergeant David Pool, an expert marksman, who did the shooting with unfailing accuracy.

the body. The portion shot through was the flatter area, the bone being turned with the internal surface forwards. The bones were cleanly perforated at right angles to the surface, at a distance of 20 ft., the bullets being recovered from the cotton waste, through which no previous or subsequent shooting had been carried out. In the thinner calvaria the entry in the inner table was sharp, circular, of the exact diameter of the bullet; the bone around the exit was comminuted and shot away for a circular area 1.5 cm. in diameter. The bullet was quite undamaged except for a slight flattening of its apex. In the case of the thicker calvaria, the actual perforation was slightly larger than the bullet, and not geometrically circular. The inner table around the entry was comminuted and wasting over an area about 1.5 cm. in diameter. On the opposite aspect, the comminution and loss around the perforation extended over an area about 2.5 cm. in diameter. The bullet: the base of the mantle is split as far as the second of its transverse grooves (1 cm.) and everted; the aluminium cone and lead are evacuated.

Not to labour the matter, it is clear that the basal expansion and splitting of the mouth have occurred on striking (the apex of the bullet is flattened) and

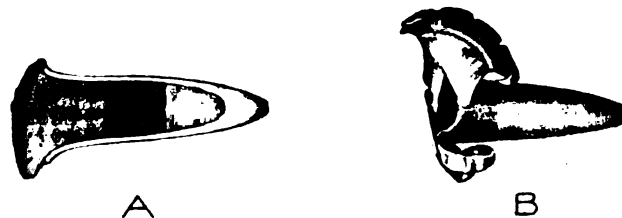


FIG. 10.—A, section of British bullet, showing early expansion or mushrooming of the lead at its base and a resulting uniform recurvation of the free edge of the mantle, without actual splitting. The accurate flattening of the apex shows that a direct hit was made. From the seat of war in France.

B, a British bullet of which the mantle is deeply split and recurved from the base towards the apex. The apex is accurately flattened showing that a direct hit was made. From the seat of war in France.

Figurae explicatio.—A, Missile Britannicum cujus cortex apud basem recurvatum sine scissuris est, plumbo expanso. Gradus diruptionis primus.

B, Missile Britannicum cujus cortex apud basem valde in longitudinem scissa est. Magnitudinis naturalis.

not after perforation, since the distorted mantle accurately fits the irregular entry and can only be passed through by manœuvring the missile, with a slight obliquity.

Is the Leaden Core Fluid when the Bullet leaves the Rifle?

That the older leaden British unmantled bullet, as fired from the Martini-Henry rifle, did not soften in its flight was shown by the fact that after being fired at steel targets, the base remained intact and with it the circular groove close above, the rest becoming mushroomed and fractured. But of the modern, high-velocity, mantled missile, it has been stated that at the time it leaves the

rifle the lead is in a molten state, from the heat generated by the rifling and that of the explosive acting directly upon it. This assertion has been as confidently denied.

The question is of sufficient interest to make it worth inquiry. The particular method devised by myself was as follows: The service bullet was removed from its cartridge; this can be done by placing the latter in a vice, tapping the point of the bullet with a hammer so as to drive it behind and obliterate the indented parts of the cartridge which hold it in position, and then withdrawing it from behind forwards. The bullets used were of American make, in every way like the British except that there were two circular grooves towards the base instead of one; the exposed lead was stamped in low relief, U S. Into the centre of the lead a short channel was clearly drilled out 0.5 cm. in length. The bullet was replaced in its cartridge, and fired, at a distance of 20 ft. directly into the box of cotton waste, from which it was afterwards recovered. Were the lead at any moment molten, the drilled channel would, of course, become obliterated. On filing the bullets from the side, the channel was found quite unobliterated. Although it is clear that the lead is at no moment molten throughout, there is nevertheless sufficient heat generated by the explosion to melt the face of the core exposed at the base. This appears from the fact that the raised letters U S in the base of the unfired bullet, are invariably found obliterated when the missile is recovered in the intact state from cotton waste. The pressure within the barrel is one expressed not in pounds but in tons; but seeing that this would be equal at every point upon the base of the lead the stamp would not be obliterated from this cause alone. The melting point of lead is $377^{\circ}\text{C}.$: the heat generated by the explosive now used, viz., cordite, is about $4,000^{\circ}\text{C}.$; and the metal being a bad conductor of heat, its exposed surface is melted without the whole mass.

Effects of Compression upon the British Bullet.

The following observations were made to see whether "expansion" at the junction of the aluminium and leaden portions of the core resulted from compression:—

(1) A bullet was placed in a powerful parallel vice, i.e., one in which the jaws approach in a perfectly parallel direction, and slowly compressed from side to side until quite flattened. No splitting of the mantle occurred either longitudinally or transversely at the junction of the aluminium and lead of the core; a considerable amount of the lead was expressed through the open base.

(2) A bullet was placed obliquely from point to base at an angle of 45° . Under pressure the point and base became flattened obliquely to the proper axis of the bullet, but without any splitting of the mantle or distortion occurring elsewhere.

(3) A bullet was compressed directly, point to base, the egress of the lead from the base being in this way quite prevented. After slight flattening of the point and base it began to curve; the compression was carried on until the curve had almost reached a semicircle. No splitting of the mantle occurred; and no lateral protrusion of the included lead at the line of its junction with the aluminium cone. This observation was repeated with precisely the same result. The longitudinal section made of one of the last two showed that no lateral expansion of the lead upon the aluminium had taken place, the deformity being due to flexion of the lead and invagination of the otherwise intact mantle on the concave side.

To sum up: Although the British bullet is not designed (like the Jeffrey¹) to expand—the aluminium being used in order to carry the centre of gravity nearer to the base and reduce oscillation in flight; nevertheless the want of continuity between the two parts of the core theoretically weakens the bullet at the plane of apposition. Point to base compression, however, is not accompanied with mushrooming of the lead over the harder aluminium cone and splitting of the mantle at this spot. The bending which is started by slight deviation in the axis, proceeds, without splitting of the mantle, and extrusion of the core taking place even when the curvature is carried to a semicircle. Nevertheless, it must be admitted that a certain amount of vulnerability results from the fact that the British mantle is of ductile copper-nickel alloy and not of steel. The thickness of the British mantle is 0.5 mm.; that of the German (steel, plated with nickel), 0.7 mm.; in the latter, too, the amount turned in over the base of the lead is slightly greater. The diameter of the two missiles is identical. The difference in the resistance of the two mantles, I was able to show by having the weight which was necessary to start their splaying out, measured. Two equal lengths were tested after extraction of the core (no heat being used for this purpose). A steel ball $\frac{5}{16}$ in. in diameter was placed on the opened end of each of the two mantles:

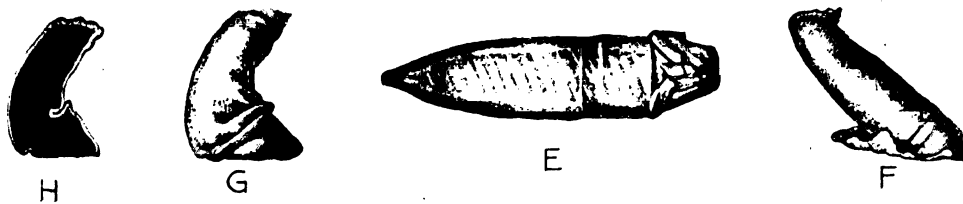


FIG. 11.—E, British bullet pressed flat in a parallel vice. No protrusion of the leaden core and splitting of the mantle have occurred over the site of the aluminium cone. Some of the lead is expressed from the open end of the mantle. G, British bullet compressed from point to base until curved to a semicircle. H, Section of the same; no protrusion of the lead over the aluminium has taken place, nor splitting of the mantle. F, British bullet compressed from point to base at an angle of 45°. No distortion at the junction of the aluminium and the leaden core has resulted.

Figurae explicatio.—Missilia Britannica in machinâ compressa. Deformantur omnia sine fractura. E, missile a latere compressum. F, oblique. G, in axe verticali. H, sectio verticalis missilis. (G) aluminiumi conus intactus, et cortex in plumbum involutus.

the British bullet had not been fired, the German had. The load was increased 11 lb. at a time. The British mantle began to yield under a load of 180 lb.; the German mantle, under a load of 390 lb.

What happens in the case of the British rifle bullet is, that on direct impact against a high resistance, the comparative weakness of the mantle allows the lead to splay out, and to split the mantle at its free edge over the open base. Thence the splitting may extend, the strips being curved outwards and displaced towards the apex, somewhat like the sepals of a fuchsia. The actual lines of fissure may be started by the grooves in the mantle due to the rifling. The percussion of the loosened lead against the harder aluminium may then lead to the expulsion of the former through the widely open base of the mantle. The expansion in a word, takes place, not apically, but basally.

¹ In this bullet the mantle is split in a series of longitudinal lines between the apex and base, which the momentum of the leaden core opens out on the missile striking.



FIG. 12.—A dissection of the thigh made after hardening of the parts in Kaiserling solution, showing the displacement of the fragments of a comminuted gunshot fracture of the femur, into the surrounding muscles. From a preparation by Mr. C. F. Beadles, in the National War Collection in the Royal College of Surgeons.

Figurae explicatio.—Cruris dissectio, telis prius induratis, monstrans femoris fragmentorum dislocationem in musculos circumjacentes missili causatam.

Section of Pathology.

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On the Inheritance of the Specific Isoagglutinable Substances of Human Red Cells.

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With a Note on the Possible Existence of a Lethal Factor.

By S. C. DYKE.

THE observations forming the basis of the present paper were undertaken with the view of testing the hypothesis formulated by von Dungern and Hirschfeld (1910) [1] as to the inheritance of the specific agglutinable properties *A* and *B*, the presence of which in, or absence from, the human red cells determines the blood group of the individual. The authors made observations on 384 individuals, constituting seventy-two families, and came to the conclusion that the properties *A* and *B* can never appear in the offspring without having been present in at least one of the parents, and furthermore that, when inherited, these properties appear in the offspring in accordance with recognized Mendelian laws.

HISTORICAL.

The fact that all of a series of bloods observed by him might be divided into groups according to the interaction of their sera and corpuscles was demonstrated by Landsteiner (1901) [2]. He described three groups corresponding to what are now known in this country as Groups II, III, and IV. Group I, being rare, was not encountered by him. To account for these groups he pointed out that it was necessary to postulate only two agglutinable properties in the corpuscles, which he called *A* and *B*, and two corresponding agglutinins in the serum, which may be designated *a* and *b*, though these designations were not used by Landsteiner. His three groups then would have their constitutions as follows :—

	Corpuscles				Serum		
Group II	<i>A</i>	...	<i>b</i>
Group III	<i>B</i>	...	<i>a</i>
Group IV	<i>O</i>	...	<i>a</i> and <i>b</i>

Landsteiner observed that the blood group of a child might be quite different from that of its mother.

Decastello and Sturli (1902) [3] confirmed these findings, but found four persons in their series of 155 who fell into none of Landsteiner's three groups. From the paper it is obvious that these four persons belonged to what is now known in this country as Group I, but the significance of their observations was not appreciated by these workers. They were the first to demonstrate by absorption tests the correctness of Landsteiner's hypothesis so far as the three then recognized groups were concerned.

It was not until another five years had elapsed that the significance of Decastello and Sturli's four unplaced individuals was recognized by Jansky (1907) [4]. In a series of his own, he met with similar bloods which could be allocated to none of Landsteiner's three groups, and recognized that in them he was dealing with blood of the constitution—

Corpuscles *A* and *B*. Serum *O*.

This group he described as No. IV. Since his time no further type of blood has been observed, and it would seem, on the present evidence, that the blood of all human beings falls into one of the four groups which Jansky described as follows:—

					Corpuscles		Serum	
Group I	<i>O</i>	...	<i>a</i> and <i>b</i>	
Group II	<i>A</i>	...	<i>b</i>	
Group III	<i>B</i>	...	<i>a</i>	
Group IV	<i>A</i> and <i>B</i>	...	<i>O</i>	

This classification places as Group I what is usually known in this country as Group IV and vice versa. This is due to the fact that here the nomenclature adopted by Moss (1910) [5] is still in vogue. Moss recognized the four groups of blood independently of Jansky, but explained their constitution in a different manner.

Moss's hypothesis postulates three agglutinable substances in the corpuscles and three corresponding agglutinins in the serum. It may be expressed schematically as follows:—

					Corpuscles		Serum	
Group I	<i>A, B, C</i>	...	—	
Group II	<i>B, C</i>	...	<i>a</i>	
Group III	<i>A, C</i>	...	<i>b</i>	
Group IV	—	...	<i>c</i>	

Moss's suggestion is, on the face of things, as good an explanation of the interaction of the corpuscles and sera of the various groups as that of Landsteiner. The readiest means of ascertaining which of the two suggestions is correct is by the absorption of the agglutinins of the sera by their appropriate corpuscles. The test case is that of Group I (Jansky), IV (Moss). According to Landsteiner and Jansky serum of this group should contain the agglutinins *a* and *b*; absorption of this serum by corpuscles of Group II bearing the agglutinable substance *A*, or of Group III bearing the substance *B*, should in each case leave one agglutinin behind; in the first case *b*, and in the second *a*, while absorption by corpuscles of Group I (Moss), IV (Jansky), bearing both agglutinable substances *A* and *B* should remove all agglutinins. According to Moss's schema, all the agglutinin *c* should be removed from serum of Group IV (Moss), I (Jansky) by absorption either by corpuscles of Group I (*A B C*), II (*B C*), or III (*A C*).

Experiments by Hektoen (1907) [6], Koeckert (1920) [7], Schütze (1921) [8] and Dyke (1922) [9] have amply confirmed for this and all other cases the correctness of the view of Landsteiner and Jansky as to the constitution of the groups. Although Jansky's classification has the priority, that of Moss has hitherto been in use in this country, and has been used by one of us (Dyke) [9], [10], [15] in previous communications. It is accordingly retained in this paper.

From what has been said above it is evident that in their appearance in the blood, the agglutinable properties *A* and *B* and their respective agglutinins *a* and *b* behave as reciprocals. When *A* alone is present in the corpuscles *b* alone appears in the serum, as in a Group II blood; when *B* appears in the corpuscles, *a* is present in the serum, and the result is a blood of Group III; when both *A* and *B* are present in the corpuscles, as in Group I, neither agglutinin appears in the serum: and when both are absent from the corpuscles, as in Group IV, both agglutinins appear in the serum.

RATIONALE OF GROUPING TESTS.

On these facts is based the method of determining the presence or absence of the agglutinable properties *A* and *B* of the corpuscles, and consequently the blood groups, in any case. For this purpose sera of known Groups II and III only are required. Since serum of Group II bears the agglutinin *a* and that of III *b*, it follows from the above that corpuscles of Group I will be agglutinated by both the II and III sera; those of Group II by the III serum only; those of Group III by the II serum only, and those of Group IV by neither. Certain details as to the actual technique are given below.

THE FACTORS IN INHERITANCE.

Von Dungern and Hirschfeld, on studying the inheritance of Landsteiner's agglutinable properties *A* and *B*, found that they never appeared in the offspring without having been present in the parents. On their evidence they concluded that they behaved as Mendelian dominants. As in the case of Mendel's sweet peas, the dominant Long formed one half of an allelomorphic pair with the recessive Not-Long or Short, so they concluded that *A* and *B* constituted each one half of an allelomorphic pair, of which the other halves were Not-*A* and Not-*B*. But they pointed out that Not-*A* and Not-*B* are not negative qualities but, on the contrary, are represented by the reciprocal agglutinins of the serum, called by them α and β , but referred to hereafter as *a* and *b*. Thus, as in the case of Mendel's sweet peas, an individual showing the dominant Long might still bear, hidden in its gametic complex, the recessive Short, and might thus have short offspring, so an individual showing the dominant *A* (Group II) might still bear, hidden in his or her gametic constitution, the recessive *a*; if present in such an individual, this would not be expressed in his serum, but might appear in the offspring.

For a full understanding of this point, it is necessary to bear in mind the difference between an individual and a gamete. A Group II individual has the formula Corpuscles *A*, Serum *b*. Granted that this individual is of a pure race or homozygous, segregation of the factors in the production of the gametes will proceed in such a way as to produce gametes of the formula *Ab* only. Supposing, however, that the individual is not homozygous, but hybrid or heterozygous, then the recessive *b* will be present but masked in the individual

by the dominant A . The expressed formula of such an individual will be Ab , but the actual formula will be $Ab(a)$, and in formation of the gametes segregation will take place in such a way as to produce gametes of the formula Ab and ab . Matings of two such Group II individuals may give rise to zygotes of the formulæ $Ab\ Ab$ (homozygous Group II), $Abab$ (heterozygous Group II) or $abab$ (Group IV).

Learmonth (1920) [11] made observations on 180 individuals belonging to forty families, and found only one exception to von Dungern and Hirschfeld's hypothesis. Learmonth, in accordance with usage of workers on heredity, designated the dominant agglutinable properties A and B and their allelomorphic recessives a and b , this being the nomenclature adopted in the present paper.

Further confirmatory evidence has been produced by Ottenburg (1921-2) [12] [13] in America.

AUTHORS' DATA.

The observations forming the basis of the present communication were made upon material provided by the maternity ward of St. Thomas's Hospital. With the co-operation of the nursing staff, blood was collected from the umbilical cords of infants at birth and used for ascertaining the group of the child. Corpuscles for the same purpose were obtained from the mothers as they lay in the wards, and from the fathers on their visits. In this way observations were conducted upon both parents and offspring in ninety-eight cases.

The blood for ascertaining the groups of the infants was collected in a test tube and allowed to clot. The serum was then pipetted off and the corpuscles washed three times in a 2 per cent. sodium citrate solution in normal saline. Blood was obtained from the parents by finger puncture, the drop being collected in similar citrate solution. The serum of the parents was not tested, the group being determined by the reaction of the corpuscles alone.

The actual grouping tests were performed, with slight modifications, in the manner already described by one of us (Dyke [9], [15]). This involves making a dilution of approximately 1 in 200 of a 50 per cent. corpuscular emulsion in the saline citrate solution and mixing one drop of this with one drop of the serum against which the corpuscles are to be tested. A cover-glass preparation is made from this corpuscle serum mixture and examined under the low power of the microscope. No originality is claimed for this technique, but it is believed to give infinitely finer and more clear-cut results than the macroscopic method. The data collected in the present investigation are detailed below (p. 39).

CONSTITUTION OF BLOOD AT BIRTH.

Many writers have pointed out that while the agglutinable properties may be demonstrated in the corpuscles of infants at birth, the reciprocal agglutinins may be absent for some months. This is quite evident in the data given in the above table. Of the thirty-seven babies in the series identified by the reaction of their corpuscles as belonging either to Group II or III, only nine, or about twenty-five per cent., had demonstrable agglutinins in their serum which would have made it possible, by evidence derived from that source alone, to allocate them to their proper groups. Of the fifty-nine babies whose corpuscles were agglutinable by either Group II or III serum, and which were consequently

TABLE I.

Case number	Placental Blood												Baby's group	
	Group		Red cells				Serum							
			+ Serum II		+ Serum III		+ Red cells II		+ Red cells III					
			Father	Mother										
<i>Mating Type I + II.</i>														
91	...	I	...	II	...	—	...	—	...	—	...	—	...	IV
<i>Mating Type I + IV.</i>														
12	...	IV	...	I	...	+	...	—	...	—	...	—	...	III
<i>Mating Type II + II.</i>														
38	...	II	...	II	...	—	...	+	...	—	...	—	...	II
45	...	II	...	II	...	—	...	—	...	—	...	—	...	IV
54	...	II	...	II	...	—	...	+	...	—	...	—	...	II
59	...	II	...	II	...	—	...	+	...	—	...	—	...	II
63	...	II	...	II	...	—	...	+	...	—	...	—	...	II
77	...	II	...	II	...	—	...	+	...	—	...	—	...	II
78	...	II	...	II	...	—	...	—	...	—	...	—	...	IV
90	...	II	...	II	...	—	...	+	...	—	...	—	...	II
100	...	II	...	II	...	—	...	+	...	—	...	—	...	II
103	...	II	...	II	...	—	...	+	...	—	...	—	...	II
115	...	II	...	II	...	—	...	+	...	—	...	—	...	II
122	...	II	...	II	...	—	...	+	...	—	...	—	...	II
125	...	II	...	II	...	—	...	—	...	—	...	—	...	IV
132	...	II	...	II	...	—	...	+	...	—	...	—	...	II
134	...	II	...	II	...	—	...	+	...	—	...	+	...	II
137	...	II	...	II	...	—	...	—	...	—	...	—	...	IV
<i>Mating Type II + III.</i>														
13	...	II	...	III	...	+	...	—	...	+	...	—	...	III
68	...	III	...	II	...	+	...	+	...	—	...	—	...	I
70	...	II	...	III	...	—	...	—	...	—	...	—	...	IV
83	...	III	...	II	...	—	...	+	...	—	...	—	...	II
136	...	II	...	III	...	+	...	+	...	—	...	—	...	I
<i>Mating Type II + IV.</i>														
15	...	II	...	IV	...	—	...	+	...	—	...	—	...	II
16	...	IV	...	II	...	—	...	+	...	—	...	—	...	II
19	...	II	...	IV	...	—	...	+	...	—	...	—	...	II
21	...	IV	...	II	...	—	...	—	...	—	...	+	...	IV
26	...	IV	...	II	...	—	...	+	...	—	...	+	...	II
27	...	II	...	IV	...	—	...	—	...	+	...	±	...	IV
28	...	II	...	IV	...	—	...	+	...	—	...	—	...	II
29	...	II	...	IV	...	—	...	—	...	—	...	—	...	IV
31	...	IV	...	IV	...	—	...	+	...	—	...	—	...	II
32	...	II	...	IV	...	—	...	+	...	—	...	—	...	II
33	...	IV	...	II	...	—	...	—	...	—	...	—	...	IV
43	...	IV	...	II	...	—	...	—	...	—	...	—	...	IV
44	...	II	...	IV	...	—	...	—	...	—	...	—	...	IV
47	...	IV	...	II	...	—	...	—	...	—	...	—	...	IV
52	...	IV	...	II	...	—	...	+	...	—	...	—	...	II
62	...	IV	...	II	...	—	...	—	...	—	...	—	...	IV
74	...	II	...	IV	...	—	...	—	...	+	...	—	...	IV
75	...	II	...	IV	...	—	...	+	...	—	...	—	...	II
76	...	IV	...	II	...	—	...	+	...	—	...	—	...	II
101	...	II	...	IV	...	—	...	+	...	—	...	—	...	II
105	...	II	...	IV	...	—	...	—	...	+	...	—	...	IV
107	...	II	...	IV	...	—	...	+	...	—	...	+	...	II
108	...	II	...	IV	...	—	...	—	...	—	...	—	...	IV
109	...	IV	...	II	...	—	...	—	...	+	...	—	...	IV
113	...	IV	...	II	...	—	...	—	...	—	...	—	...	IV
116	...	II	...	IV	...	—	...	+	...	—	...	±	...	II
120	...	II	...	IV	...	—	...	+	...	—	...	±	...	II
123	...	IV	...	II	...	—	...	+	...	—	...	—	...	II
124	...	IV	...	II	...	—	...	—	...	—	...	+	...	IV
126	...	IV	...	II	...	—	...	—	...	—	...	—	...	IV
129	...	II	...	IV	...	—	...	—	...	—	...	—	...	IV
133	...	II	...	IV	...	—	...	—	...	—	...	—	...	IV

TABLE I—Continued.

Case number	Placental Blood												Baby's group	
	Group		Red cells				Serum							
			+ Serum II	+ Serum III	+ Red cells II	+ Red cells III								
	Father	Mother												
<i>Mating Type III + IV.</i>														
25	...	IV	...	III	...	{ —	...	—	...	+	...	+	...	IV.
42	...	IV	...	III	...	{ +	...	—	...	—	...	—	...	III
53	...	III	...	IV	...	—	...	—	...	—	...	—	...	IV
65	...	IV	...	III	...	+	...	—	...	—	...	—	...	III
69	...	IV	...	III	...	—	...	—	...	—	...	—	...	IV
89	...	IV	...	III	...	+	...	—	...	+	...	—	...	III
94	...	III	...	IV	...	—	...	—	...	—	...	—	...	IV
96	...	IV	...	III	...	+	...	—	...	—	...	—	...	III
112	...	III	...	IV	...	+	...	—	...	—	...	—	...	III
114	...	III	...	IV	...	+	...	—	...	±	...	—	...	III
118	...	III	...	IV	...	—	...	—	...	—	...	—	...	IV
135	...	III	...	IV	...	+	...	—	...	+	...	—	...	III
<i>Mating Type IV + IV.</i>														
14	...	IV	...	IV	...	—	...	—	...	±	...	±	...	IV
17	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
24	...	IV	...	IV	...	—	...	—	...	+	...	+	...	IV
34	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
35	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
36	...	IV	...	IV	...	—	...	—	...	+	...	+	...	IV
40	...	IV	...	IV	...	—	...	—	...	+	...	—	...	IV
41	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
46	...	IV	...	IV	...	—	...	—	...	+	...	—	...	IV
48	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
50	...	IV	...	IV	...	—	...	—	...	+	...	—	...	IV
51	...	IV	...	IV	...	—	...	—	...	+	...	+	...	IV
57	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
58	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
61	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
66	...	IV	...	IV	...	—	...	—	...	—	...	+	...	IV
67	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
73	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
79	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
80	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
81	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
84	...	IV	...	IV	...	—	...	—	...	—	...	+	...	IV
85	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
88	...	IV	...	IV	...	—	...	—	...	±	...	—	...	IV
95	...	IV	...	IV	...	—	...	—	...	+	...	—	...	IV
117	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
119	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
121	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
127	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV
131	...	IV	...	IV	...	—	...	—	...	—	...	—	...	IV

* Twins, binovular.

placed in Group IV, only nine, or about 15 per cent., showed both agglutinins *a* and *b* in their serum, while ten showed only one of the agglutinins—in four cases *a* and in six cases *b*—instead of the two which should theoretically be present in the serum of a Group IV individual.

Whether or not it is justifiable to allocate bloods to groups on the evidence of the corpuscles alone is open to doubt. Happ (1920) [14], who investigated the blood of a number of children at birth and later in life, states definitely that changes may occur not only as the result of the appearance of agglutinins absent from the serum at birth, but also from the appearance of either or both agglutinable substances absent from the corpuscles earlier in life. This matter requires further investigation, but does not affect the thesis of von Dungern

TABLE II.

Type of mating	LEARMONTH					OTTENBURG					DYKE AND BUDGE				
	Unions observed	Offspring				Unions observed	Offspring				Unions observed	Offspring			
		Groups					Groups					Groups			
		I	II	III	IV		I	II	III	IV		I	II	III	IV
	AB	A	B	O	AB	A	B	O	AB	A	B	O			
I + I AB + AB		Not observed					Not observed					Not observed			
I + II AB + A	3	4	2	5	2	4	1	1	
I + III AB + B		Not observed				1	1	Not observed				
I + IV AB + O	3	12	2	...	5	5	...	1	
II + II A + A	7	...	11	...	1	16	...	13	...	4	16	...	12	...	
II + III A + B	1	2	...	1	...	3	1	...	2	2	5	2	1	1	
II + IV A + O	14	...	32	...	8	17	...	18	...	16	32	...	16	...	
III + III B + B		Not observed				3	...	6	1	...	Not observed				
III + IV B + O	3	4	4	13	13	8	12	...	7	6	
IV + IV O + O	9	...	1*	...	18	15	23	31	31	
Totals	40	100				75	124				98	99			
(Grand Total						Unions, 213; Offspring, 328.									

and Hirschfeld that the agglutinable substances *A* and *B* cannot appear in the offspring unless present in the parents. This question and its medico-legal bearing has been discussed elsewhere by one of us (Dyke [15]).

The present data are summarized and compared with those of Learmonth and Ottenburg below.

The above table shows the results of observations on 213 married couples and 323 of their offspring. In every instance except one there is no appearance of the *A* or *B* properties in the offspring without their having been present in the parents. The one exception occurs in Learmonth's series and is marked with an asterisk in the table above. In such a large series one illegitimate offspring can hardly be regarded as constituting a large proportion of the total. The experimental method is obviously inapplicable to an investigation such as the present, and such conclusions as can be drawn must be arrived at by deduction. This being so, the figures given above seem to offer reasonable ground for the acceptance of von Dungern and Hirschfeld's hypothesis.

MODE OF INHERITANCE OF GROUPS.

In considering to what group the offspring of any two parents must belong, it must be remembered that it is not the group which is inherited. The dominants *A* and *B* and the recessives *a* and *b* are the inheritable factors and it is on the presence or absence of these that the blood group depends. For all groups, except Group IV, two or more genetic constitutions are theoretically possible and in determining to what group the offspring of any given parents may belong all the constitutions must be taken into account.

These possible constitutions are as follows:—

For Group I	...	<i>AB AB, AB ab, AB aB, AB Ab</i>
" II	...	<i>Ab Ab</i> (homozygous), <i>Ab ab</i> (heterozygous)
" III	...	<i>Ba Ba</i> (homozygous), <i>Ba ab</i> (heterozygous)
" IV	...	<i>ab ab</i> (double recessive)

The constitution of the offspring will depend upon that of the parents. So far as groups alone are concerned, ten types of mating are possible, but it is evident from the table of possible constitutions given above, that within all those types, with the exception of IV + IV, many possibilities exist. These possibilities are summarized below:—

(1) I + I. The offspring may belong, according to the constitution of the parent and the manner in which segregation of the factors has occurred, to any one of the four groups, either I (*AB ab, AB AB, AB aB* or *AB Ab*), II (*Ab Ab* or *Ab ab*), III (*Ba Ba* or *Ba ab*), or IV (*ab ab*).

(2) I + II. Here again the offspring may belong to any of the four groups. Leaving aside the possibilities under Group I, the only difference from (1) is that although a heterozygous Group III individual may result, it is impossible for the offspring to be homozygous Group III as in (1). As pointed out above, however, practically, homozygous individuals are indistinguishable from heterozygous.

(3) I + III. As in (1) offspring of all groups may occur, the only difference, leaving aside the possibilities under Group I, being that offspring belonging to Group II can only be heterozygous.

(4) I + IV. Again, as in (1) all possible groups may occur in the offspring, the only differences, leaving aside those under Group I, being that offspring belonging to Group II or III must be heterozygous.

The full possibilities of the constitution of Group I offspring have not been gone into, in considering the above four types of mating, since they are so

numerous and since moreover there is some evidence that only one type of constitution actually occurs.

(5) II + II. Offspring must be either Group II (*Ab Ab* or *Ab ab*) or IV (*ab ab*).

(6) II + III. Offspring may be of any group and of any constitution except homozygous Group II or III.

(7) II + IV. Offspring must be either heterozygous Group II (*Ab ab*), or Group IV (*ab ab*).

(8) III + III. Offspring must be either of Group III—homozygous (*Ba Ba*) or heterozygous (*Ba ab*), or of Group IV (*ab ab*).

(9) III + IV. This mating can only result in offspring of heterozygous Group III (*Ba ab*) or of Group IV (*ab ab*).

(10) IV + IV. Both parents in this type of mating are double recessives; no dominant is present in them and such cannot appear in the offspring. Children of two Group IV parents can then only be of Group IV (*ab ab*).

The above considerations are summarized below :—

TABLE III.

When blood groups of parents are	Offspring can be of groups
I and I	I, II, III, or IV
I and II	I, II, III, or IV
I and III	I, II, III, or IV
I and IV	I, II, III, or IV
II and II	II, or IV only
II and III	I, II, III, or IV
II and IV	II, or IV only
III and III	III, or IV only
IV and IV	IV only

CONCLUSIONS.

(1) The agglutinable properties *A* and *B* are demonstrable in the blood of the newborn.

(2) These properties *A* and *B* in the genetic behaviour are dominants.

(3) The agglutinins *a* and *b* are in their genetic behaviour recessives.

(4) The properties *A* and *B* cannot appear in the blood of the offspring without having been present in the blood of the parents.

The thanks of the authors are due to Dr. J. S. Fairbairn, of St. Thomas's Hospital, for giving access to the maternity wards of the hospital; to the Sister and Nursing Staff of Mary Ward, St. Thomas's Hospital, for their kindness in collecting with all the necessary care over 150 samples of placental blood; to Miss M. Robertson, of the Lister Institute, for valuable advice and suggestions; and to the Medical Research Council, without whose assistance the work could not have been carried out.

A NOTE ON THE POSSIBLE EXISTENCE OF A LETHAL FACTOR.

By S. C. DYKE.

The means of a clear and rational comprehension of the significance of the blood groups were first supplied by the work of Hirschfeld and Hirschfeld (1919) [16]. These authors, working at Salonica during the war when individuals of many races were gathered there and available for

investigation, found that while in races from North and Western Europe the *A* property was the commoner, individuals bearing the *B* property became proportionally more frequent as races from the lands to the south and east of Central Europe were investigated. Thus while Group II formed 43·4 per cent. of the English population and 43 per cent. of the Germans, and Group III only 7·2 per cent. and 12 per cent. respectively, the matter was reversed among natives of India who showed 41·2 per cent. of Group III individuals and only 19 per cent. of Group II. Up to the present this work remains almost all that has been done along these lines. Approaching the matter from a somewhat different angle, however, Verzár and Wezeczky (1921) [17] have investigated the relative proportions of the blood groups in three different types of population all found in Hungary, but racially very different. The first type consisted of the inhabitants of some villages near Buda-Pesth, colonized two centuries ago from Germany; the second of the inhabitants of a Hungarian town, and the last, of the Gypsies who wander through Hungary but do not marry outside themselves. The first population gave a distribution of the four groups, similar to that found by von Dungern and Hirschfeld in Germany; the second showed a distribution of the groups differing from this and approximating to that found among the Turks, while the Gypsies showed a distribution of the groups differing from both of these but approaching that found among the inhabitants of India.

The explanation of these facts offered by L. Hirschfeld and H. Hirschfeld is that the properties *A* and *B* respectively are typical of two different sorts of blood, each characteristic of a special type of the human race. They suggest that these two types of the human race may have had separate origins, the one in North-western Europe and the other in Central Asia. Originally the race arising in North-western Europe would have consisted entirely of pure homozygous Group II individuals having the formula *Ab Ab*, while the Central Asian race would have consisted of Group III individuals, having the formula *Ba Ba*. By their commingling, these two pure races would give rise to all the four blood groups as we now know them.

POSSIBILITY OF A LETHAL FACTOR.

Here arises a point of some interest. It is obvious that the offspring of the first filial generation resulting from the union of two individuals of the formula *Ab Ab* and *Ba Ba*, must all have the constitution *Ab Ba*—that is to say, they will all belong to Group I. The gametes produced by these offspring will have the four possible formulæ *AB*, *Ab*, *Ba*, and *ab*. The results of the interbreeding of the offspring of the first filial generation is shown in the chess board below:—

<i>AB</i> <i>AB</i>	<i>AB</i> <i>Ab</i>	<i>AB</i> <i>Ba</i>	<i>AB</i> <i>ab</i>
<i>Ab</i> <i>AB</i>	<i>Ab</i> <i>Ab</i>	<i>Ab</i> <i>Ba</i>	<i>Ab</i> <i>ab</i>
<i>Ba</i> <i>AB</i>	<i>Ba</i> <i>Ab</i>	<i>Ba</i> <i>Ba</i>	<i>Ba</i> <i>ab</i>
<i>ab</i> <i>AB</i>	<i>ab</i> <i>Ab</i>	<i>ab</i> <i>Ba</i>	<i>ab</i> <i>ab</i>

This is an application of Mendel's 9, 3, 3, 1 Law and from it it would appear that in the union of two homozygous individuals of Group II and III, no offspring of Group IV can appear in the first generation, while in the second they can only appear in the proportion of 1 in 16 of the total offspring and 1 to 9 of those of Group I. These considerations would lead us to expect that in a mixed population individuals belonging to Group I would be as common as, if not commoner than, those of Groups II and III. Group IV, consisting of double recessives, mating between whom can only produce other individuals of the same group, is in a different category and will always tend to increase in numbers in a mixed population. Actually such data as are to hand show that in all races, no matter what are the relative proportions of Groups II and III, Group I is always the least numerous. It would seem that some factor exists inhibiting the ready production of Group I individuals while no such inhibition exists in the case of other groups.

Some suggestion as to the possible nature of this inhibiting factor is supplied by the known facts in regard to the lethal effect exerted in certain instances by the doubling of the dominant. The classical example of this is the case of the dominant yellow in tame mice. Cuénot [18] first pointed out that the union of gametes each bearing this particular dominant resulted in a non-viable zygote, and ample evidence in support of his thesis has been advanced by American workers. Morgan and his co-workers (1919) [19] have found a number of instances of a similar lethal effect of the doubling of a dominant in the case of the fly *Drosophila*.

It seems possible that something analogous may occur in regard to the dominants *A* and *B* in the human being. Simple doubling of the dominant *A* or *B* in the zygote can hardly constitute a lethal factor, as, if it did so, homozygous individuals of Group II or III, with the formula *Ab Ab* and *Ba Ba* respectively could not exist. Zygotes containing but one of the dominants as in heterozygous Group II (*Ab ab*) or III (*Ba ab*), or containing neither, as in Group IV (*ab ab*), are also perfectly capable of full development. That is to say of the known formulæ we have some containing neither dominant *A* nor *B* (Group IV), some containing only one (heterozygous Groups II and III) and some containing one dominant doubled (homozygous Groups II and III).

In all these cases it is to be observed, in the first instance, that a dominant never occurs without its reciprocal recessive and secondly that, for all these formulæ, it is possible for segregation of the factors to take place in such a way as to result in two gametes, neither bearing more than one dominant.

Of the four possible formulæ for Group I the two above propositions hold for one only. The four theoretically possible formulæ are *AB AB*, *AB Ab*, *AB aB* and *AB ab*. In the first formula no recessives are present at all, in the second and third two dominants are present unaccompanied by their reciprocal recessives. In the first formula segregation of the factors must result in gametes each containing two dominants; in the second and third (*AB aB* and *AB Ab*), one gamete must contain two dominants. The fourth formula (*AB ab*) fulfils both propositions and resembles the known existent formulæ for the other groups; each dominant is accompanied by its reciprocal recessives and segregation of the factors can occur in such a way as to produce gametes each bearing only one dominant (*Ab* and *Ba*).

Two possibilities exist: (1) That in Group I only individuals of this last formula, *AB ab*, exist; and (2) that the three other formulæ may exist in individuals, but that the gametes produced by them bearing two dominants are not capable of fruitful union.

On this second assumption an individual of the formula $AB AB$, would be completely sterile, while individuals of the formulæ $AB aB$ and $AB Ab$ would produce only half their proper number of fruitful gametes.

Either assumption would account for a relative diminution of Group I as compared to Group IV individuals. Which is correct can only be ascertained by a series of observations of unions involving Group I individuals and such a series, owing to the rarity of Group I, must necessarily be difficult of collection.

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PROCEEDINGS
OF THE
ROYAL SOCIETY OF MEDICINE

EDITED BY
SIR JOHN Y. W. MACALISTER
UNDER THE DIRECTION OF
THE EDITORIAL COMMITTEE

VOLUME THE SIXTEENTH

SESSION 1922-23

SECTION OF PSYCHIATRY



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1923

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CONTENTS.

November 14, 1922.

	PAGE
F. L. GOLLA, M.B.	
The Organic Basis of the Hysterical Syndrome	1

December 12, 1922.

MARY C. BELL, M.B.	
The Use and Abuse of the Relationship between Doctor and Patient in the Practice of Psychotherapy	12

January 9, 1923.

J. LEITCH WILSON, M.B., D.P.M.	
The Endocrine Factor in Mental Disease	21

March 13, 1923.

HUBERT J. NORMAN, M.B.	
Genius and Insanity	33

February 13, 1923.

S. A. KINNIER WILSON, M.D.	
Pathological Laughing and Crying	39

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OXFORD HOUSE,
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Section of Psychiatry.

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The Organic Basis of the Hysterical Syndrome.

By F. L. GOLLA, M.B.

A SEARCH for the organic disturbance that underlies the hysterical syndrome may appear to be a very unpromising quest. It is generally accepted that hysteria is a purely mental disorder, that its origin is psychogenic and that it can only be fruitfully studied by inquiry into the subjective states of the patient. Now, as physicians, we are concerned only with purely scientific methods of investigation, and to the man of science such a term as "disorders of mind" can have no scientific meaning. All that is given us, in the study of the symptomatology of hysteria, is a disorder of conduct; of mind and the possibility of disorders of mind we have no objective knowledge.

The study of conduct is a purely objective study of all those activities which collectively represent all that we can know by observation of the exterior aspect of life. It requires a trained and experienced observer for ever on his guard against any tendency to introjection, that is, to interpretation of what is objectively given, in terms of his own subjective states. Laboratory methods are out of place in such an investigation. When, however, we attempt to discover the disorders of the bodily mechanisms by which conduct is expressed, we have to deal with a physiological problem which should be investigated by the methods of experimental physiology. That a disorder of the mechanism of conduct must precede the evolution of the hysterical syndrome appears to be a certainty. By some strange confusion of thought those observers who profess to find by analytical methods an initial psychic trauma at the origin of all neuroses tend to deny this pre-existence of a disordered nervous mechanism, though really it is a necessary inference from their own observations.

One fact emerges from all the investigations that have been conducted in recent years by the methods of analytical psychology, namely, that in the vast majority of cases the experience of the neurotic differs in no way from those that fall to the lot of the ordinary healthy man. The data furnished by the war are often cited as an argument to the contrary, but these really furnish irrefutable evidence that an organic disturbance or failure of organic equilibrium preceded the manifestations of the neurotic symptoms and could not have been caused, however much it may have been aggravated, by the individual experiences. In the armies of the allies and those of the enemy millions of men were exposed to similar conditions, yet only a small number were brought to

2 Golla: *The Organic Basis of the Hysterical Syndrome*

the hospitals suffering from a neurosis. Doubtless all who were in the line showed for a longer or shorter period the disturbances due to fatigue or functional hypertrophy of certain nervous mechanisms, but only those succumbed who were organically unsound. Sir Frederick Mott was able to obtain a history of pre-existing neurotic symptoms in 80 per cent. of the cases of soldiers under his care for neurotic disorders. Were the patients in these cases of organic nervous weakness the victims of experiences in infancy and childhood which had left an indelible mark on their nervous constitution? There is no reason to suppose that as a whole their experiences differed from those of the average child in their particular social milieu. We are justified in assuming an organic disability as an antecedent to every neurosis, and in employing methods for the objective evaluation of organic efficiency in looking for it. Even granting such an underlying bodily disorder, it might appear by no means likely that it could be demonstrated by the methods at our disposal, were it not that the application of pathological and physiological methods in the study of a similar problem in psychiatry has within the past few years yielded such brilliant results. A short time ago the pathology of dementia præcox stood in much the same position as that occupied by the neuroses to-day. It is due to the epoch-making researches of Sir Frederick Mott and his pupils that dementia præcox is now recognized as a degenerative condition profoundly affecting practically every bodily function. The study of morbid anatomy has revealed the existence of wide-spread degenerative cell changes, and the chemical researches of Pighini in Italy, and of Sidney Mann in Sir Frederick Mott's laboratory, have shown that profound metabolic disturbances are the functional expressions of many of the anatomical changes. In the light of these findings it should be impossible for anyone to maintain the possibility of a psychogenetic origin of dementia præcox. Since dementia præcox is merely a unit in the constellation of hereditary psychoses, any of which may equally well befall the other members of a psychopathic family, there is little room to doubt that a similar organic basis will be discovered for all the psychoses.

Now hysteria, for obvious reasons, does not permit of investigation by the methods of morbid histology, and consequently we are dependent on physiological methods. The clue to what particular system of the bodily mechanism is most likely to be affected must be sought in a survey of the clinical aspect of hysteria. The mass of contradictory views on hysteria lends colour to Lasague's dictum: "The definition of hysteria has never been given, and probably never will be." Much of the chaotic condition of thought on this subject arises from an attempt to study the psychology of the neuroses from a subjective rather than from a purely objective point of view. The objective method, it is true, cannot furnish more than an objective account of the problem, teleological significance cannot be expressed, nor can desire or conation be properly discussed: but at least it has the merit of making it possible to represent hysteria as a clinical entity in terms of behaviour. Bernheim was the first to adopt an objective standard in discussing the hysterical syndrome, and he pointed out that all the symptoms of hysteria were such as might be produced by hypnotic suggestion. Babinski has amplified this view by demonstrating not only that hysterical symptoms are such as might be caused by suggestion but that they can actually be removed by counter-suggestion. To quote Babinski's own words: "Hysteria is a pathological state, manifested by disorders which it is possible to reproduce exactly by suggestion in certain subjects, and can be made to disappear by the

influence of persuasion (counter-suggestion) alone." This definition of Babinski's has been received with almost universal assent. Its acceptance allows us to determine the clinical entity to which this label can be affixed without committing us to any theory of the psychology of hysteria. It implies however that hysteria can only manifest itself through mechanisms that are normally under voluntary control. Limited by the subjectivism that defines hysteria as only differing from fraud in its moral aspect, the school of Babinski declines to consider as hysterical any symptom that cannot be produced by an effort of will. This is a return to the subjective method, *par excellence*.

Of will and its limitations we have no objective knowledge, and, as Spinoza said: "No one has yet learned from experience what the body, regarded merely as a body, is able to do in accordance with its natural laws, or what it cannot do. For no one knows enough about the constitution of the body to examine all its functions." This attempt by Babinski and his followers to eliminate from the syndrome of hysteria all symptoms that are not directly under control of the will, has led to many errors, the most glaring of which is the relegation of late hysterical contracture to the position of a reflex contracture set up by irritation of the wound, on the ground that it is not directly curable by counter-suggestion, and that the contracted muscle shows certain abnormalities of response to electrical stimulation, and is not being completely relaxed under anæsthesia. It is now generally admitted that the persistence of the contracture when volition has been abolished by anæsthesia is due to degenerative change in the muscle, whose normal metabolism and lymph-supply has been interfered with by the persistent hysterical contraction. Such errors are bound to creep in when an attempt is made to delimit the power of the brain to effect bodily changes—thus, when an unpleasant thought is voluntarily recalled, it may be accompanied by some such emotional concomitant as blushing, and a voluntary act is here manifested by an involuntary mechanism. Such difficulties need not affect the truth of Babinski's definition, though they impose a certain elasticity on its application. An evaluation of the psychology of the hysterical individual presents considerable difficulties. The problem is to discover whether the clinical entity is an expression of a pathological nervous state, and it is only too easy to confuse the symptoms that the patient exhibits because he has hysteria with those that might indicate a nervous diathesis. Most psychological investigations of hysterics fail because of this difficulty. It is not from the specialist who sees the patient in the full possession of his malady, but from the general practitioner or an intelligent schoolmaster who has known him for most of his life, that we may expect the most valuable evidence. The picture obtained from these sources is that of an eccentric individual without strong or durable emotions, though anxious to impress the outer world with the gravity and intensity of his experiences. Always more ready than a well-bred person should be to impress his fellows, he would not willingly apply himself to any task involving strenuous exertion, fatigue, or danger. The total personality conveys an impression of flimsiness. He appears to be a very different person from the volcano of ill-suppressed sexual passions that some writers on psychotherapy have portrayed.

There is one other cardinal symptom of the hysteric that has been emphasized by all observers, and that is his abnormal suggestibility. So far, then, all the data obtained from a purely objective survey of hysterical conduct point to an egocentric weakling, deficient in that stability of the

4 Golla: *The Organic Basis of the Hysterical Syndrome*

nervous system which resists outside suggestion. The data are meagre, but such as they are they apparently point to some disturbance of emotional activity.

In the nervous system there are two systems of afferent function. One system reacts to stimuli of a nocuous nature; the effector organs which manifest its activity are either organs not under the power of conscious effort or else certain combinations of reflexes which cannot be voluntarily activated. The great terminal ganglions for this system are thought to lie in the thalamus. This system is phylogenetically older than the cortical system. It originates the defensive reflexes by which the body reacts as a whole to nocuous stimuli, whether by glandular, visceral, vascular, or muscular pattern reflexes. The other system of nervous mechanism is that by which appropriately adjusted reflex patterns are combined in response to stimuli not in themselves possessing either nocuous or benign characteristics. The terminal co-ordinating centre of this system is the cortex. Its function is discriminatory and this discriminatory adjustment of certain reflex mechanisms to various stimuli is correlated with consciousness.

The two systems are not, however, independent. When the activity of the cortical processes is impeded by opposition arising from either external or internal stimuli these obstructing stimuli constitute what is in effect a nocuous stimulus, inasmuch as they threaten the persistence of the normal activities of the organism. In reply to such a nocuous stimulus the resources of the body as a whole are mobilized to overcome the resistance, or in other words an activation of the thalamic system occurs. Conversely, changes of the activity of the thalamic effector organs, when they reach a certain degree of intensity, stimulate the cortical receptors and we become aware of a change in the common sensation of the body. The consciousness of such a disturbance constitutes what we term an affective state, or, as some prefer to call it, an emotional state.

It would be impossible within the limits of this paper to deal with even a condensed exposition of the methods by which the organic resonance of affection is determined. I can only indicate the various mechanisms in which responses can be instrumentally recorded, and at the same time I must remind you that the most obvious and at the same time the most instructive of all the signs of emotion—namely, the facial expression—eludes by its very nature all attempts at instrumental control, whilst attempts to record inflection of the voice, which constitutes in itself the language of emotion, are as yet unsuccessful.

In the first place I would draw your attention to the muscular response to a nocuous stimulus as manifested by an increase of tonus and, as I originally pointed out in the Croonian lectures, this increase of tonus is not in the nature of a voluntary tetanic contraction, as it shows none of the characteristics of the electromyogram of a voluntary contraction. The increase of tonus affects those muscles that are relaxed rather than those already in some state of postural contraction and its influence may be well demonstrated in the knee-jerk. If the knee-jerk be recorded with some of the precautions to which I have drawn attention elsewhere, it is possible by its fluctuations to follow, only roughly of course, some of the fluctuations of affective state in the patient. We next have the respiratory response to a nocuous stimulus. As you will observe there is no obvious point of distinction between the two records of grief and anger. Here again great care must be taken to avoid the records being complicated by unnatural breathing, which invariably occurs when the

attention of the patient has been directed to it. I find that the best results are obtained from unintelligent patients who can be persuaded that the apparatus placed on the thorax is for the purpose of recording the heart beat. The correlation shown in the slide, between the muscular tonus of the quadriceps and the flattening of the respiratory curve indicates the probability that an increased tonus of the respiratory muscles may play some part in the altered respiratory movement. Next we observe the alteration of the pulse rhythm in response to a nocuous stimulus, an alteration which, as I have pointed out elsewhere, is more probably due to stimulation of the accelerator than to inhibition of the vagus. The next slide shows the plethysmographic response of the arm to a nocuous stimulus causing a reflex vasoconstriction. The affective rise of blood pressure is also shown. The extraordinary sensitiveness of the pressure of the cerebro-spinal fluid and the relation of such changes of pressure to respiratory oscillations of emotional type are shown in the accompanying slide. Now all these reactions, and one or two more which concern the internal pressure of the hollow viscera, have this in common: though they are not under control of the will they can yet be materially altered by the voluntary innervation of neighbouring structures. Thus the respiration can be controlled by forced breathing, the muscular tonus altered by voluntary contraction, the blood-pressure by fixation of the thoracic muscles, &c. There is, however, one type of affective reaction that cannot be affected by voluntary effort: this is the diminution of skin resistance that takes place in response to a nocuous stimulus, commonly termed the psychogalvanic reaction. Physiologically there seems to be little doubt that this increase of conductivity or reciprocal diminution of skin resistance is the expression of an increase in tissue activity occurring in the sweat-glands of the skin in response to a nocuous stimulus. I have elsewhere dealt with the inevitable character of this easily measured reaction. It is not under the direct control of the will, it can only be elicited by stimuli that have a truly nocuous character, whether such stimuli be physical or psychical. Thus a prick, the threat of a prick, the pangs of hunger, or the prospect of losing a fortune on the Stock Exchange, will be in their several measures efficient stimuli.

The threat to cause bodily discomfort is the simplest form of verbal stimulus. From this point onwards the stimuli may be elaborated by the representation of circumstances, either past or present, that are opposed to the well-being of the subject or are in conflict with the general tendency of his conduct—that is stimuli such as will on the affected side evoke the feelings of grief or anger. So much for the galvanic response to external stimuli, but even more important is the galvanic response to stimuli whose immediate origin is in the autogenous neural processes of the subject. The spontaneous representation of some disagreeable or painful circumstance will serve equally well to elicit the galvanic reflex. Whether the subject silently recalls some past experience of a painful nature, or whether he communicates it verbally to the observer, the representation will in both cases evoke a diminution of skin resistance. The simulation of emotional states invariably fails to give the galvanic reflex; it is possible to recite emotional poetry and to abuse an imaginary opponent with the greatest energy without causing any movement of the galvanometer spot. It is indeed instructive to sit quietly by oneself and observe what thoughts and reminiscences have an affective tone and in how many instances subjects on which we think we ought to feel strongly, fail to display an organic emotional reaction.

I have found that the galvanic response exhibits the same latent period as

6 Golla: *The Organic Basis of the Hysterical Syndrome*

the other form of response of the affective mechanism—the convenience with which it can be calibrated and studied, and the impossibility of voluntarily controlling it, make it the most suitable response of the affective mechanism for experimental investigation though it is by no means the most delicate. A study of the time relations of the response illustrates the dissociation of the affective response from the discriminatory or cortical response to a stimulus. I show on the screen the photographic record of an experiment which makes this clear and incidentally throws a new light on a question over which psychologists still quarrel owing to their devotion to introspective methods—namely, the validity of the James-Lange theory. This is the record of the reaction of a subject to a nocuous stimulus manifested by the galvanic reflex—a voluntary motor reaction and a start. The subject was instructed to press a key actuating a signal when he heard a pistol shot. The record shows that the first event—an involuntary motor reaction or start—took place about '008 second after the shot. This is, as Sherrington showed, a mesencephalic reflex and need not, for the moment, detain us; though, as we shall see later, it is not without bearing on certain hysterical manifestations. The next event was the voluntary depression of the key by the subject as soon as he perceived the shot, the latent period of the voluntary discriminatory response was a normal one of 0'2 second. The third event is the movement of the galvanometer fibre at the beginning of the galvanic reflex. The latent period of this reflex is 2 seconds. If the galvanic reflex be taken to indicate one of the many bodily responses that make up the general state of bodily activity the sensory equivalent of which is an affective state, then this affective or emotional state is arrived at quite 1'8 seconds after the initial stimulus has been perceived and acted upon. Now this observation is typical of all records of affective or emotional response—the discriminatory or intellectual mechanism acts in the first instance alone; later the general bodily reaction to the nocuous stimulus occurs and the affective state is perceived. Now in the hysterical subject we have to a greater or less extent an enfeeblement, or even a virtual suppression of the affective response.

In 1918 I had the opportunity of examining all the cases of neurosis that passed through the Maudsley Clearing Hospital, which, at that time, had over six hundred beds. I found that the large class of patients exhibiting those motor and physical symptoms which constitute the hysterical syndrome could be readily differentiated from those exhibiting other types of neurosis, by the extraordinary depression of the galvanic response to any form of stimulus. Such patients would start or tremble at a loud noise or painful stimulus when a normal man would hardly budge, but their galvanic response would be either absent or less than normal. Though they might talk of great emotional perturbation there was no corresponding galvanic reflex. The *mise en scène* of the galvanometer room seemed to be extremely conducive to the exhibition of hysterical crises, and I had the opportunity of observing the galvanic reflex during many such scenes. One young soldier suffering from a hysterical contracture broke down during an examination; tears rolled down his cheeks, he addressed his dead brother in language savouring of a South London melodrama. He asked why he himself had not been killed in his brother's place, so that the favourite son might have been left to comfort the poor old father, and all the time, while he wailed and wept, the spot of light from the galvanometer mirror remained steady.

Now the counterfeiting of rage or grief, no matter how dramatically performed, is unaccompanied by the electric signs of activity of the affective

mechanism. In these observations we have then, I think, the key to hysterical behaviour. It is purely imitative. This was my view in 1918 and I have since found nothing to cause me to modify it. Not only is the response to the alleged emotional states absent in the hysterical crisis, but the hysteric reacts as a rule subnormally to all forms of adequate—that is nocuous—stimuli. A study of the time relations of the affective and the discriminatory response has shown that the nocuous stimulus is recognized and elicits an appropriate cerebral response an appreciable time before the bodily mechanism of affection reacts. We have in fact a kind of dualism: on the one hand the discriminatory mechanism associated with intelligent behaviour, and on the other, the reaction of the organism as a whole to a nocuous stimulus. Herein lies the explanation of hysterical conduct. The hysteric is as capable as the normal man of recognizing that the situation in which he may find himself is one of potential peril or discomfort, he can respond immediately to the situation by an appropriate cortical reaction, but if the stimulus be continued, his activity lacks the reinforcement supplied by bodily reaction. To determine his line of conduct a representation of the situation is necessary and such a representation must normally involve the activity of the mechanism of affection since in no other way can the unpleasantness of the situation—the affective state that reinforces the intellectual appreciation of its danger—be recognized. Deprived of this determining force his reaction to the situation will be at the mercy of any casual stimulus. Since the affective reaction fails him, he has recourse to other forms of expression more or less distantly connected with the feeling of unpleasantness. The association of bodily ailments with the feeling of distress will most readily furnish the subject matter of the drama by which the patient endeavours to represent to himself and to symbolize to the external world the fact that his activities are threatened or impeded by some nocuous influence in the environment. Hence the hysterical symptoms really constitute a method of self-expression, primarily for egoistic and secondarily for social needs, which has been conditioned by an organic disability of the mechanism of affection.

There is another cardinal hysterical symptom that is referable to the same organic disability—namely, the abnormal suggestibility of the hysteric. Suggestibility has been defined by McDougall as a process of communication resulting in the acceptance with conviction of the communicated proposition in the absence of logically adequate ground for its acceptance. Our strongest bond with logical reality is that furnished by the feelings or emotions, for these constitute our strongest defence against the irrational. Mind, disassociated from feeling, is mind very much at the mercy of any suggestion. The bodily reaction against a suggestion that is in discord with the general tendencies of our activity is the real guarantee against its acceptance. It is just this defence that the hysteric lacks; to a greater or less extent his activities can be unduly influenced both by autogenous and heterogeneous suggestions. In other words, the innate and habitual pattern reflexes, having lost the mechanism of bodily reaction by which they reply to stimuli tending to interfere with their activities, are no longer a potent protective system as they are for the normal man and can be displaced or disassociated by any new stimulus of sufficient potency.

Another, but less important, phase of the hysterical syndrome that has impressed most observers is the excessive motor reflex response of the hysteric. One of the most characteristic bodily responses to mental or physical effort is a marked increase of muscular tonus—every muscle is tightened and the

8 Golla: *The Organic Basis of the Hysterical Syndrome*

respiratory movements are reduced to a minimum. Owing to his disassociation from states of feeling the hysteric appears to assume readily that state of muscular hypertonicity which accompanies effort, and, as I have shown elsewhere, there is an intimate connexion between this hypertonic state and the increase of the deep reflexes. In hysteria the knee-jerk may not only be increased but may be followed by a series of irregular clonic contractions which, however, as shown in the accompanying slide, may be readily differentiated from the true quadriceps clonus of a lesion of the pyramidal tract when recorded by the oscillograph or the string galvanometer. The response to an auditory stimulus frequently takes the form of an exaggeration of the normal "start" which is, as we have seen, a mesencephalic reflex.

Is the diminution of the affective response in the hysteric of central or peripheral origin? We have not at present the data to decide this question but the two slides that I show on the screen are at least suggestive. The resistance of the skin was shown by Waller to undergo a diurnal variation following closely the diurnal variation of temperature. I took advantage of this observation to determine the relation of the galvanic responses evoked by uniform stimuli to the initial skin resistance. Maximal stimuli were used, and for this purpose a pistol shot was found most convenient. The chart represents the mean of observation conducted on three successive days, but the difficulties in obtaining the subject at any one time under approximately the same condition are very great. The observations clearly indicate that the maximum affective response occurs round about mid-day when the resistance of the skin is at its lowest, and there is a rough correspondence between the resistance of the skin and the reflex throughout the day. There is good reason to suppose that the galvanic reflex at any rate—and most probably all forms of affective or emotional response—are effected by the vegetative nervous system. Now one portion of the vegetative system is constituted by the sympathetic, and the excitability of the sympathetic system may be readily increased by the administration of thyroid extract. By giving large doses of thyroid in the early morning I was able both to diminish the mean value of the skin resistance and to mask its diurnal variation. At the same time the galvanic response to uniform sound stimuli was considerably increased as shown by the second slide. Now the diminished affective response of the hysteric is accompanied by an increased skin resistance, and in four cases I have succeeded in producing a normal type of response together with a decrease of skin resistance, by the exhibition of thyroid. In view of the suggestibility of the hysteric I should hesitate to attach any value to his statements of an improvement in their general condition. Thyroxin sensitizes the nerve endings of the sympathetic and the organic insufficiency of the affective system must be central. It is not, however, impossible that a drug acting on the peripheral mechanism might tend to mask a deficient activity on the part of the centre. Goetsch has devised an intradermic test that serves to demonstrate the existence of hyperexcitability of the sympathetic system. The reaction depends on the demonstration of sympathetic hyperexcitability by injection of minute quantities of adrenalin into the superficial layers of the epidermis. Using this test, as modified by Ascoli and Faggioli, I have generally found that the excitability of the subepidermal test lies between dilutions of 1 in 200,000 and 1 in 1,000,000 of Parke Davis adrenalin solutions in normal cases, that in hysterical cases the excitability lies between dilutions of 1 in 200,000 and 1 in 500,000, whilst in some cases of disturbance of the menopause, of arterial hypertension, of Graves' disease, and sometimes in

pregnancy, the excitability is increased, giving further positive results with dilutions up to 1 in 20,000,000. I attach no importance to this test except that it affords evidence that the excitability of the sympathetic system is never increased in hysteria and, if anything, tends to lie on the hypo-excitabile side.

In regard to this point other experiments are in progress, and I hope by means of an exquisitely delicate test for blood-sugar devised by Dr. Calvert that Captain Mann and myself may gain some additional information as to the lability of the vegetative nervous system to central and peripheral stimuli in the normal hysterical subject. Experiments are also in progress at the Maudsley Hospital which may tend to show whether the apparent sluggishness of the mechanism of emotional response is associated in hysteria, as it is in dementia præcox, with a disturbance of oxidation processes. The age at which hysteria is most common suggests that its manifestations are in the main coincident with the period when the stress of the external world bears most heavily on the nervous organization which has not yet established habitual reactions. The following table is that of Kraepelin :—

PERCENTAGE AGE-INCIDENCE OF COMMENCEMENT OF SYMPTOMS.

10th year	15th year	20th year	25th year	35th year	40th year	45th year	50th year
0·9	2·1	36·8	12·1	6·3	4·4	1·9	2·1
per cent.	per cent.	per cent.	per cent.	per cent.	per cent.	per cent.	per cent.

Although Kraepelin's statistics distinctly show that the incidence of hysteria in his 430 cases is most marked during the period of adolescent stress, I find it hard to believe that such a sharp descent in the percentage of incidence between the twentieth year and the twenty-fifth year is only explicable by the formation at that age of suitable responses to the exigencies of adult life. It suggests the definite amelioration of the organic response rather than that a disappearance of environmental stress is the responsible factor. One thing at any rate seems to be clear, and that is that the hysterical syndrome tends to fade away with increasing age, and it seems more than a coincidence that, contrary to what one would expect, the affective response tends to increase in magnitude with increasing years. The late Dr. Waller first called my attention to this interesting fact, which I can unreservedly confirm.

In presenting the foregoing objective view of the hysterical syndrome I have endeavoured to confine myself entirely to the problem of the organic basis of hysteria. In the elucidation of this problem reference to the investigations of workers who adopt the methods of psycho-analysis would be out of place. Whatever may be the value of this method, it can no more give us information about the organic basis of a neurosis than it could about the state of the meninges in general paralysis. There is no necessary antagonism between the methods of introspection and those of pure behaviourism. Should the views that I have here propounded in a tentative fashion prove to have a solid foundation in pathology, any investigation from the standpoint of analytic psychology must ultimately amplify our conceptions of the underlying disorders of the nervous mechanism.

DISCUSSION.

Dr. STANFORD READ said that Dr. Golla had pointed out, as many other opponents of a more or less purely psychogenetic viewpoint with regard to the psychoneuroses had done, that the normal individual tended to have the same early environmental

10 Golla: *The Organic Basis of the Hysterical Syndrome*

experiences as the neurotic, and that, therefore, psychic traumata in childhood could hardly have any ætiological significance. The psychogenic school, however, only pointed to a likely exaggeration or persistence of such traumata and dwelt rather on the fact that it was the abnormal attitude towards such factors that had the deleterious effect and so brought about later pathological results. The fact that organic changes could be set up by emotional causes had been amply established by war neurological experience, and Sir Frederick Mott's researches in dementia præcox by no means proved an organic basis for that condition, though, as the organism was an integrated whole, this factor could not be neglected. McDougall had lately attacked Mott on this question. Babinski's ideas were probably less widely accepted now in England than formerly, and the results of therapeutics on his lines had wrought much havoc among war pensioners, through neglect of the mental state underlying the symptoms. Neither Babinski nor Dr. Golla had explained in any way why the hysteric was so hyper-suggestible. Too much stress could not be laid on the point lately insisted on by Sherrington, that it was stepping from one world to another—and to an incommensurable one—to pass from a nerve impulse to psychical processes, and that it was quite impossible to deal with mental actions in terms of nervous action or vice versa. Dr. Golla's experiments were of interest, but in no way did they throw any light on the sources of the hysterical syndrome. Kempf's work was of importance as an attempt to correlate physiological changes with mental processes and states.

Dr. MILLAIS CULPIN said that he found some difficulty in placing the "hysterical syndrome" among the varied cases that were met in the psycho-neuroses, and asked the lecturer for a more precise clinical definition of the type of case in which the reactions he described were to be found. Speaking as a clinician he asked whether pure science was as helpless in regard to the hysterical syndrome as it was in regard to dementia præcox.

Sir FREDERICK MOTT said that he thought there was some misapprehension with regard to Dr. Golla's position in respect to psychological influences upon the bodily condition. Dr. Golla did not deny this, but he emphasized the importance of studying objective signs by every means at our disposal. He (Sir Frederick Mott) supposed that Dr. Golla assumed that in the subject of hysteria there was a genetic bodily inadequacy by which the patient, under the influence of psychological stress, was unable to make normal mental adjustments. Dr. Golla had endeavoured by the application of physical methods to show this. A study of a large number of family pedigrees had shown that psycho-neuroses and psychoses were found in various members of a stock often endowed with genius or high intellectuality. This fact he (the speaker) regarded as evidence of an inborn tendency to neural instability affecting neuron structures of the highest and latest evolutionary level. He wished to express his appreciation of the valuable researches carried on by Dr. Golla at the Maudsley Hospital during the last three years. These researches had the great merit of being an addition to knowledge upon a subject of which very little was known owing to the many technical difficulties encountered. He was sure that Dr. Golla would be the first to admit how little was known and how much there was to learn before the system of physical methods of measurement could be applied to the objective study of mental processes and utilized in clinical diagnosis, but he (the speaker) was convinced that the methods Dr. Golla had employed were a step forward in the right direction.

Dr. CRICHTON MILLER said that he had been much interested in Dr. Golla's paper, and recognized the amount of painstaking research represented by its findings. For his own part, however, he felt that so long as they restricted themselves to the observation of neurological reactions, they were only contributing to the diagnostic side of the work. Sir Frederick Mott's remarks about heredity had also considerable value, but that again was largely limited to prognosis. Treatment must ultimately depend upon the conception of individual ætiology; and for that they would constantly find themselves confined to two great categories: (a) the bio-chemical, covering the endocrine factor; and (b) the psychological, covering emotional conflict.

Dr. GOLLA (in reply) said that he feared he had failed to make it sufficiently clear that the investigations which he had put before the Section were solely directed to the demonstration of the organic disturbances underlying and preceding the establishment of a neurosis. It was obvious that the actual orientation of the particular neurotic symptom was determined by the preceding psychical events. Such investigations as he had here put before them admitted of no direct application by the clinician. In years to come it might of course happen that the groundwork now being slowly accumulated would bear some practical fruit.

Section of Psychiatry.

President—Dr. C. HUBERT BOND, C.B.E.

The Use and Abuse of the Relationship between Doctor and Patient in the Practice of Psychotherapy.

By MARY C. BELL, M.B.

ANY understanding I may have upon this subject is mainly the outcome of observations made in my own hypnotic and analytical practice, of my own reactions to my analysts, and of my knowledge of the writings of psychoanalysts, but that does not mean that the subject is only of value from the psycho-analytical point of view. The relationship between doctor and patient is of primary importance in any psychotherapeutic practice, whether that of hypnotism, persuasion, re-education, or analysis.

The personality of the doctor as an asset in the building up of a successful practice is recognized in the term "a good bedside manner," and the man who could inspire confidence without causing irritation, and would save his patients trouble by dictating a pleasant course of future action, was almost sure of a large clientèle. But this is a rough and ready method compared with the finesse needed in the more prolonged and infinitely more intimate relationship of psychotherapy. To give one instance only of the change: the older doctors were very willing to assume responsibility for their patients: to say, "come here," "go there," "take up such and such work," "go for a sea voyage." The tendency now is to encourage independence of thought and action on the part of the patient: to impart wisdom instead of demanding submission; and this necessitates a knowledge not only of the strong and weak points of the patient's psychology, but also of one's own.

I will first put the problem from the patient's point of view, and then from that of the physician; finally I will make suggestions with regard to the conscious handling of the relationship between the two.

The patients are like frail vessels embarked on a great adventure, an unknown quantity being the doctor. Like all of us, they have made mal-adaptations to life, and though they may recognize and acknowledge the gross results of the mal-adaptations they do not see where the kinks originally occurred, since those arose in early childhood. Given a certain environment, a child with a certain temperament will react in a given way. The original mal-adaptation was an innocent enough affair, but the gross results are weighed against the often distorted standards of conventional herd thought, and the patient becomes burdened by an overwhelming and crippling sense of shame; for conventional standards are regarded as absolute authority, and thereby invested with too great a value.

[December 12, 1922]

Many of the difficulties in life arise from the discrepancies between the unconscious standards and over-valuations that we make for ourselves or adopt from others, and the standard to which we are capable of attaining in reality.

Now in order to understand the attitude of the patient to the doctor, we must consider the problems of projection and identification. By projection I mean the unconscious placing of our own thoughts and phantasies outside ourselves, and the failure to recognize them as our own. We all recognize striking examples in the projections of paranoia, but we do not always realize the great part played by projection in everyday life. For example, a mother crossing a field containing cows is afraid, and thinks that her child is afraid; a father projects on to his son his own desire that the son should take up a certain career; or a person who is not very truthful is surprised at the deplorable lack of honesty in his friends. In one way or another a great number of our own unrecognized psychological tendencies are projected in the form of criticism.

By identification I mean the actual psychological union of oneself with one's ideas and dreams, with emotions, or with surrounding objects. The child not only acts the princess or hero, but *is* the princess or hero, and demands suitable treatment and reverence from nurse and companions; the nun not only adores the Christ, but *is* the Christ, and develops the stigmata of the crucifixion; the politician is identified with the creed of his party, and any attack upon his party is an attack upon himself. It is identification with ideas and feelings that is of such great importance to us. If we are not identified we can stand aside and see the idea or emotion in its right proportion and judge dispassionately. The child who is not identified with the princess or hero can distinguish the phantasy from the reality, and the phantasy does not permanently affect her outlook on life, but the woman who is still identified with her "princess" or "perfect lady" phantasy unconsciously expects from life the deference she imagines a princess would receive, and then she is dissatisfied with and refuses to accept reality.

Now these processes of projection and identification both take part in the relationship of the patient to the doctor: unrecognized attitudes and desires are projected on to the doctor and form the basis of a positive or negative relationship, or transference, as it is called. All of us are more or less under the sway of unrecognized infantile reactions: in certain situations, peculiar to each one of us, we meet the problems of adult life with the emotions or thoughts of infancy. In the type of patient coming to our consulting rooms this discrepancy between the problem and the understanding of it is peculiarly well marked: and although, in many cases these patients may have an intellectual appreciation of their defects, they do not feel them in their "bones," or see their manifestations in life. For instance, a friend of mine, in spite of having led an apparently full and busy life, had always asserted, to the amusement of her friends, that she was born lazy. That was a true, but purely intellectual appreciation of a certain aspect of her psychology, but it is only of late years that she has understood it sufficiently to observe its very subtle manifestations, and to attempt to eliminate her laziness. In the same way our patients have partial realizations of their defects and desires, and this knowledge of themselves clamours for conscious recognition; but the buffers associated with pleasure and pain, and inculcated ideas of morality, prevent them from acknowledging the desire or the defect. Now a vague feeling that someone is lazy, or childish, or cruel, cannot continue unattached; and as barriers hinder it from coming home to roost, it is projected into the environment, and in most cases, during psychotherapeutic treatment, it is

the physician who becomes the perch. Naturally this is more noticeable in treatment by analysis when the patient is being encouraged to recognize her own birds and bring them home to roost, than in repressive treatments such as hypnotism and the method of Coué, where the birds are merely shut up in an incubator out of sight; but even in these treatments projections may become a barrier.

A patient, aged about 40, comes into my room and says, in a childish way, "I have not brought you any dreams, doctor, I'm afraid you will be annoyed." "Why should I be annoyed?" I say: and then after some search I find that the patient is projecting on to me her fear of one or other parent, usually, in my case, the mother, or of some governess or mistress in the past. Or the reverse occurs: the patient who has suffered from too little love and understanding in childhood has built up an imago of the perfect mother, and wants to sit at my feet like a child; moreover she unconsciously judges all women by this perfect standard. Now both these types will fail to progress if their attitude to me is not discussed and understood, for how can an adult child who has been unable to give confidence to her own mother have confidence in me if she is unconsciously expecting me to sneer at or misunderstand her? or how will she grow up and become independent and willing to bear her burdens in life if I let her go on sitting at the feet of a mother imago? It may be the father who has kept the child bound, in some cases choosing a career for her, and then she will persistently invest the physician with the status of father-counsellor, or, it may be, lover.

Or perhaps the patient has to recognize some kink of untruthfulness, or cruelty, or untidiness, &c.; and then we may be told we are not so truthful as we might be, or that we take a cruel pleasure in dissecting our patients, or that we dress badly. Or the patient may be dealing with unrecognized sex desires, and these, too, may first announce their approach by some homosexual or heterosexual attitude to the physician, a projection that is often violently resisted by the patient.

Megalomaniac ideas, on the other hand, may lead to identity with the analyst; the patient thinks he can get on with his own treatment, or tries to analyse the doctor. Very often the relationship of patient to doctor is a mixed relationship, alternating from positive to negative and back again. For example, I had a patient with a definite positive transference, who periodically became distinctly negative, saying I was cruel, and forced her to do things she was quite unable to do—to turn up punctually at my consulting room when she was "feeling all anyhow." She suffered from very constant attacks of nausea, and a feeling of a wedge in the back of the head since she was 9 years old until her present age (37). Part of the negative transference disappeared when she realized that she was identifying me with her older step-sisters, who when she complained of feeling sick in childhood used cheerfully to say: "Oh! no, you don't—come along!" thus causing her much uncertainty about the veracity of her own feelings. Another, the patient of an absent colleague, had a very marked negative attitude underlying a childish adoration—she felt my colleague as an awful power, who expected all she said to be accepted as gospel truth—this was partly a projection of the patient's own power psychology, and partly a projection of the difficulty experienced by her irrational infantile psychology, in accepting the very rational psychology of her family. Common sense appeared to her like a Juggernaut. We must remember that these projections are phantasies, the imaginings of a distorted outlook, and in no way indicative of the true state of any situation. And so, as I have said, the patients'

birds come home to roost on the "doctor perch," though cases do occur in which the bulk of the transference goes over on to some relative or friend.

Now we must approach the problem from our own point of view. Every relationship is reciprocal; we think a great deal of the patient's contribution, but perhaps we do not enough consider our own share. We, too, are the victims of unconscious infantile reactions; some of us have honestly tried to eliminate, as far as possible, their effects, by undergoing a long analysis ourselves; some of us, perhaps, have not, and any reaction we have not yet understood in ourselves we may project on to our patients, and the beam in our own eye will prevent us from seeing the moles in our patients' eyes.

I have heard psychotherapists say that "there is nothing in the transference." They stand self-condemned, for nobody who has been analysed by a competent analyst can fail to understand the delicacy and working value of the relationship; the resistance of the so-called self-analyst to analysis by another person is proof of the activity of reactions that must put up a barrier between physician and patient, and these barriers will keep the work on a superficial plane. To mention one barrier only. The patient who realizes after a time that the physician has not himself been through this exacting and heart-searching ordeal—and I have more than once heard this criticism made—is apt to resent the self-knowledge gained through one who searches, but will not be searched; and I have known cases in which the physician's lack of understanding of his own psychology, a lack of understanding that was apparent to the patient, has converted a good relationship into a bad one, with subsequent rupture of the treatment.

It may be argued that the leaders of the two main schools of psychological analysis had no one to analyse them. That may be true; but in spite of that, they recognize the relationship, and the danger or value of it as the case may be; and one of them, at any rate, Dr. Jung, of Zürich, has utilized the relationship to make good the absence, in later years, of an analyst for himself. For, the relationship being reciprocal, it is possible for a sincere worker to analyse himself by his reactions to his patients and their criticisms; but it is a *sine qua non* that in order to do this, he must recognize that there is such a thing as a relationship, and of what the relationship consists.

The patient looks at the doctor through spectacles the lenses of which are the varying projections of unrecognized psychological identifications which may produce a highly distorted image, and the doctor does the same by the patient. If in a day's work with six or seven patients I find it necessary to discuss the same problem with more than two of the patients, I begin to take stock of my own psychological problems, and to inquire of myself whether I am seeing clearly, or whether I am distorting my patient's problems by my own. It is possible that two patients in a day may be dealing with the same problem, but three patients I regard as a warning to myself. When every patient seems to us auto-erotic or obsessed with sex, we must take heed to our own psychology. When every patient brings me a different problem, I take it for granted that my own lenses are fairly clear. We must not project on to our patients intuitions concerning our own psychological state. Recognized as belonging to ourselves, these intuitions are most valuable—projected on to a patient to whose psychology they are alien, they are cruel and devastating, and may change a positive into a negative relationship. The patient who knows nothing of projections will try to fit them on to his own psychology, or realizing their falsity but unable to formulate his realization,

will develop a negative attitude, and be unable to proceed with or benefit by the treatment. In the purest form of psycho-analysis where the analyst acts as a mirror and no form of discussion of the patient's problems takes place, presumably the risk of projection by the physician is small, but in any form of treatment by discussion or persuasion, and in the actual diagnosis of the situation, the unrecognized projections of the physician may play no inconsiderable part. Indeed, projection is a prime factor in all human relationships. If all of us could see one another clearly without any blurring from our own projections, how much more just should we be, and more tolerant of one another. I think I have found in my own experience that people with a highly developed faculty of intuition are particularly prone to experience intuitions concerning their own psychology, and to project them on to others.

The psychology of the physician will also affect the handling of the patient, and the method of treatment employed. A person with an unrecognized power or authority complex will tend to keep the patients in an inferior or infantile condition, and will manage them and dictate to them too much, and so unconsciously prevent them growing up. In those cases patients will return again and again for a spell of help, because they are not made to realize that they are still dependent on authority and the protector. I think, too, the doctors with an unrecognized power complex will tend to make use of a rather dominating form of hypnotism in their practice, whether as pure hypnotists or as analysts employing the hypnotic cathartic method of analysis. There must be a danger that their patients will only slowly free themselves from the relationship and grow up. I use the term "growing-up" as expressive of an adult adaptation to life; we should be just our age; neither too young nor too old; any precocity or retardation is a mal-adaptation to the law of normal development; but every person has his own rate of growth. Now, having recognized the patient's contribution to this relationship and our own, how must we handle it? Are we to take the patient's liking as a good sign, and leave it at that, or skilfully turn the patient down if there is dislike, which may be mutual? Emphatically no! The relationship must be used as the pivot of the treatment, as the sensitizer for determining the measure of the patient's progress, and sometimes as the one prop to which the patient can cling when he suddenly sees life as illusion, and is groping in the dark for material with which to build anew.

So in the first place the physician must be willing to accept the relationship, in all its intensity if necessary, as a temporary splint. Some analysts tell their patients that they will not have them falling in love with them—you might as well refuse to offer a finger to a baby who is learning to walk, it would be as rational. With patients who are not very ill because their adaptation to reality has been fairly good, there is not much need of the splint; they, as it were, have only a sprain. But in the very bad cases where reactions have been entirely infantile, and contact with reality has been prevented by a life-long screen of phantasy, the patient literally does not know where or to whom to turn when the screen has been destroyed; he is like the healed blind man who saw men as trees walking; everything is out of perspective, and until his vision has accommodated to the new conditions he needs must have a prop. A careful building up of the positive relationship at the beginning of treatment may save patient and physician from a catastrophe as the reductive analysis proceeds. Some may ask if it is necessary to pull down the screen of phantasy, but often it is life itself that has destroyed the

screen, and the too sudden smashing down of buffers in a person with small capacity for adaptation is the cause of the neurosis; then the physician is the one fixed point to which the patient can cling until he has gained the sense of his own solidity.

This building up of a transference needs courage and sincerity on the part of the physician. Our natural reserve does not make it easy for us deliberately to ask our patients what they think of us. Some patients, of course, discharge their own feelings of psychological discomfort in biting criticisms of the doctor; others again are too shy to be honest in their criticisms, and have patiently to be shown that a frank expression of their like or dislike of the doctor is part of their contribution to the treatment, as the wishes and fears projected on to the physician reproduce forgotten attitudes to ideas and people important to them in the past. It is not easy to face the full blast of a negative transference when we are subjected to accumulated long repressed feelings and ideas of a very unpleasant nature; but our own discomfort must be merged in the realization of the still greater discomfort of the patient, and of the urgent need of bringing such feelings out into the open, lest after further repression and accumulation they emerge at length with insane—perhaps paranoic—force. The doctor must remember, too, that any criticism may not be wholly a projection of the patient's psychology; it may also be a true criticism of the physician. Our patients may be ill, but many of them are by no means fools, though perhaps it is easier for us to realize that fact from the very pertinent remarks they make about other physicians whom they have visited, than from their criticisms upon ourselves. But if we recognize the criticism as true of ourselves, and are not ashamed to avow it, barriers may be destroyed, and progress hastened, whereas a refusal to discuss the criticism may perpetuate a feeling of distrust, and a sense of inferiority on the part of the patient. It is practically a continuation of the adult attitude to children, "You must not say that to father, it's rude." Sometimes the criticisms show that it is advisable to alter one's manner to certain patients, to become less calm and more aggressive, for example, or to appear stupid, in order to make a lazy patient work.

And, lastly, the physician must welcome and encourage the growing independence of the patient. It seems to me the patient is trying his new-found wings at the expense of the doctor. In youth he is always under authority; people may criticize him and be rude to him, but he must not answer or be rude in his turn, or he must not talk about things of great interest to him; he is perpetually competing against people of greater physical and mental capacity than his own (this is especially noticeable in the case of the youngest children of large families, who, in my opinion, are more to be pitied than only children), and he is perpetually competing against his own over-valuation—repressing here, and striving there—and then, at last, he meets with someone who regards him, and expects, in essential matters, to be regarded by him as an equal struggling along the path of life; who sees him as un-understanding perhaps, but not a hopeless failure; and so, gradually he begins to take the doctor at his word, testing each new adaptation by its effect upon the physician, for the measure of his relationship to this teacher is the measure of his relationship to the world; if he can talk and behave as an equal to his doctor, surely he can meet the world on equal terms. And so, his old infantile reactions having been projected on to the doctor, accepted calmly by the doctor, discussed and understood, he finds himself at last face to face with him as an adult, and can replace his original

infantile attitude by an ordinary human relationship between two adult beings. And as these adaptations proceed, we notice changes in the patient. We feel that we are talking to an equal and not to a child. The face of an infantile patient will become the face of a woman in a few weeks of treatment, lisps and high-pitched voices disappear. In short, the patient, having found and accepted her own hole, instead of thinking that everybody else's hole is better than her own, no longer needs all the camouflage and mannerisms of the past.

I have taken the relationship mainly from the point of view of the analyst, but naturally it affects the situation in hypnotism and re-education.

I have spoken of the possible tendency on the part of a physician with an unrecognized power-complex, to dominate his patients too much, and unfortunately some patients with a parent-complex like to be dominated; they prefer the attitude of the sleeping child to the protecting parent, or perhaps of the woman to the more dominating male; they do not want self-revelation, and the stress and struggle of re-orientation to reality; "Rocked in the cradle of the deep, I lay me down in peace to sleep" is their attitude to life, and their dependence is not lessened by hypnotic treatment alone. Other patients, on the contrary, those with an authority-complex, dread the apparent self-surrender to, and domination by the hypnotist. In the past, many patients have told me they did not want to be hypnotized, and had I overpersuaded them, they would almost certainly unconsciously have resisted my efforts: but when they realized it was purely a technique, and that by following my directions they could put themselves into a state of receptiveness without a word or movement on my part, they readily gave me permission to deepen the self-induced hypnosis, and to reinforce their suggestions.

Hypnosis appears to be a means of increasing our identification with a given idea, so that more and more we come under the unconscious domination of that idea, and are therefore less our own masters. If the reinforced idea produced an unpleasant instead of a pleasant feeling tone, we should call it an obsession. This increase of identification with pleasant imaginative ideas I believe to be the rationale of the Coué system: whether it leads towards individual psychological freedom is a doubtful matter.

I am glad to have had the opportunity of introducing this subject for discussion as I am convinced that a great deal of the distrust of the transference is due to sheer misunderstanding. Having worked under three well-known analysts and come safely through a negative transference to one of them, and having previously had considerable experience in hypnotic and re-educative methods of treatment, I can speak from practical experience. I have often been distressed at the view of the transference expressed by the medical profession and the lay public. A few months ago a patient told me that she had been to a lecture the day before in which the lecturer had said that everyone must fall in love with the analyst, and she did not want to fall in love with me. As a matter of fact, I was able to show her a few minutes later that, far from falling in love with me, she had a slight negative attitude towards me as she was identifying me with a head mistress of whom she had been afraid when a child. When she became conscious of—and understood—this negative attitude to me, it disappeared.

In the *British Medical Journal* of December 2, 1922, I read the following in a review of a book on Crime and Analysis:—

"The possible, or even probable, risk during the process of psycho-analysis of the transference of the patient's feelings towards other persons (whatever the feelings may

be) to the psycho-analyst himself is strongly emphasized by the author. 'It will be seen,' he writes, 'that psycho-analysis is a method of great difficulty, requiring knowledge of a special technique, much experience, and perhaps certain gifts which are not possessed by all. It is a trying process for the psycho-analyst; until an attempt has been made no one can conceive what a severe strain this method is.'"

Then the reviewer goes on to say :—

"One reflection following on the reading of these words of his is that a very small proportion of men or women doctors could possibly be expected to combine all the qualities necessary to practise psycho-analysis properly, even were its soundness, as based on science, and its successful application in practice, to be generally admitted."

I accept the words "the possible, or even probable risk," just as I accept the possible, or even probable, risk arising from the administration of an anæsthetic if given by an insufficiently skilled administrator: in both cases, the risk is proportional to the skill of the administrator and the concentration of the drug, and it is lessened by the due administration of air. If the feelings are projected on to the physician in bulk, and are allowed to accumulate and fester without ventilation or relief, I admit the risk; and that is why one should be as careful in the choice of an analyst as in the choice of an anæsthetist. But if the relationship is kept as a pressure gauge between the two, it becomes an indicator of safety, and forms no abnormal tie when the treatment is over, whereas in hypnotic work the relationship is not discussed, and I have seen violent transferences occur. The same emotional situations arise between priest and penitent in the religious world. Try how you will, you cannot prevent such relationships, and therefore it is much wiser to keep them well ventilated, and to make of them an instrument and not a risk.

DISCUSSION.

Dr. CRICHTON MILLER said that Dr. Mary Bell had admirably expressed the Zürich view of the transference but he felt that she had not made it sufficiently clear that transference might take the form of any sort of relationship; not merely that of the child to the father or mother, but of brother to sister, brother to brother, and so on. In fact, any conceivable human relationship could be represented in the transference. The aim of the analyst should be to transform transference to the relationship of the climber to the guide, and to eliminate the element of authority progressively.

Dr. H. G. BAYNES said that having recently spent three years in assisting Dr. Jung his remarks might be regarded as representing Jung's present standpoint on the question of transference. Jung, he said, looked upon the transference as *the* dynamic factor underlying the whole analytical process. The analyst reaped the advantage of this energetic value not from any special personal value he might possess, but because he represented the possibility of a new attitude. Hence he appeared as a function of life, thus becoming associated with the figure of father, husband or priest, whose psychic archetypes gained their energetic value from the same fact. Since the aim of the libido was transformation and expression the analyst was bound up with the whole complex of metamorphosis, and the nature of the transference determined the nature of the analytical transformation. The transference, Jung held, was inherent in the analytical process and quite outside the analyst's intention. This being so, the analyst could not evade the responsibility it entailed. If the responsibility was admitted, the further problem of individual development could not well be avoided, carrying with it the need of investigation, and, if possible, formulation of the principles of human development. Since no interpretation of any sort was possible without a standpoint, Jung maintained that criticism of standpoint was more essential than criticism of method.

The transference might be regarded, then, as the life-values of the libido seeking expression in the form of human relationship. Although fundamentally instinctive, the transference only took a frankly sexual form as a *faute de mieux* when the more conscious and synthetic relationship was either denied or ignored. When individuation was accepted as a conscious and deliberate aim on both sides, the purely instinctive quality of the transference quickly disappeared. The only cure for the positive transference was to transform it into individual relationship. In this relationship the life-values that were seeking expression in the blind instinctive transference attained conscious appreciation and recognition. Hence the sincerity of the analyst was a factor of greater moment than the particular method employed.

Dr. MARY BARKAS said that the remarks of the previous speaker showed clearly one of the fundamental differences between the schools of Jung and of Freud—namely, that Jung believed that the analyst should impose his own ideals of moral values and personality on the patient, whereas Freud held that the analyst should be a neutral person, acting like a mirror in reflecting the patient to his own sight, and being guided entirely by the workings of the patient's own psychology.

Dr. W. A. POTTS said that he also had noticed the importance Freud attached to re-education in his latest work, "Introductory Lectures on Psycho-analysis"; he also noted that Freud said that repressions should be dealt with by suggestion. Dr. Potts concluded therefore that Freud's method was to drive his own ideas home; so that it seemed at first as if he had deserted his own school. On further reflection Dr. Potts thought this was not the case, because recently at a medical meeting, where most of those present were Freudians, when he (Dr. Potts) had said that psychological analysis achieved its end by giving more light, his hearers had insisted that any good done must have been effected by suggestion. Dr. Potts could not agree with Dr. Bell that it was ever right to "act"; the analyst should always be natural and just himself; nor could he agree that intuition necessarily led astray; at any rate he knew how useful intuition had been in his own analysis, and he also knew that in the case of some of his own patients it had not led him astray; he thought intuition should be developed. An objection had been made to going to a personal friend for analysis, but he (Dr. Potts) supposed there were different points of view, because recently after he had given some lectures to nurses on this subject, one of them said she would like to have such treatment, except for the reflection that it meant telling intimate facts to a stranger. His (Dr. Potts's) experience had been that there was no special difficulty in taking an acquaintance as a patient, but he felt that it would not be wise to try to treat an intimate friend.

Section of Psychiatry.

President—Dr. C. HUBERT BOND, C.B.E.

The Endocrine Factor in Mental Disease.

By J. LEITCH WILSON, M.B., D.P.M.

ENDOCRINOLOGY has been prominent in recent years in every branch of medicine, and its relation to mental disease was discussed at the meeting of the Medico-Psychological Association in Edinburgh, July 1922.

Sir Frederick Mott, especially, has excited the interest of all students of mental pathology, by his discoveries with reference to the sex glands in dementia præcox, and in recent papers he has laid stress on the importance of the internal secretions as factors in all mental disorders.

The recent researches of Cannon [1], by which he has demonstrated a change in the internal secretions as one of the results of an affective state, and the work of Golla [2] in investigating the various objective signs of the neuroses, show that the present tendency is to make some attempt to correlate the work of the psychologist with that of the physiologist.

Unfortunately in our special branch of medicine a great deal of careful research as to the causation of mental troubles has been lost to science because of the schism between the psychogenetic and the physicogenetic schools. This schism is due to the fact that their efforts can never be collaborated to advance our knowledge so long as they are expressed in different languages. The technical terms and the findings of the psychologist can never be tested at the post-mortem table nor up to the present has the materialist been able to explain human behaviour in terms of neurones and dendrites. Now, however, with the work of Cannon and Golla showing methods by which the state of the mind may be tested objectively, a new era has arisen in the study of mental pathology, and it is from this point of view that I have chosen as the subject of my paper, "Endocrines in Relation to Mental Diseases." I am fully cognizant of the difficulties of the subject, but I hope it will be remembered that our choice in mental science lies between an attempt to explain psychical symptoms on a physiological basis and an acceptance of the purely speculative concepts of psychology.

THE BIOLOGICAL RELATIONSHIP OF THE ENDOCRINE SYSTEM.

The endocrine glands may be defined as certain specialized glandular tissues—widely distributed in the body—which pass their secretions directly into the blood. Each of these internal secretions has a specific stimulative or inhibitive effect on the metabolic processes, differing according to the gland

of origin. There is, however, such a close inter-relationship between the endocrine glands that their secretions act physiologically in concert, forming what is known as the "endocrine balance." The principal glands usually included in the system are the pituitary, thyroid, parathyroid, adrenal bodies, pancreas and gonads, and to these may be added others such as the thymus, the pineal body, the glands of the intestinal and gastric mucosa, &c., as better knowledge of their functions becomes available. Ontogenetically, most of these structures appear from the fourth to the sixth week of foetal life, the anterior or buccal part of the pituitary gland being the first differentiated. With regard to the adrenal body, Keith [3] says that "although arising from the blastema of the sympathetic system it is differentiated before the nerve cells of that system as if it represented the product of an earlier evolution." Phylogenetically, as the endocrine system produces its effects by chemical activity, it appears to have a morphological relationship to a similar system in primitive forms of life—where the linking up of the various chemical products, by a circulatory apparatus, fulfils, in the absence of a nervous system, all the adjustments necessary to adapt the organisms to its environment. Jelliffe [4] holds that the endocrinous gland system is phylogenetically the oldest part of the vegetative nervous system. Langdon Brown [5], in a biological study of the endocrines, concludes that "the internal secretions result from the specialization of the old chemiotactic mechanisms to which primitive animals reacted before there was a nervous system at all." Loeb [6] has shown that even in more highly organized animals such as bees and wasps, possessing a nervous system, the instinctive behaviour may be heliotropic in character. In certain caterpillars the motions of the legs are automatically controlled by the chemical changes taking place in symmetrical elements of the retina under the influence of light. So that, even in the presence of a nervous system, the real adaptation of the creature to its surroundings may still remain under the control of its chemical activities.

Carpenter [7] describes the Ascidian mollusc as illustrating the simplest form of nervous system consisting of a single ganglion with afferent and efferent fibres. Its function is to prevent noxious particles from entering the pharyngeal sac by a sudden contraction of its muscular envelope in order to expel the contents of the sac. Here we have an illustration of three principles which still apply to all forms of life: (1) That a rapid response to noxious stimuli is the first necessity for continued existence; (2) that the whole vital power can be mobilized for self-defence; (3) that nutrition is interrupted under the imperative need of self-preservation.

It is interesting also to note that, in the primitive form, the nervous system is first developed for the purpose of preserving the organism from injury, because, when an attempt is made to trace the relationship of the endocrine system to the nervous system in vertebrates the connecting link is found in the chromaffin tissue of the adrenal medulla, an organ primarily related to the defence mechanism of the body.

The question then arises, in view of the enormous development of the central nervous system, to what extent it has taken over the duties and responsibilities of the older chemical or glandular system, and whether the older system, while delegating to the more efficient machine all the finer adjustments and adaptations of human behaviour, may not have preserved to itself the orientation of the whole organism, in subserving the primal instincts of nutrition, self-preservation, and propagation of species.

In an attempt to estimate the comparative functional value of the two

systems, we know from our study of physiology and neurology that, from the simple reflex to the highest intellectual development, the nervous system tends to automatic machine-like action. In a simple reflex the appropriate stimulus will always produce the same quality of response. In conditioned reflexes, which involve the association areas, the same stimulus will go on producing the same response until a new association is formed. There is always a tendency to the formation of habit mechanisms and even habits of thought processes. Neurologists know that certain tracts have specified functions and when these tracts are destroyed by injury or disease their functions are definitely lost and on this principle the diagnosis is based. Sherrington [8] has shown that the most recently developed cortical areas of the brain are stereotyped as to function; and Darwin [9] has pointed out that emotional expression in man and animals is automatic in its character.

On the other hand the endocrine system with its constant ebb and flow of chemical activity, its intimate connexion with all body metabolism, and its close relationship with the nervous system, seems more fitted to be the originator and controller of that wonderfully complex ever-changing syndrome, human conduct.

In a study of the basis of behaviour it may be helpful to quote the view of a modern psychologist. McDougall [10] says:—

"I am convinced that it is only . . . by recognizing the essential similarity of human *instincts* to those of animals . . . that the continuity of the human with the animal mind can be displayed, and that a science of human character and will can be built up."

And he defines instinct as follows:—

"Each innate specific conative tendency has at its service an innately co-ordinated system of motor or efferent nerve channels: . . . these belong together functionally and phylogenetically as one feature, one psycho-physical disposition of the inherited constitution of the organism: . . . each implies the other; . . . instinctive activity always involves their co-operation: and . . . of the two the conative tendency is the more essential and fundamental feature of the total innate disposition which is the *instinct*."

There are some indications that, by a study of endocrine physiology the physical basis of the "innate specific conative tendencies" may be found, just as the "innately co-ordinative system of motor or efferent nerve channels" is based on the nervous system. I propose therefore to discuss the functions of the various internally secreting glands in relation to the primal instincts of nutrition, sex, and preservation from injury.

(1) *Endocrines governing Nutrition.*

The *pituitary gland* has the peculiarity of being the least accessible and best protected gland in the body. Its extirpation in animals is followed by death in two or three days, whereas decerebrate cats—where all the brain in front of the pons is removed—can be kept alive in some cases for three weeks (Bazett and Penfield [11]). In man hyperpituitarism as associated with adenoma of the anterior lobe presents the remarkable phenomenon of *acromegaly*—a gradual anabolic change which alters the whole physiognomy of the affected person by heterogeneous development of certain structures. The brows become prominent, the malar bones and lower jaw are increased in size, the tongue and lips are thickened. There is enlargement of the chest;

the heart, lungs, arteries and veins hypertrophy, and the renal substance is increased. The most typical effects are seen in the hands and feet, which are uniformly enlarged—bones, joint surfaces, ligaments and muscles. Keith [12] calls this disease a “disorderly manifestation of all the results which follow increased use of muscle-bone strength.” There is also some evidence that gigantism is caused by hypersecretion of the pituitary gland in early life. Here is an example of a gland which, by its excessive function, appears to exercise on the anabolic processes of the whole body an intelligent co-ordinating influence, over a considerable period of time, producing a new type of human being.

On the other hand, when a part of the pituitary gland is removed from young animals they fail to develop properly; milk teeth and lanugo are retained, epiphyses do not ankylose, the thyroid and thymus are enlarged, the cortex of the suprarenal bodies and the sexual organs fail to develop, the animal becomes fat and there is *distinct evidence of mental dullness* (Macleod [13]).

Another interesting point is the enlargement of the pituitary to two or three times its size in pregnancy just at the time when the foetus is being nourished *in utero* and when there is going on in the mother a definite purposive anabolic change affecting the circulatory, skeletal and reproductive systems.

The *thyroid gland*, like the pituitary, shows increased function during pregnancy, and also when there is loss of pituitary substance. There is hypertrophy of the pituitary in thyroid deficiency. The powerful influence of the thyroid on growth and development is so well known that only a brief reference is necessary. The stunted growth of body and more particularly the *lack of mental development* caused by insufficiency of its secretion, and the restoration of normal conditions by thyroid feeding, furnish one of the most potent arguments that glandular activities are the physical basis of our instinctive tendencies. In the complete absence of thyroid in early life there will be no mind—in other words idiocy. Then again, it is necessary that this gland should function throughout life to preserve mental power, for if withdrawn in later life myxœdema arises, causing a gradual approach to dementia.

The *adrenal cortex* is also associated with body growth. It has been shown [14] that in the human foetus there is a great development of the innermost part of the cortex which gradually disappears after birth—and that in anencephaly this zone is absent—a fact which suggests that there is probably a causal relationship between the adrenal cortex and brain development. This gland is also enlarged during pregnancy. Its removal is incompatible with life as shown in recent animal experiments by Wislocki and Crowe [15].

The *pancreas*, through its internal secretion, insulin, also takes part in anabolic activity and the *pineal gland* is said to exercise a restraining influence on sexual development during the stage of growth in childhood.

(2) *Endocrines directing Sexual Development.*

The relationship of the internal secretions of the gonads to the physical conformation of the adult male and female has been the subject of a great number of experimental investigations of recent years. It is now generally accepted that the testis, by the production of an internal secretion, causes the development of all the secondary male sexual characters. The ovarian secretion helps to develop and maintain the secondary female sex characters

such as the growth of the uterus and mammary glands, and also determines the onset of menstruation. The most important effect of these secretions, however, is evidenced by a profound alteration in the mental outlook, the beginning of a new epoch during which the emotional capacity is enormously developed. The brief outbursts of joy, sorrow, chagrin and anger which swiftly appear and disappear in the child, become at puberty the more lasting moods of exaltation, depression, anxiety and bad temper with occasional storms of extreme hilarity, morbid impulses, anguish and angry passion.

While the gonads are primarily responsible for all these changes, there is much evidence to prove that other glands, the adrenal cortex, the thyroid and the pituitary, play an important part in sexual development. There is ample proof that the causal factors of all phases of sexual activity may be found in the endocrine system.

(3) *Endocrines concerned in Self-preservation.*

That the *adrenal medulla* plays a very important part in the defence mechanism of the body has been demonstrated by the experimental injection of adrenalin into an animal intravenously. The effects produced are consistent with a general stimulation of the sympathetic division of the vegetative nervous system. There is raising of blood-pressure, increase of rate and amplitude of the heart's action, withdrawal of blood from the abdominal cavity to increase the skeletal supply, thus insuring a better blood supply to the voluntary muscles. The bronchioles of the lungs are dilated to allow of an increased respiratory exchange. The glycogen stores of the liver are tapped to increment the blood sugar preparatory to more efficient muscular action, and from the same organ comes a substance which hastens the clotting of the blood in case of injury. The pupils of the eyes dilate to facilitate visual observation. The sweat glands are stimulated to keep the skin cool in action, and finally there is an interruption of the digestive processes due to the inhibitory effect on the smooth muscle of stomach and intestine. Thus we have a clinical picture of the whole organism adjusted to the best advantage for the purpose of offensive or defensive action. Cannon [16] has demonstrated by animal experiments that psychical stimuli producing physical signs of the major emotions (pain, fear and rage) cause a physiological secretion of adrenalin into the blood as one of the visceral changes which accompany the emotion.

The *thyroid gland* also takes a part in the bodily reaction to noxious stimuli through its internal secretion, which is believed to have an antitoxic function and therefore is a defence against disease. But it is also involved in protecting against painful psychical stimuli—fright, mental distress and violent emotions are given by McCarrison as causes of exophthalmic goitre which is accompanied by hypersecretion of the thyroid. A temporary enlargement of the gland has often been noticed under conditions which produce the emotion of fear.

This short sketch of the endocrine system gives sufficient data to show that the "innate conative tendencies" (McDougall) have a close relationship with the functioning of the various glands of internal secretion.

RELATIONSHIP OF EMOTION TO INTERNAL SECRETIONS.

Considered physiologically, emotion may be defined as a psychical experience which stimulates the whole body to mobilize its latent energy for the purpose

of augmenting, prolonging and perfecting, or causing inhibition of, normal instinctive action. Sherrington [18] says: "The pseudo-affective reactions indicative of resentment and defence are, after ablation of the cerebral cortex, short-lived, the simulacra of mere flashes of mimetic passion." "Reflexes to which emotion is adjunct are not only prepotent but are imperative, that is, volition cannot easily suppress them." The pseudo-affective reflexes of Sherrington in decerebrate animals are of great interest because they demonstrate the process of simple instinctive behaviour divorced artificially from its emotional component. This process may be described in three stages: (1) the stimulus; (2) necessary bodily adjustments preparatory to (3) resulting conduct. The intervention of emotion in this reflex arc may be stimulative or inhibitive in its action and it may be interpolated either before or after the first instinctive act has taken place. (a) Emotion may be added before action, in order, by mobilizing reserve energy, to increase the effect of the response—for example, a man becomes angry before striking out; (b) be felt after the first instinctive response has taken place when its function is to augment and prolong the bodily adjustment in preparation for continued activity. This is instanced by the sinking feeling which arises after a narrow escape from danger, a preparation for further eventualities; (c) intervene to inhibit instinctive action—as when we feel prepared to perform some feat but our "heart fails us"; or (d) may succeed the instinctive act to prevent further activity as when we have done something of which we feel ashamed.

In discussing the genesis of the emotions, psychologists relate them to the instinctive tendencies and regard each affective state as one of the means by which an instinct expresses its influence on behaviour—"the arrest of an instinct is that which most frequently excites the emotion connected with it" (Shand, quoted by Prideaux). Prideaux [19] defines emotion as "a subjective feeling . . . occasioned by situations which powerfully oppose or facilitate the aim of any instinctive impulse." Now, if we correlate the instinctive tendencies with the glands of internal secretion, we should expect to find a change in their secretion associated with the emotions, and this change has actually been demonstrated by Cannon. We know from personal experience that there is an appreciable latent period between the exciting cause and the subjective feeling of an emotion, which is quite consistent with the time required for an alteration in the endocrine balance to affect the central nervous system. Such an effect will be twofold—a stimulation or inhibition of bodily processes, and the efflorescence in consciousness of an affective state.

THE ENDOCRINE SYSTEM AS THE BASIS OF FEELING-TONE OR PSYCHÆSTHESIA.

The primal instincts have been associated in the first part of this paper with the functioning of the glands of internal secretions, and the emotions have been attributed to changes in these secretions. Affective states, however, are not essential to everyday behaviour, and there are all gradations of emotion in quality, intensity and extensity, so we must conclude that they do not arise *de novo*, but are simply variations of the normal processes of the bodily economy. There is, then, a pre-existing feeling-tone upon which the emotion is superimposed, and to which the body normally reverts when the emotional crisis has passed, and this normal state of feeling, which we may call psychæsthesia, represents the balance struck by the various primal instincts which govern and direct all the adaptive processes of the body. From the physiological stand-

point, psychæsthesia is the crystallization in consciousness of the resultant effect of all the endocrine secretions working in concert, and, through the nervous system, it determines and energizes all behaviouristic phenomena in response to stimuli of external and internal origin. Golla [20] says: "Our strongest bond with reality is that of the feelings, which constitute our strongest defence against the irrational. Mind dissociated from feeling is mind very much at the mercy of any suggestion."

Mott [21] considers that "sexual desire, upon which so many of our mental activities directly and indirectly depend, is primarily due to the sensitizing influence of bodily structures on the brain."

NORMAL VARIATIONS IN PSYCHÆSTHESIA.

The endocrine system, functioning through the blood-stream, must be kept at a certain level of chemical concentration in order to support life, and therefore the sudden loss of some of its constituent glands, e.g., pituitary or adrenal, causes death. The potential, however, may vary within certain limits in the normal individual, and, in fact, it is always in a state of flux, either through the influence of physical factors such as nutritional disturbance, or psychical stimuli, such as the hearing of good or bad news or the sight of a friend.

A more pronounced psychæsthetic change is produced by the taking of alcohol, after which there is increase of pulse-rate followed by activity of glandular secretion, causing a feeling of power and well-being, and shortening the latent period of reflex activity and of behaviour responses in general. This temporary exaltation of the feeling-tone results in a subsequent reaction, with lessened glandular activity, feeling of depression, slowing of metabolic processes, and conduct of a negativistic resistive type.

To illustrate the effects of a psychical stimulus, I quote from Darwin [22] the case reported by Crichton-Browne:—

A young man, hearing by telegram that a fortune had been bequeathed him, first became pale, then exhilarated, and soon in the highest spirits, but flushed and very restless — later staggering in his gait, uproariously laughing, irritable in temper, incessantly talking and singing loudly in the public streets—there was no question of spirituous liquor, though every one thought he was intoxicated. Later there was vomiting, then he slept heavily, and was well on awakening except that he suffered from headache, nausea and prostration.

There is no need to dwell on the normal changes in the psychæsthesia, as these are familiar to all, and instances of alterations of behaviour based on feeling can be multiplied in the experience of everyone.

The relationship of the instincts and feelings to the endocrine system and to conduct may now be summarized in the following two propositions:—

(1) The primitive instincts, based on the endocrine system, are, in health, harmoniously co-ordinated, each reinforcing or inhibiting the other for the general welfare of the whole organism.

(2) The resultant of their interaction is represented by the endocrine balance, and this, acting through the nervous system, determines the affective tone and at the same time energizes and directs or inhibits the bodily behaviour in response to stimuli.

MORBID STATES OF FEELING AS CAUSES OF MENTAL SYMPTOMS.

A change in the affective tone is one of the outstanding features in a clinical picture of mental disease, and is well illustrated in states of mania, melancholia and dementia præcox.

Mania.

The early stages of mania are characterized by a gradual change in the emotional outlook, a feeling of well-being and increased capacity for active pursuits. This alteration of feeling is reflected in movement (as Sherrington says: "Emotion moves us")—and there is at the same time a "liveliness" among the associated mechanisms in the brain. There is a speeding-up of thought, speech and action, as observed in the flow of ideas, the rapid articulation and the waves of emotional expression. Afferent stimuli are also sensitized so as to cause hyperacuity of all the senses. As the tide of exaltation rises, activity becomes more manifest and less purposive, conversation is garrulous and disconnected, instinctive processes are more easily aroused and less controlled. When the acme of the attack is reached, the slightest stimulus, even the revival of past impressions in the form of voices or visions, is sufficient to cause an explosion in instinctive behaviour, shouting, singing, cursing and unrestrained violence—a state of mental tetanus or *psycho-paræsthesia*. It seems as if some subterranean volcano had erupted, pouring out smoke and flames into the atmosphere without serving any useful purpose, except to relieve the great pressure within.

As the attack subsides, there is a retrogression of all the symptoms, coincident with a lowering of the feeling-tone and a gradual return to normal, often followed by a reactionary period of depression.

An example of a more subacute type of exaltation is furnished by an early case of general paralysis. The patient says he never felt better in his life, he feels a Samson in strength, and as if he were walking on air. Consequently all bodily and intellectual processes seem easy to him, and on this groundwork are built up all his expansive delusions. No criticism can reach him, because in his elated state he only pities anyone who is not so fortunately placed as himself. I have had lately under my care a typical case of general paralysis, a patient with maniacal symptoms and full of grandiose ideas, e.g., "that he could drive twenty golf balls 300 yards into a space of 2 ft. square, where they would be all piled up into a pyramid." He was treated with injections of phlogetan, and within a month his mental symptoms had disappeared. During that time there was a gradual diminuendo of his feeling-tone, *pari passu* with the clearing up of his delusions, as instanced by his conversation and the letters he wrote. The idea of great muscular power was the last to go, and disappeared rather suddenly when he collapsed after a walk in the grounds. The interesting part of this case is that the physical signs, tremor of face, fibrillary tremor of tongue, and marked hesitation of speech became more pronounced after the mental symptoms had disappeared. It would seem, therefore, that the treatment, while it had a minimal effect on the organic lesion, apparently cured the mental symptoms, which would be in accordance with the hypothesis that the latter were of glandular origin.

Melancholia.

A consideration of the symptoms of melancholia suggests a partial loss of the capacity of feeling in varying degrees. The opportunity presented itself of studying closely the case of a very intellectual lady who had suffered for some years with involutionary melancholia which had a sudden fatal termination. In the intervals when she seemed to be less despondent, she conversed freely on her symptoms and was able to describe very clearly her emotional tone. She persisted in the statement that there was an entire lack of sensation which affected the whole body; that her brain was dead; that no matter what she ate she did not taste; she read books and understood them, but they had no meaning for her. Objectively, there was no loss of skin sensation, protopathic or epicritic, and hearing, sight and smell, were normal. The conclusion drawn was that there was a real loss of the capacity to feel and therefore ordinary sensations did not react on the mind in the normal way—this produced a feeling of lifelessness or deadness summed up in the expression “my brain is dead.” This patient gave a very clear description of the lack of psychical feeling. She said: “When I am told that my friends have come to see me I feel no emotion either pleasurable or otherwise; I meet them and know who they are, and that I ought to be happy to see them, yet I can feel nothing, therefore I would rather not see them.” The lack of feeling-capacity in depressed states seems to be the origin of the delusions—which are an attempt by the patients to account for their abnormal affective tone—e.g., “there is a devil inside,” “the taking of food feeds the evil spirit within.” “I ought to be in Hell!” and “I am going to die”; “food is mixed with filth,” &c. A great many melancholics seem to approach very near to a correct diagnosis when they say “My soul is lost.”

Dementia Præcox.

In the case of dementia præcox there appears to be a lack of emotional development at the critical stage of puberty, and the patient's power of emotional sensation becomes stunted. He cannot understand or interpret the various stimuli from within or without which appeal to the æsthetic side of his mind, because he fails to develop the necessary sensitive plate on which these sensations impinge. There is a strange aloofness from ordinary life with a loss of comprehension of soul-stirring events that appeal strongly to his fellows.

White gives a good description of the emotional deterioration:—

“The expressions of joy or sorrow, if they occur at all, are shallow and of short duration. A death, a birth, a marriage, the visit of a long absent relative, are all apprehended with the same lack of emotional expression. No matter how much pleasure or pain the event might be supposed to give, or would give in a normal person, the patient receives it with indifference, without surprise, without an expression of interest, often in the most matter-of-fact sort of way, as if such things were occurring hourly.”

Such a lack of the power of feeling may be termed psych-anæsthesia, an absence of driving force, leading to want of co-ordination in thought and action, erratic and child-like responses, and all the vagaries of behaviour characteristic of loss of mind.

This is in accordance with Mott's [24] conclusions on pathological grounds, that there is in dementia præcox an inborn germinal deficiency of productive energy of the reproductive organs, associated with a progressive deterioration of psycho-physical energy.

Towards the end of life there may occur such a degeneration of the emotional apparatus as to constitute an almost complete loss of feeling-capacity, as illustrated by cases of senile dementia.

There may also be mentioned the temporary paralysis of feeling tone which occurs during the more active period of life—due to shock, illness or acute mental disease, viz., the various stuporose states. The characteristic symptom of stupor appears to be a loss of energizing or emotional capacity which may last from a few hours to a few months, an acute and often curable form of anæsthesia of the psyche.

CONCLUSIONS.

(1) The gross changes in behaviour, characteristic of mental disease, have not up to the present been correlated with any definite lesion in the nervous system, therefore it is justifiable to look elsewhere for the underlying cause of mental symptoms.

(2) The endocrine glands and their apparently close relationship with the primitive instincts and emotions offer a field of study which may be prolific in results to the alienist as well as to the physiologist.

(3) There is evidence that the system of endocrine secretions may be affected pathologically by both psychical and physical causes.

(4) The endocrine system may prove to be the mechanism through which mental disease is produced.

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DISCUSSION.

Dr. F. PARKES WEBER alluded to the mental symptoms sometimes occurring in endocrine disorders, for instance, in myxœdema and Graves' disease, and perhaps also in pituitary gland diseases. Possibly mental symptoms in cases of syphilitic meningo-encephalitis might be partly of endocrine origin, due to involvement of the pituitary gland. He suggested that the well-known euphoria of advanced pulmonary tuberculosis and of severe septic conditions might be due to the effect of the toxæmic condition on the endocrine system. (Similarly, it might be suggested that the endocrine system played a part in puerperal insanity, post-pneumonic mania, &c.)

Dr. H. CRICHTON MILLER said that the question of hormonal balance was to his mind the real crux of psycho-physical interaction. We should never understand the relation of mind to body, and body to mind, nor yet the physical factors influencing mental disease—or the mental factors influencing organic disease—until we had recognized the importance of the endocrine balance. He (Dr. Crichton Miller) regarded the endocrine system as a sort of diaphragm between body and mind, which could be influenced from either side, and already it was universally accepted that such an intermediary existed. For instance, no one doubted the emotional result of hypothyroidism; nor did anyone question the physical result of fear, as manifested in hyper-adrenia. If, therefore, we regarded endocrines in the light of an intermediate zone between mind and body, we would find it easier to understand such relationship as that existing in the ætiology of dementia præcox, namely, the gonad deficiency, so completely determined by Sir Frederick Mott, and the psychological failure of the father-complex, as detected by the analytical researches of Jung and others.

Dr. C. F. HARFORD said he spoke as one who was especially studying the psychical aspects of ordinary illness. He had already made various contributions to the psychological side of ophthalmology and more recently of tropical diseases; and this had led him to formulate certain definite conceptions of the relation of the mind to bodily functions. He (Dr. Harford) regarded the mind, and particularly that part of mind-work which was unconscious, as the supreme controlling force of all our powers. He believed that a large part of mind energy was carried out through the intermediation of the endocrines acting through nervous channels. He thought that too much emphasis was laid upon the effect of the endocrines upon mind as compared with the effect of mind upon the endocrines. He believed that psychotherapy might be employed to good purpose in the early stages of all illnesses associated with endocrine disturbance and that much good would result.

Dr. PERCY T. HUGHES said he questioned the scientific accuracy of regarding the secretion of the various endocrine glands in terms of excess or diminution only. Absence or marked diminution of the thyroid secretion was undoubtedly the cause of myxœdema, but was it equally evident that Graves' disease with all its mental accompaniments was due solely to excessive output of normal thyroid secretion? Dr. Hughes suggested that the symptoms of Graves' disease were due not merely to excessive thyroid secretion but to a toxæmia caused by the pathological changes in the thyroid owing to which its secretion was so much altered as to become definitely toxic. He would apply the same suggestion to pituitary conditions resulting in acromegaly. Again, having regard to the pronounced pathological changes in the testicle and ovary in dementia præcox, as described by Sir Frederick Mott, might not the mental symptoms in this disease be due, or partly due, to a toxæmia arising directly from the pathological changes in these glands?

Lieutenant-Colonel HAYNES said he would draw attention to the fact that during the evening nothing had been said regarding the restoration of the endocrine balance. We had no evidence that the action of endocrine extracts introduced by the mouth, or even subcutaneously, was similar to that of the living gland. He suggested that work to ascertain the value of such extracts and the most useful method of administering them would prove of the utmost value.

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Dr. HUBERT BOND (President) said that Dr. Leitch Wilson had collected and summarized much important work that had been done and published upon the subject, and several valuable suggestions had helped to clarify points of difficulty. He (the President) felt encouraged and hopeful for the future when he saw someone whose work was mainly clinical, thus studying and thinking out his cases in terms of laboratory results; and a paper such as this added strength to his (the President's) reiterated contention that sound clinical work could not be carried out in any mental hospital without the aid of the laboratory.

Section of Psychiatry.

President—Dr. C. HUBERT BOND, C.B.E.

Genius and Insanity.

By HUBERT J. NORMAN, M.B.

FOR many centuries it has been recognized that there is a definite correlation between genius and insanity. Aristotle (*"Problemata"* noted) the frequency with which "melancholy" occurred in statesmen, philosophers, poets and others who occupied a prominent position in the community. Seneca, basing his statement on Aristotle's observation, said that "*Nullum magnum ingenium sine mixturae dementiae fuit*" (*De Tranquillitate Animi*). It was from Seneca that Dryden apparently derived this idea, and he embodied it in his "Great wits are sure to madness near allied": the immediate application being to Lord Shaftesbury in "Absalom and Achitophel," in which he is described as:—

"A fiery soul, which, working out its way,
Fretted the pigmy-body to decay,
And o'er-inform'd the tenement of clay."

This is an admirable description of what has been sadly and frequently exemplified in the life-histories of men of genius.

It was, however, not until a later time that the suggestion was worked out in a more definite way, and an endeavour made to deal with it as far as possible from the physiological aspect. Lélut, in 1837, published his book, "*Du Démon de Socrate*," in which he showed conclusively that Socrates suffered from hallucinations. The subject was followed up by other French writers, notably by Morel and by Moreau (de Tours). Moreau indeed went so far as to say that genius was a *névrose*, or nervous disorder, his contention being that "originality of thought and quickness or preponderance of the intellectual faculties were organically much the same thing as madness." This theory was supported by Lombroso in his "*Genius and Madness*," and it is upon Lombroso that most of the odium has fallen for the promulgation of an opinion which, as has often been pointed out, is really a very ancient one. It may be said in passing, however, that Lombroso's views on this matter and his work in criminology deserve respectful consideration rather than the petulant detraction to which they have so often been subjected. It was to Lombroso that Max Nordau dedicated his vigorous and entertaining volume, "*Degeneration*," in which he subjected those whom he considered decadents to the most mordant criticism.

No sketch of the progress of this conception would be complete without

a reference to one of the most able books written on the subject, namely, Nisbet's "The Insanity of Genius" (1891).¹ The work of one who was not a trained psychologist or physiologist, it is remarkable for the manner in which both psychology and physiology are invoked to show that great intellectual power is not found, as a rule, "without some disturbance of the healthy equilibrium of the brain and nervous system." This corollary seems irrefragable: biographies of eminent people reveal the fact that nervous instability is an almost invariable concomitant, while actual insanity occurs with extraordinary frequency.

Problems in psychology—and even in psychiatry—have been too frequently discussed from an abstract point of view. In spite of much that is still said to the contrary, the only real advance has been made from the physical and biological approach. Psychology based upon abstractions which do not take into account the dependence of mental action upon nervous processes must continue to be unfruitful—allowance being made, of course, for the plentiful crop of Dead Sea fruit always to be found in the metaphysical garden! Consider for a moment the weary speculations about dualism and psycho-physical parallelism. In either case we are asked to presuppose a discarnate something which may impinge at will on some part of the brain or body. This kind of supposition has presented us with the fatuities of spiritualism and dowered us with seven thousand devils! Why not in the same way conceive a dualism between Respiration (with a capital R) and lungs, or between Digestion and stomach? It is amazing to have a distinguished scientist saying at the present time that: "Biology is distinct from psychology. . . . Biology must be regarded as an independent science."² While another remarks that: "The gap between the physiological and the psychological is unbridgeable." Apparently when a process becomes so highly refined as to elude our methods of investigation for the time being, all we have to do is to make an arbitrary division, give the later part a new name, then say that it has no connexion with its commencement. "When the human mind could advance no further," said Swedenborg, "it admired its last result and accordingly took it for God."³

It may be taken for granted—here, at any rate—that mental processes are a subject for the study of the physiologist as well as for the psychologist, and it does not matter whether these processes are normal or morbid. Those of the genius differ only in degree and not in kind from those of others. Genius does not depend upon some mysterious inspiration or divine influx, but is the result of a difference of a dynamic kind, of energy working in a structure which differs in the quality and quantity of its nervous arrangements in one individual as compared with another. What "energy" is remains a question for the biologist and the physicist. "Genius to my mind," said Huxley, "means innate capacity of any kind above the average mental level. From a biological point of view I should say a 'genius' among men stands in the same position as a 'sport' among animals and plants, and is a product of that variability which is the postulate of selection both natural and artificial. . . . On the general ground that a strong and therefore distinct abnormal variety is, *ipso facto*, not likely to be so well in harmony with existing conditions as the normal standard, which has been

¹ A new edition, with an introduction by Dr. Bernard Hollander, was published in 1912 (Lond., Stanley Paul and Co.).

² J. S. Haldane, "The Fundamental Conceptions of Biology," *Brit. Med. Journ.*, March 3, 1923, p. 363.

³ Swedenborg, "The Infinite and the Final Cause of Creation," London, 1908, p. 81.

brought to be what it is largely by the operation of those conditions, I should think it probable that a large proportion of 'genius sports' are likely to come to grief, physically and socially."¹ Obviously Huxley does not use the word "innate" in the sense in which it was employed by some philosophical writers, but rather as implying the inborn difference of potential which enables one person to develop more rapidly or more fully than another. Hugh Elliot² suggests that: "The genius is a man in whom mental energy is distributed unevenly—great strength in alliance with weakness"; and he points out that there must be intellectual capacity as well as emotional abnormality. "Intellect alone can accomplish nothing; it is the emotional force pushing from behind that is the prime agency."

There does not seem to be the necessity in such individuals for that "transcendent capacity of taking trouble," which Carlyle mentions as the attribute of genius. The mind of the genius is of such a kind that it readily absorbs information—that is, the cerebral reactions are rapid, canalization is easy, and there is also great facility for association. Instead of the process being laborious and slow, it is rather unduly quick; and conscious effort is necessitated, as a rule, by the relatively vast extent of mental work attempted. Or again a feeling of strain may result in one who, accustomed to do much with ease, finds himself temporarily unable to accomplish even his usual amount of work.

In such a brain there is a relatively large proportion of cellular elements in an unstable condition (according to Hughlings Jackson's dictum that the latest acquirements in the course of evolution are the most complex and least organized). Further, as any organization is likely to respond to stimuli and endeavour to fulfil its function, the strain upon the more complicated mechanism will be greater, and the tendency to breakdown will, therefore, be more evident. This tendency which we should on *a priori* grounds expect, as Huxley points out, to be present, is undoubtedly demonstrated in the life-histories of those of pre-eminent intellectual capacity. It does not follow that in their case, any more than in that of ordinary people, the breakdown is a permanent one. The stoppage of mental work, which inevitably results in so many instances, may be part of Nature's scheme to give rest to overworked tissues, just as sleep has been arranged in order to "knit up the ravell'd sleeve of care" and to act as a "balm of hurt minds." Thus we may find that there are phases of intense mental energy, comparable to the waking stage, which are followed by periods of sluggish brain action when no productive work can be done. This is strikingly exemplified in the earlier part of the life of Nietzsche before a complete failure of his mental energies took place. Until we know more of the etiology of the manic-depressive conditions it is not possible to speak dogmatically about the matter, but it certainly seems as if the normal flux and reflux of energy are merely accentuated in these cases. It is obvious that, taking into account the variations of the phases in one individual as compared with those in another, there are degrees of intensity such as would allow of cases being graded insensibly from ordinary fluctuations of cheerfulness and depression onwards to those instances in which acute mania and acute melancholia alternate. In the milder cases there seems to be no reason why the onset of the excitement—or even the whole of that phase—should not be accompanied by an increase in productiveness, especially in so far as work

¹ See Nisbet, "The Insanity of Genius," 2nd ed., 1893, p. xxi. (Cf. also "The Life and Letters of T. H. Huxley," i, p. 346.)

² Hugh Elliot, "Human Character" (Lond., 1922), p. 196.

of an imaginative character is concerned. What seems to be necessary is a decrease of inhibition or an increase of stimulation—or both at the same time. The great importance of the products of the ductless glands must be recognized in speeding up the action of the nervous system.¹ Then there are substances like alcohol, opium, hashish, and heroin. It seems to be indubitable that such writers as Coleridge, De Quincey, Edgar Allan Poe, and Verlaine, to mention no others, were considerably influenced by drugs: though it should always be remembered that the drug addiction may be secondary to the nervous instability. There are, too, other toxic factors. The fluctuations of temperature in tuberculosis, pointing as they do to metabolic changes and to morbid stimulation have their influence, and they cannot be left out of the account, in considering the lives of, for example, Keats, R. L. Stevenson, and Aubrey Beardsley. The flight of ideas in mania is obviously due to such a degree of lack of inhibition that associations take place so rapidly as to be apparently incoherent. It is not difficult, however, to trace the close relationship between this condition and that which exists in the mind of the writer or of the artist at certain periods. In both we find the fantastic elements which are symptomatic of the "fine frenzy." Careful editing has deprived the world of much valuable evidence in this respect: and though this may be no great loss from the literary or the artistic point of view it is decidedly so in so far as psychiatry is concerned. When dealing with a record of abnormal mental phases the biographer is only too apt to suffer from *suppressio veri*. If accurate records could be obtained it would be interesting to ascertain the exact association of the various phases of mental disorder with intellectual work. If Napoleon, Julius Cæsar, and Mahomet really suffered, as has been stated, from epilepsy, their superabundant energy may sometimes have shown itself in the form of fits, while at others it would have an outlet in increased cerebration; or there may have been a number of attacks of masked epilepsy which would not have been recorded as epileptic seizures. Paranoid and delusional insanity may generally co-exist with a high degree of intellect, as the lives of Swedenborg and of Rousseau demonstrate. Sexual abnormalities appear to be a not unusual concomitant of superior intellectual capacity. It seems clear, however, that the more pronounced the insanity the less is the capability for producing what may be really regarded as the work of a genius-mind. But here the difficulty arises of deciding what is meant by a product of genius. What is described by devotees as such is looked upon by less prejudiced observers as fantastic, eccentric, or morbidly abnormal. It is difficult to arrive at a decision in general principles. Each case must be considered on its merits; for what may be the product of insanity in one instance may be in others imitation by the "sedulous apes" who, noticing that the public like that kind of thing, see that they get it. Even where we are dealing with true genius there are apt to be, as might be expected, great inequalities in the value of their work. To say that a person may be the "greatest, wisest," and at the same time, the "meanest of mankind" is perhaps rather an example of poetic license: but it conveys tersely the condition of unequal cerebral development so characteristic of genius. It helps to elucidate also the conjunction of credulousness in one direction with scepticism in another, of charity with bigotry, and of general benevolence with private cruelty. "*Vir teres atque rotundus*," mentally and physically, is a pious aspiration rather than an actuality.

¹ The well-known works of Cannon and of Crile may be mentioned. The subject is dealt with at length in a most interesting way by Dr. Louis Berman in "*The Glands regulating Personality*" (New York, 1921).

Genius, then, is a product of a brain in a state of unstable equilibrium. The possessor being more or less of a "sport," is not, therefore, one of the "fittest," that is, he is not one of the best adapted to environment. In many cases this results in his being eliminated by the more stable members of the herd, in the same way as the weakly children were exposed to death by the Spartans. Or, as he lives for the most part in a vicious circle because of the tendency to overwork or to react unduly to stimuli, the threshold of sensibility being still further lowered in the process, the chances of a temporary or a permanent breakdown are much greater than in the less impressionable, less reactive individual.

Genius and insanity are both results of nervous instability. Insanity does not cause genius but for the most part is inimical to intellectual effort: though, because cerebral deterioration is often a slow and insidious process, the mental defect may not be obvious for a considerable time after the onset of the condition which will eventually lead to definite insanity. It may be said with more truth that genius is much more likely to lead to insanity: insanity being the price which nature exacts in this instance for valuable but delicately-constructed gifts. The genius, because of his organization, is an intractable person who is apt to place an undue strain upon his resources and who has to pay the penalty for doing so. When it is realized that his mental work—like that of ordinary people—is dependent upon the functioning of physical structures, more care may be taken to conserve the nervous mechanism. But that is a counsel of perfection which is disregarded by the genius, and is little likely to be taken by the average man!

DISCUSSION.

Dr. STODDART doubted the accuracy of Dr. Norman's premises. He had had the good fortune of personally knowing two geniuses, viz.: Dr. Hughlings Jackson and Professor Freud, and he had never met two saner men. As to the acknowledged geniuses of all ages, Hippocrates, Galen, Aristotle, Copernicus, Galileo, Shakespeare, Goethe, Hegel, Newton, Darwin and Dickens for example, he (Dr. Stoddart) was not aware that there was any evidence that these great men had been insane, although admittedly some had had their peculiarities. Dr. Stoddart also combated Dr. Norman's materialistic conception of mental phenomena. Nobody had ever doubted the intimate connexion between mind and brain, but modern psychological investigation of the neuroses and psychoses had afforded a satisfactory explanation of them, such as had never before been achieved.

Dr. W. WALLACE said that he had been interested in this subject for many years. Dr. Norman had mentioned Nisbet's "Insanity of Genius," but Dr. Wallace would like to know if Dr. Norman had read it from cover to cover. It was a book that had done much harm, and its conclusions and vast assumptions were ludicrous and mischievous. So glaring was Nisbet's complete lack of knowledge that one wondered why he had not made out a case of insanity from housemaid's knee. Milton was said to have lost his sight from congenital causes; a new view to take of glaucoma.¹ A great deal had been made of Beethoven's so-called eccentricities, but so fully conscious was the composer that his conduct, due to his deafness, might be misunderstood, that he pledged his doctor to publish a report on his "case" when he was dead. He did not die insane, although the post-mortem appearances would have justified this conclusion, in the absence of any clinical history or knowledge of the man. The speaker showed a photograph of Beethoven's skull, with great thickening of the right parietal bone. The

¹ See W. Wallace, "The Musical Faculty," Lond., 1914, in which Nisbet's views are discussed at length.

post-mortem report stated that the auditory nerves, especially that on the right side, were atrophied, there was ulceration of the larynx and the nodulated liver was shrunk to half the normal size. Lauder Brunton gave the opinion that these lesions were syphilitic in origin.¹

Dr. Wallace said that throughout his life he had associated intimately with creative artists, sculptors, painters and musical composers, not all geniuses perhaps, but men who had secured national, and in some cases world-wide recognition, but he had not met with a case of insanity among them, either from his own observation or by hearsay.

¹ This view is not accepted by Dr. Georges Canuyt. See "La Surdit  de Beethoven" in *Annales des Maladies de l'Oreille* for January, 1923. Apparently the writer had not seen the photograph of the skull or the post-mortem report.

Section of Psychiatry.

President—Dr. C. HUBERT BOND, C.B.E.

Pathological Laughing and Crying.¹

By S. A. KINNIER WILSON, M.D.

THE problem presented by certain cases of abnormal emotional expression, in the guise either of exaggerated or uncontrollable laughing or crying, or conversely of paralysis (at least in part) of the same mechanism, has not attracted much attention in recent years, nor has much advance been made on the views advocated by Nothnagel and by Brissaud, some thirty years ago or more. The time has come when the whole question is ripe for reconsideration.

Reference is here made solely to cases of organic nervous disease in which, as a sequel to, and consequence of, a recognizable cerebral lesion or lesions, "attacks" of involuntary, irresistible laughing or crying, or both, come into the foreground of the clinical picture. Cases are also examined in which there is conservation of voluntary facial movement with paresis or paralysis of the same musculature for the involuntary movements of facial expression.

Among the organic affections apt to be associated with these phenomena are double hemiplegia, pseudobulbar paralysis, disseminated sclerosis, &c.; the exact nature of the morbid affection is of less importance than its site. They may occasionally occur in unilateral cases.

A considerable number of personal cases are analysed to show the nature and the varieties of the syndromes under investigation. Attention may be directed to some particularly valuable observations dating back nearly a century, recorded by Sir Charles Bell and by Stromeyer. The association of involuntary respiration with involuntary facial movement is such that both of these may be unilaterally paralysed if the lesion is appropriately situated.

The natural question that arises is whether the emotional outbursts correspond to or reflect the mental state of the individual concerned at the moment of their expression. It can be shown that in not a few instances they do not. Patients may be forced to express a particular emotion in opposition to their real feelings. The bearing of the facts on the James-Lange hypothesis is obvious. It is clear that the bodily reverberation, as James calls it, is not *per se* the emotion, and the theory must be materially modified to bring it into line with the observations reported.

The mechanism of the emotional expression is one in which, on the somatic side, facial and respiratory musculatures are implicated.

¹ The paper will appear in full in a forthcoming number of the *Journal of Neurology and Psychopathology*.

Clinical study reveals the existence of three types of interrelated motor disorder in this connexion: (1) The face may be unilaterally paralysed for voluntary movement but not for emotional expression; (2) it may be bilaterally thus paralysed; (3) voluntary control may be perfect, while emotional facial expression is unilaterally (or it may be bilaterally) affected. Consideration has led to the development of the idea that there are separate and distinct paths for emotional and for volitional facial movement. Examination of the hypotheses of Nothnagel and of Brissaud shows that they do not entirely accord with all the known facts.

There is a voluntary control over the facio-respiratory mechanism, and there is an involuntary control. The voluntary control is exercised by way of the cortico-ponto-medullary path; for the involuntary control we have two distinct paths, the existence of which may be demonstrated by physiological experiment—a non-pyramidal, arresting path and an accelerating path. These have been traced from the cortex, through the *regio subthalamica*, to the appropriate nuclei. Involuntary laughing and crying are allowed by lesions of the voluntary path; emotional paralysis is produced by lesions of the other. The two clinical conditions may occur in association. The thalamus cannot be more than a link in the chain, of which the cortex must form a part.

PROCEEDINGS
OF THE
ROYAL SOCIETY OF MEDICINE

EDITED BY
SIR JOHN Y. W. MACALISTER
UNDER THE DIRECTION OF
THE EDITORIAL COMMITTEE

VOLUME THE SIXTEENTH

SESSION 1922-23

SECTION OF SURGERY

[Including the *Proceedings* of the SUB-SECTION OF PROCTOLOGY]



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1923

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SECTION OF SURGERY.

CONTENTS.

November 1, 1922.

JAMES BERRY, F.R.C.S. (President).	PAGE
The Progress of Surgery and the Rise and Fall of Surgical Operations ...	1

January 3, 1923.

W. SAMPSON HANDLEY, M.S., F.R.C.S.	
On Subcapsular Pyelotomy, with Remarks on the Origin and Treatment of Renal Calculi	2

February 7, 1923.

G. GREY TURNER, M.S. (Newcastle-upon-Tyne).	
A Case in which an Adenoma weighing 2 lb. 3 oz. was successfully removed from the Liver: with Remarks on the Subject of Partial Hepatectomy	43
GARNETT WRIGHT, F.R.C.S. (Manchester).	
Primary Carcinoma of the Liver excised by Operation	56
CLAUDE FRANKAU, C.B.E., D.S.O., F.R.C.S.	
A Case of Resection of the Liver for Malignant Disease spreading from the Gall-bladder	59
PHILIP TURNER, M.S., F.R.C.S.	
Case of Excision of an Adenoma of the Liver which had ruptured spontaneously, causing Internal Hæmorrhage	60
FRANK KIDD, M.Ch.Cantab., F.R.C.S.Eng.	
Case of Primary Tumour of the Liver removed by Operation	61
Discussion (pp. 62-64): Mr. A. J. WALTON, Mr. CYRIL NITCH.	

May 2, 1923.

J. P. LOCKHART-MUMMERY, F.R.C.S.	
The Technique of Resection and Anastomosis of the Colon for Tumour ...	69
CHARLES A. PANNETT, F.R.C.S.	
The Technique of Axial Anastomosis of the Alimentary Canal ...	81

March 7, 1923.

SIR G. LENTHAL CHEATLE, K.C.B., F.R.C.S.	PAGE
The Sites of Origin and Methods of Growth of Fibro-adenomata of the the Breast	85
ERNEST H. SHAW, M.R.C.P.	
Demonstration on the Immediate Microscopic Diagnosis of Tumours at the Time of Operation	85
CECIL ROWNTREE, F.R.C.S.	
Two Cases of Sarcoma of the Small Intestine	85
FRANK KIDD, M.Ch., F.R.C.S.	
Case of Large Spindle-celled Sarcoma arising in the Mesentery of a Coil of Ileum successfully removed at Operation	86

June 6, 1923.

R. H. ANGLIN WHITELOCKE, M.D., F.R.C.S.	
The Treatment of Fractures of the Patella	111
H. P. WINSBURY WHITE, F.R.C.S. Eng.	
The Closure of the Suprapubic Urinary Fistula following Suprapubic Prostatectomy; Observations on Sixty-eight Cases	119

SUB-SECTION OF PROCTOLOGY.**November 8, 1922.**

SIR CHARTERS SYMONDS, K.B.E., C.B., M.S. (President).	
Gonorrhœal Stricture of the Rectum	13

February 14, 1923.

J. P. LOCKHART-MUMMERY, F.R.C.S.	
A New Method of treating Ischio-rectal and other Abscesses	65
LIONEL E. C. NORBURY, F.R.C.S.	
Case which was clinically one of Inoperable Carcinoma of the Rectum treated by Colostomy and Subsequent Injections of Cuprase-Colloidal Selenium and Colloidal Cuprum for over Two Years, with Disappear- ance of the Growth	67
Shown by PERCIVAL COLE, F.R.C.S.	
A Specimen of Colon, showing Multiple Perforations resulting from Dysentery	67

April 11, 1923.

HERBERT H. BROWN, O.B.E., M.D.	
Patient upon whom an Operation was performed in June, 1920, for Cancer of the Rectum, by the Abdomino-anal Method	89
J. P. LOCKHART-MUMMERY, F.R.C.S.	
Case of Early Tabes Dorsalis	90

Contents

v

May 9, 1923.

DISCUSSION ON ULCERATIVE COLITIS.

Sir HUMPHRY ROLLESTON, K.C.B. (p. 91), Sir THOMAS HORDER (p. 96), Mr. J. P. LOCKHART-MUMMERY (p. 97), Dr. W. E. CARNEGIE DICKSON (p. 100), Professor LEONARD S. DUDGEON (p. 104), Dr. A. F. HURST (p. 106), Sir CHARLES GORDON-WATSON (p. 109), Mr. DOUGLAS DREW (p. 109).

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OXFORD HOUSE,
83-91, GREAT TITCHFIELD STREET, OXFORD STREET, W. 1.

Section of Surgery.

President—Mr. JAMES BERRY, F.R.C.S.

The Progress of Surgery and the Rise and Fall of Surgical Operations.

PRESIDENT'S ADDRESS.

By JAMES BERRY, F.R.C.S.

MY first duty, and a very pleasant one, is to thank the Section for the honour it has conferred upon me by appointing me your President for the ensuing year. It is an honour which I appreciate very highly, and although I think you might easily have found a better president, I will only say that I will do my best to justify the confidence which you have so kindly placed in me. May the ensuing season be productive of much good work, may our discussions continue to be carried on with fearless outspokenness, with good temper and with liveliness, as they have been in the past.

My second duty is a more onerous one, and the responsibility of it weighs heavily upon me. It is to inflict upon you a Presidential Address. Any form of public address is always a sore trial to me, but I am encouraged by the thought that I have before me a kindly and sympathetic audience.

During my surgical life it has fallen to my lot to witness the most wonderful progress that I suppose surgery has ever made in so short a time.

When a student I just saw the termination of the old pre-antiseptic days. I was able to watch the gradual development, first, of antiseptic surgery in all its varying phases, of operating in a fog of carbolic spray, of treating clean operation wounds with poisonous and irritant lotions—the washing out of the peritoneal cavity with 1 in 20 carbolic acid lotion and with solutions of perchloride of mercury and other abominations so distasteful and so harmful to the living peritoneum.

Tuberculous joints were for many years treated by most surgeons by extensive and mutilating excisions, performed, even in early cases, with the usually mistaken idea that all the tubercle could thereby be eradicated. Indeed so far was this practice carried that on one occasion I saw a perfectly healthy head of the femur excised from an unfortunate child who was supposed to have early tuberculous disease of the hip.

Then came the gradual development of aseptic surgery under which so many of the greatest advances of modern operative surgery have been made. Even aseptic surgery, as opposed to antiseptic, has at times been carried too far, as was found during the early stages of the war when some of the younger

surgeons, who had been brought up only in the strict aseptic school, endeavoured to apply asepsis (I use the term in its narrow sense) to the treatment of foul and gangrenous wounds which were more suitable for treatment by antiseptics.

In the eighties the operative surgery of the skull and brain was little more than the trephining or elevation of compound depressed fractures and the occasional, and usually fatal, opening of a cerebral abscess.

Deep dissecting operations at the root of the neck were comparatively rarely performed, on account of the extreme danger of opening up the cellular tissue of the neck in days when *antiseptics* were in their infancy, and *asepsis* in its modern form unknown. Thoracic surgery was mainly the opening of empyemata, with an occasional adventure into a suppurating pericardium and now and then some kind of operation upon the chest wall for old empyema. Abdominal surgery was in its infancy. The fear—and the fully justifiable fear—that most surgeons had of opening the peritoneal cavity, seems strange to us, who now, thanks to improvements in surgical technique, think nothing of so doing. I have seen a man die of acute peritonitis after a trivial operation upon the abdominal wall which involved a small opening of the peritoneal cavity, by one of the most noted surgeons of the day. I remember witnessing what must have been one of the very first operations for the removal of a renal calculus, an operation which we have all been doing for many years with ease and safety, and in most cases with complete success.

Hysterectomy in those early days was being done with a wire *écraseur* and extraperitoneal treatment of the stump. Cancer of the tongue was usually removed by means of the barbarous chain or whipcord *écraseur*. A curious instance of the modern revival, in an improved form, of an ancient operation, is the quite recent, and now somewhat fashionable, operation of removal of the tongue by diathermy. One of the first operations I ever saw for removal of the tongue for cancer (I think it was the very first) was done with a galvanic cautery by the late Mr. Callender. It is well to remember that the reasons which led then to the rejection of the cautery—namely, the danger of sepsis and secondary hæmorrhage, still exist. If I may be permitted to express an individual opinion it is that a properly executed operation for removal of the tongue from the outside, the method that I nearly always myself perform for bad cases of cancer, gives better results than diathermy, although I know that many excellent and experienced surgeons hold an entirely opposite view.

Perineal lithotomy was a common and favourite operation in my early days of surgery, although it quickly went out of fashion, being superseded by the much more easy, and, on the whole, more satisfactory, suprapubic operation. The old-fashioned lumbar colotomy was being replaced by the more satisfactory inguinal operation. When this later operation first came in accidents and complications connected with it were not uncommon. Such were prolapse of the bowel, from not taking sufficient care to draw the sigmoid well down before fixing it, dropping back of the sigmoid into the peritoneal cavity because the mesentery had not been firmly fixed to the abdominal wall, the escape of a few feet of small intestine into the bed a few days afterwards because the wound in the abdominal wall had been made too long or incompletely closed. All these accidents I have seen more than once and they still sometimes occur in cases in which colotomy is done for obstruction with distension.

At the present time colotomy for intestinal obstruction is a little out of favour and there is a tendency to replace it by cæcotomy. It is perhaps not sufficiently recognized that the opening of the cæcum under conditions of great

distension is by no means always a simple or easy operation and many a cæcum has been burst in the process. It is easy enough when the abdomen and bowel are not distended.

It is a truism to say that most of the progress that has been made in surgery is due to the application first of antiseptic and later of aseptic principles to the treatment of operation wounds. I well remember the old days when I used to see surgeons of eminence operating in old frock coats, the discards of the consulting room, splashed and stained with blood and pus, the result of many previous operations. The more dirty the coat, the greater seemed to be the pride of the surgeon who wore it. He did not always even trouble to wash his hands before undertaking an operation. To those accustomed only to the white gowns, masks, rubber gloves and all the necessary paraphernalia of the modern operating theatre, such a condition of affairs must seem almost incredible. I have seen surgeons in the olden days undertake major operations with scarcely a pretence of cleanliness. I have seen a hospital surgeon stop in the middle of an operation to scratch his head and think, put his hand in his pocket, draw out and put on his spectacles, and then at once proceed with his operation. I have seen another bite a ligature with his teeth when the scissors were not handy.

Many of the highly technical operations of the present day depend for their success not merely on skilful technique and strict asepsis. These can be learnt more or less thoroughly from books. But many of them demand also a thorough knowledge of the pathological conditions likely to be met with in the actual performance of them. This knowledge is more difficult to acquire and the lack of it may easily lead by an error of judgment to a fatal disaster. Take the case of cholecystectomy, a very successful and satisfactory operation in the hands of skilled surgeons. But in my opinion it often requires much judgment to know *when* it should be performed and when it is better to substitute for it the simpler and easier operation of cholecystotomy.

"I can teach my assistants *how* to operate in six months, but to teach them *when* to operate takes twenty or thirty years"—is a recent saying attributed to an eminent French surgeon.

We are all of us, I think, a little too apt to be impressed by the brilliant results that may have been obtained by some star performer after years of study and experience. A cholecystectomy that in a particular case may be quite the right operation for a Moynihan or a Mayo may be quite the wrong operation for Mr. A. or Mr. B. to perform, as he may find to his cost and to that of his patient. An extensive operation for a tumour of the brain may be a brilliant success in the hands of a Harvey Cushing or a Sargent, but a terrible disaster if undertaken by anyone who has not had special training in this line of practice. A slight error in the placing of the suture in a gastro-enterostomy, the careless application of a single ligature in a thyroidectomy, the placing of a clamp in the wrong place in a cholecystectomy, may each of them easily lead to the death of the patient and have often done so.

We sometimes hear of the mortality of such and such an operation. There is no such thing. The mortality depends upon the condition for which it is done, upon who does it, and how and when it is done. I am reminded of the old story, doubtless an invention, of the man who was about to undertake a very serious and dangerous operation and he assured the patient that it would be entirely successful. When pressed for the reason of his great confidence, he said, "Well, the mortality of this operation is 95 per cent. I have already done it nineteen times and all the patients have died. This is the twentieth and he is bound to recover."

In these days of greatly increased knowledge in diagnosis, in pathology and technique, it has become possible, with the help of asepsis, to penetrate with impunity to regions of the body which formerly no surgeon would have dared to touch. Extensive operations upon the bladder, the intestine, the stomach, the biliary passages, the lungs, the thyroid and the brain, to mention only a few, are among the wonders of modern surgery. It is quite unnecessary, however, for me to embark on any detailed description or eulogy of these triumphs of surgery, which are sufficiently well known to all of us.

At one period of my surgical career (1892-98) I had the good fortune to be surgical registrar to one of the largest London hospitals for more than five years. During that period it was one of my numerous duties and privileges to make all the surgical post-mortem examinations, about one thousand in number. During this same period, I was also full surgeon to another large metropolitan hospital with a medical school.

The experience gained in the one department was invaluable to me in the practice of the other, and especially in the complicated and difficult field of abdominal surgery. A surgical registrar doing a post-mortem has the opportunity of imagining that he is operating on the living, and of learning what are the conditions and the difficulties that he may expect to find when he comes to operate on his own patients. I venture to think that a young surgeon who aspires to make a name for himself in surgery can have no better training than is given to him by holding the post of surgical registrar, if that post involves, as it should do, the clinical examination of the patients in the wards, the witnessing of all the important operations upon them and the performance of post-mortem examinations upon those that die.

I cannot help feeling that the modern practice of entrusting surgical post-mortems to the non-practising pathologist, which is becoming the fashion now, is in some respects a retrograde step. As a rule, the pathologist is much more interested in medical than in surgical matters. He is almost necessarily not interested in, or does not know, those little matters of surgical detail so important to the operating surgeon, and consequently fails to record them in his notes.

The loss to the young surgeon of this valuable post-mortem training is surely obvious. Every young aspirant to a hospital staff who can do so, after a year or two in the anatomy department, should soak for several years in pathological museums, in operating theatres and in post-mortem rooms. He can scarcely have too much of any of the three. He should devote all his energies to learning his business and should neglect no opportunity of acquiring knowledge that will be useful to him in after life. He must learn his anatomy and surgical pathology thoroughly, he must watch the performance of innumerable operations and see hundreds and thousands of clinical cases before he is really fit to practise first-class general surgery, and to avoid making mistakes when he comes across some condition which is a little out of the common. Even then he will inevitably make mistakes. We all do. But he will probably make fewer.

He must expect to have a hard time in his earlier days, and he will probably find it difficult to make a living, unless he has private means, which few young surgeons have. But he can earn enough to live upon by teaching, by giving anæsthetics and in various other ways. He ought not to expect to do much surgical practice of a remunerative kind until he has had ten or twenty years of steady hard work at learning the elements of his profession. I know well that it is a dog's life, but for all that he is a happy dog who leads it!

Brilliant as are the results which have been attained in all operations in which strict asepsis is possible and essential, I am not quite so sure that we have any right to congratulate ourselves so much when we come to a totally different group of operations. I mean those operations which are undertaken where sepsis is already present. I refer more especially to acute suppurative conditions of the abdomen. Not only is complete asepsis, from the very nature of the case, impossible, but it is far more difficult for the young operator to acquire the necessary experience for the correct treatment of these often most difficult cases. It is easy enough by visiting great surgical centres, as most of us do from time to time, to learn from the great masters of surgery, who will often arrange their operations at a time which will suit our convenience. But a surgeon cannot arrange for us an afternoon in which he will operate upon, say, a fulminating appendicitis, a perforated gastric ulcer, a herniotomy for gangrenous intestine, and an acute intestinal obstruction. Experience of such operations is of the greatest importance to the practising surgeon. But these are not operations that can be arranged in advance and demonstrated before an admiring crowd of practitioners, or at a surgical congress. Many of them, perhaps most of them, we perform at night, or at odd times, rarely on regular operating days. Only long and assiduous attendance at hospitals can enable young men to see much of them and thus to gain the experience that is so needful. It is not so much mere technique that he requires for the successful performance of such operations, but rather experience, the knowledge of what to do and what not to do.

An operation which I think is performed too frequently is that for appendicitis. It is of course quite true that the great majority of operations upon the quiet appendix are easy of performance and almost devoid of danger. But there is a minority in which the operation may be a very severe and difficult one, and the difficulties both in diagnosis and in performance are not easily foretold by the operator. When it is done for an acute condition, the risks are naturally much greater, but the necessity of the operation may fully justify the running of these risks. Where I venture to think most harm is done is when the case is not seen in the very early stage of the disease, and especially when the stage of abdominal distension has already been reached. No one would wish to say that operation is never to be done in that stage. But the risks then are undoubtedly much greater, and I sometimes wonder whether it is realized sufficiently that, after all, *most* of such cases do *not* die if not operated upon, and that the delay of a few days often puts the patient in a much more favourable condition for operation if it has to be undertaken. The matter is of course an extremely difficult and complex one, and I have no wish to be dogmatic about it. I know of no rule as to when an operation is to be undertaken for acute appendicitis, except that each case should be decided upon its own merits, according to the judgment and experience of the individual operator. Nor do I wish to enter now upon the very difficult subject of how to deal with these late and most dangerous cases of suppurative peritonitis. There can be little doubt that in the past there was a tendency to do too much. In the nineties for instance, when surgeons were treating such cases by free opening of the abdomen and washing out the whole peritoneal cavity, the mortality was tremendous, as any one can see if he will take the trouble to read the hospital reports of those days. Those were the days when I, as surgical registrar, used to make many post-mortems upon such cases, and, so far as I can judge from hospital records, my experience was much the same as that of registrars at other hospitals.

Two operations for acute perforative peritonitis (not appendix cases) that I witnessed about the year 1896 made a great impression upon me. Both were in young women; both patients had perforated gastric ulcer; both cases were similar in their origin and nature. One was operated upon within four hours, and was treated by extensive washing out of the whole peritoneal cavity, gallons of warm water being used for the purpose, until the peritoneum appeared to be perfectly clean everywhere. The patient nevertheless died in three days. At the post-mortem, which I performed, the damaged peritoneum showed little or no evidence of recuperative repair.

The other case was apparently a very much worse one. It was not seen and operated upon until twenty-four hours had elapsed. The abdomen was greatly distended, and the girl was extremely ill. I well remember the surgeon saying, as he discovered the perforation and found extensive extravasation: "It is no use my trying to wash out this peritoneum — she will die if I do." So he merely gently mopped out fluid from the immediate neighbourhood of the ulcer and closed the wound, inserting a drain. This patient made a good recovery.

Since that time I have naturally seen numerous similar cases of both kinds. The lesson that we have most of us now learnt is that it is generally useless to attempt thorough cleansing of the peritoneal cavity, that the peritoneum itself has enormous powers of repair, and that it is better in such cases to interfere with it as little as possible, either closing the abdomen completely, or at most putting in a drainage pipe.

In an address which I gave fifteen years ago,¹ after much study and experience of the subject, I expressed the view that, "if we were *all* to go back to the old treatment of appendicitis that was in vogue twenty-five [now forty] years ago, there would be a lesser mortality from appendicitis than there is at present." By *all* I mean not merely all hospital surgeons, but all who operate for appendicitis. Fifteen years' further experience of the subject has confirmed me in the opinion I then expressed. No one would wish to suggest for a moment that experienced surgeons are not to operate for acute appendicitis, or even that there are not many cases in which an immediate operation is imperative. But I do think that much more care should be taken than is usual, in the selection of cases for operation, both as regards time and manner. One of my earliest lessons in surgery, one which I have never forgotten, and which has been of great use to me in practice, was taught me by my friend and former teacher, the present President of the Royal College of Surgeons. Referring especially to the operative treatment of acute inflammatory conditions, he used to say: "Think, before you decide to operate, what is likely to be the natural course of the disease if it is *not* operated upon, and what possible harm your operation itself may do." I think that many of the operations for inflammatory conditions of the abdominal viscera and particularly of the appendix, are too often undertaken without due consideration of this important point, and especially when the operator is a man of small experience.

It is commonly believed and stated both by the public and by the profession that the modern custom of operating freely for acute appendicitis has resulted in great saving of life. But is this really the case? If it were, surely deaths from appendicitis would be less common now than they used to be; unless we may assume, as some boldly do, that appendicitis is a disease that

¹ *Lancet*, September 7, 1907, p. 680.

is becoming increasingly frequent. I see no reason for thinking that this really is the case, although undoubtedly it is far more frequently diagnosed, and has attained much greater prominence in the eyes both of the public and of the profession.

As food for serious reflection, and I do not claim anything more for them, I would draw your attention to the figures in these tables which I have compiled from the Registrar-General's returns. Statistics may be very misleading and want very careful examination before they can be accepted as proving anything. But these figures, if they mean anything, tend at least to show that the mortality from appendicitis has not diminished since the custom of operating for acute appendicitis has been so widely prevalent. Greater accuracy in diagnosis and in registration will account for some of, but not, I think, for all, the increased mortality.

Table I shows the total deaths from appendicitis in quinquennial periods since 1901. How many of these deaths followed operation we cannot say, and it is of course open to anyone to say that most of the deaths occurred because the cases were not operated on.

Table II shows the corresponding figures for peritonitis, and is a good illustration of one danger of the use of statistics. Taken by itself it might be interpreted to mean that deaths from peritonitis had diminished greatly in the last twenty years. The true explanation is doubtless that, owing to increased accuracy of diagnosis, many deaths that would have been registered in Table II now come into Tables I, III and IV. I give you, therefore, Table V, as well, which shows for the same periods all the deaths from all diseases of the digestive system arranged according to the Registrar-General's classification. Some curious points appear among these figures. Why, for instance, deaths from gastric ulcers in males should have risen apparently from 2,683 to 5,195, while those in females have diminished from 5,703 to 4,455 is not clear to me.

TABLE I.—TOTAL DEATHS FROM APPENDICITIS AND PERITYPHLITIS.
England and Wales (compiled from Registrar-General's Reports).

Period	Male	Female	Total
1901-05	4,873	3,418	8,291
1906-10	6,299	4,587	10,886
1911-15	7,260	5,480	12,740
1916-20	6,827	5,591	12,418

TABLE II.—TOTAL DEATHS FROM PERITONITIS (NON-PUERPERAL).
England and Wales (compiled from Registrar-General's Reports).

Period	Male	Female	Total
1901-05	2,500	2,960	5,460
1906-10	1,549	1,875	3,424
1911-15	1,113	1,627	2,740
1916-20	885	1,270	2,155

TABLE III.—TOTAL DEATHS FROM INTESTINAL OBSTRUCTION.
England and Wales (compiled from Registrar-General's Reports).

Period	Male	Female	Total
1901-05	6,332	6,427	12,759
1906-10	6,511	6,469	12,980
1911-15	6,390	6,302	12,692
1916-20	6,565	6,412	12,977

TABLE IV.—TOTAL DEATHS FROM GASTRIC ULCER.
England and Wales (compiled from Registrar-General's Reports).

Period	Male	Female	Total
1901-05	2,683	5,703	8,386
1906-10	3,457	5,329	8,786
1911-15	4,532	4,842	9,374
1916-20	5,195	4,455	9,650

TABLE V.—TOTAL DEATHS.
England and Wales (compiled from Registrar-General's Reports).

Diseases of the digestive system	1901-05			1906-10			1911-15			1916-20		
	Male	Female	Total	Male	Female	Total	Male	Female	Total	Male	Female	Total
Tonsillitis, quinsy	1,074	965	2,039	1,008	824	1,832	1,233	1,106	2,339	1,154	1,088	2,242
Diseases of mouth, pharynx, oesophagus (not spec.)	1,453	1,314	2,767	1,368	1,399	2,767	1,577	1,448	3,025	1,446	1,263	2,709
Gastric ulcer	2,683	5,703	8,386	3,457	5,329	8,786	4,532	4,842	9,374	5,195	4,455	9,650
Gastritis, gastric catarrh	11,287	12,093	23,380	7,591	8,487	16,078	7,826	8,084	15,910	6,830	6,983	13,813
Other diseases of stomach (not malignant)	—	—	—	1,422	1,283	2,705	1,722	1,575	3,297	1,344	1,282	2,626
Ulceration of intestine	12,721	11,274	23,995	2,320	1,394	3,723	3,316	1,489	4,805	3,052	1,394	4,446
Enteritis (not epidemic)	—	—	—	10,837	9,701	20,538	10,825	9,423	20,248	6,799	5,884	12,683
Gastro-enteritis	8,052	6,998	15,050	8,558	7,276	15,834	12,919	11,003	23,922	9,133	7,628	16,761
Appendicitis, perityphlitis	4,873	3,418	8,291	6,299	4,587	10,886	7,260	5,480	12,740	6,827	5,591	12,418
Hernia	3,339	3,995	7,334	3,429	4,049	7,478	3,832	4,463	8,295	4,336	5,296	9,632
Intestinal obstruction	6,332	6,427	12,759	6,511	6,469	12,980	6,330	6,302	12,632	6,565	6,412	12,977
Other diseases of intestines	688	603	1,291	734	693	1,427	1,213	1,264	2,477	1,194	1,219	2,413
Peritonitis (not puerperal)	2,500	2,960	5,460	1,549	1,875	3,424	1,113	1,627	2,740	885	1,270	2,155
Cirrhosis of liver	11,240	9,029	20,269	10,122	8,035	18,157	9,808	7,392	17,200	6,054	3,341	9,395
Other diseases of liver and gall-bladder	5,201	6,654	11,855	4,040	5,884	9,924	3,568	2,992	6,560	3,142	5,072	8,214
Other diseases of digestive system	1,238	1,216	2,454	882	913	1,795	641	554	1,195	493	472	967
	115,330			138,334			146,759			123,101		

One frequently hears it said that the war has produced great advances in surgery. No doubt in many directions it has done so. One has only to point to the great improvements in the treatment of septic wounds, of compound fractures, and to the brilliant results obtained in the plastic surgery of the face and other parts. No one can see without admiration the wonderful work that has been done on bones, joints, muscles and nerves at the many military orthopædic hospitals. But in another respect the war has had a harmful effect.

All over the country at the present time large and dangerous operations are being undertaken by those whose qualifications for undertaking them consist chiefly in a few years of practice in military surgery, the surgery of injuries, of injuries inflicted for the most part upon young and previously healthy men.

No doubt the surgery of gunshot wounds, of compound fractures, and the subsequent deformities resulting from them form an important branch of surgery. But this kind of surgery forms but a very small portion of civilian general practice. Great experience of military surgery does not in itself qualify an operator to deal satisfactorily as regards either diagnosis or treatment with such important subjects as diseases of the breast, cancer of the tongue, tumours of all kinds, diseases of the biliary passages, of the stomach, colon, prostate, &c. And yet how often do we hear of practitioners with little more than a few years of military experience behind them, gaily and almost light-heartedly undertaking the gravest operations for conditions such as the above, with little knowledge of the dangers to which they are subjecting their patients. If the case happens to be a straightforward one that can be dealt with by a text-book operation all may go well. But only too often some unexpected difficulty or complication arises for which the operator is wholly unprepared, and the result may easily be disastrous for the patient. Almost every large town in England at the present day has one, or more than one, skilled surgeon who is devoting himself, after long training and experience, to the exclusive practice of surgery. It is therefore, I think, unwise for those who have not had this special training and experience to undertake extensive and, if improperly performed, dangerous operations (except in cases of great urgency) when there is generally someone close at hand more competent to perform them.

The student who sees the modern surgeon at work, whether in a London or provincial centre, is perhaps too apt to imagine that after all operative surgery is a very easy matter. He thinks that he has only to exercise surgical cleanliness, to which nowadays everybody is trained, and that he can then do with impunity the largest and most dangerous operations of surgery that he has seen his teachers doing. Later on, when he gets into practice, especially if he has been fortified by the experience of a few years of war surgery, he is only too apt to find that the actual practice of operative surgery is not quite so easy as he has imagined. He may for instance do many simple inguinal hernias, and some day he will find a puzzling case; and it is not until he has opened the herniated bladder that he learns what this condition looks like in the living, and that it is a more common occurrence than he had previously imagined.

Since these remarks of mine were written, my attention has been called to a tragic and pathetic story recently written by Sir Frederick Treves in his usual masterly and inimitable style. It concerns a young man who had had a distinguished hospital career, had taken the Fellowship of the Royal College

of Surgeons, and had then started practice in a humble country town. He considered surgery to be his *métier*, although hitherto he had had but little opportunity of actually practising it. A serious operation has to be done on a member of his own family and, at the earnest wish of the patient, he consents to operate himself rather than send for a surgeon of more experience from a neighbouring town. The operation proves more difficult than he, in his inexperience, had anticipated, and, as the direct result of it, the patient dies.

The story is told in the guise of fiction, but who can doubt that it is founded on fact. There are probably few of us who do not know of actual cases very similar to that related by Sir Frederick. I strongly recommend everyone to read the story. It is published under the title of "The Idol with Hands of Clay," in the July number of *Cassell's Magazine*.

I had intended to say something on the thorny subject as to where the dividing line should be drawn between the practice of the general surgeon and that of the specialist. I have heard of gynæcologists who operate for appendicitis and even upon gall-bladders, of laryngologists who venture to undertake deep dissecting operations at the root of the neck, a region with which they are not necessarily familiar. On the other hand, some general surgeons have been known to stray into fields which had better be left to the specialist. This subject would doubtless have excited a lively controversy, but my time is short, and I will merely tell you a short story which bears upon it.

Many years ago, in the days when gynæcologists were beginning to do abdominal surgery, I went by chance into an operating theatre, where an elderly gynæcologist was at work. His previous training had been wholly that of a physician, with the exception of what he had received from his surgical colleagues, who had taught him the rudiments of abdominal surgery by helping him at his earlier ovariectomies and hysterectomies.

On this occasion I found him in a state of considerable excitement and sweating profusely. An open abdominal wound had about twenty pairs of forceps on a retroperitoneal mass that he was struggling to remove. He called to me to tell him what it was. A glance was sufficient to enable one to say that it was a solitary horseshoe kidney lying over the promontory of the sacrum. No one with any training in surgical anatomy or pathology could have failed to recognize its nature. He tied off the vessels that had already been cut and closed the wound.

At the post-mortem next day it was found that both renal arteries and ureters had unfortunately been tied before the mistake was discovered. It transpired that the woman had come to his out-patient department complaining of some trivial complaint. He had discovered accidentally an abdominal tumour and thought he would like to remove it. It was a singular and striking instance of a man straying into a field of practice which did not really belong to him.

On the other hand it must be admitted that the general surgeon will sometimes also make serious mistakes when he undertakes operations with which the gynæcologist, now at least, is more familiar than he. An accident which I have never actually witnessed, but which I know has occurred several times at the hands of general surgeons, was once celebrated by some waggish student (whose name I do not know) in the following lines of doggerel:—

There was a young woman named Mopsy,
Who had an ovarian dropsy;
When they plunged in the trocar
A voice exclaimed, "Ma!
They're hurting your own Popsy Wopsy."

He was evidently referring to some operation he had actually seen. I have also known a distended bladder opened by mistake for an ovarian cyst with a fatal result. Both these cases occurred more than twenty-five years ago.

I hope that it will not be thought that I have been unkind in relating the mistakes and failures of other surgeons, most of whom are now dead. Some, but not all, of these errors occurred at hospitals to which I have myself been attached. Some of the surgeons were my early teachers whose memory I cherish with feelings of gratitude, respect and affection. I am sure, that, were they alive, they would rejoice to think that their mistakes should have been utilized for the instruction of others. Indeed I hope that in the future some pupil of mine, who has been warned by the many mistakes and failures that I have myself made, may utilize them for the benefit of a new and more perfect generation. We learn as much from errors that we make ourselves, or see others make, as by our successes.

I have often been asked what is my definition of a good surgeon. Many answers might be given to this question. My favourite is: "One who always knows when to put in and when to take out a drainage tube," to my mind often one of the most difficult problems in general surgery. It is a matter that cannot be learned from books, but only from experience.

For the conclusion of this address I think I cannot do better than quote the description of a surgeon given recently by Sir Frederick Treves in language better than any that I have at my command: ¹—

"A good surgeon is born not made. He is a complex product in any case, and often something of a prodigy. His qualities cannot be expressed by diplomas nor appraised by university degrees. It may be possible to ascertain what he knows, but no examinations can elicit what he can do. He must know the human body as a forester knows his wood: must know it even better than he, must know the roots and branches of every tree, the source and wanderings of every rivulet, the banks of every alley, the flowers of every glade. As a surgeon, moreover, he must be learned in the moods and troubles of the wood, must know of the wild winds that may rend it, of the savage things that may lurk in its secret haunts, of the straggling creepers that may throttle its sturdiest growth, of the rot and mould that may make dust of its very heart. As an operator, moreover, he must be a deft handicraftsman and a master of touch.

"He may have all these acquirements and yet be found wanting; just as a man may succeed when shooting at a target, but fail when faced by a charging lion. He may be a clever manipulator and yet be mentally clumsy. He may be even brilliant, but Heaven help the poor soul who has to be operated upon by a brilliant surgeon. Brilliancy is out of place in surgery. It is pleasing in the juggler who plays with knives in the air, but it causes anxiety in an operating theatre.

"The surgeon's hands must be delicate, but they must also be strong. He needs a lace-maker's fingers and a seaman's grip. He must have courage, be quick to think and prompt to act, be sure of himself and captain of the venture he commands. The surgeon has often to fight for another's life. I conceive of him then not as a massive Hercules wrestling ponderously with Death for the body of Alcestis, but as a nimble man in doublet and hose who, over a prostrate form, fights Death with a rapier."

¹ *Cassell's Magazine*. July, 1922.

Section of Surgery.

SUB-SECTION OF PROCTOLOGY.

President of Sub-section—Sir CHARTERS SYMONDS, K.B.E., C.B., M.S.

Gonorrhœal Stricture of the Rectum :

PRESIDENT'S ADDRESS.

By Sir CHARTERS SYMONDS, K.B.E., C.B., M.S.

[AFTER referring to the loss sustained by the death of Sir Charles Ryall, who had been elected President of the Sub-section at the close of the last Session, Sir CHARTERS SYMONDS continued]:

I HAVE selected a subject for my address upon which there is no little difference of opinion. I propose to confine my remarks to one form of cicatricial stricture, a variety which I have always regarded as possessing a definite entity, and as being due to gonococcal infection.

Let me first describe the form of stricture to which I wish to refer. It is that which involves (in the majority of cases) the lower 3 or 4 in. including the anal margin, but in some cases reaches into the pelvic colon (fig. 1). It affects the whole circumference of the bowel; it is characterized by bands and bridles of indurated and fibrotic muscle, between which are pockets, from the bottom of which fistulous tracks may lead into the vagina or on to the surface round the anus. The extreme narrowing occurs at several points, and is so tight in advanced cases as to prevent digital examination or the passage of a small bougie. One characteristic is the presence of hard polypoid growths ringing the anal margin, a condition not seen to the same extent in any other form of stricture, except that sometimes attributable to dysentery. The history given by the patients is that of discharge from the rectum, together with difficulty in defæcation extending over some years. In married women these symptoms frequently date from the early days of wedded life, in others, where intercourse has been promiscuous, the patients are often younger—aged, say, 25—and a definite history cannot be obtained.

Seven cases of this malady have come under my observation, all occurring in women. Five were under my own care, and two were observed while under the charge of others. Let me call attention to these cases, and briefly relate the symptoms and complications in two of them.

Case I.—The first was that of a woman, aged 41, who was admitted to Guy's Hospital in June, 1887, under Dr. Goodhart, and later transferred to my care, for rectal stricture and abdominal pain, chiefly in the sigmoid region. She was extremely

[November 8, 1922.]

emaciated, and for three years had had a discharge from the rectum. The stricture began at the anus, and was too narrow to admit the finger; pus and blood were discharged with thin acrid faecal matter.

She stated that soon after her marriage in 1863, twenty-four years before, she had syphilis, and was severely ill for some months. There was a perforation of the palate but no other evidence of syphilis. A lumbar colostomy was performed, and it was observed that the bowel looked unusually thin. Though relief was afforded the patient never showed any sign of improvement generally, and suffered a good deal from abdominal pain, occasional diarrhoea, and frequent vomiting. Pus at times in considerable quantities escaped from the artificial anus and occasionally *per rectum*. The temperature was of the hectic type, the pulse quick and feeble, the tongue dry and glazed. Irrigation of the colon did not appear to affect the discharge of pus. She died four months after operation from exhaustion.

The stricture as shown at autopsy involved 3 in. of the rectum, and at two points the narrowing was extreme. The cause of death was seen in the caecum, for this part of



FIG. 1.

the bowel together with the greater part of the ascending colon was dark red and granular from multiple points of ulceration. The rest of the colon and down to the stricture was healthy. There was no lardaceous disease. There was a hard cartilaginous mass in the diaphragm, apparently an old gumma. There can be little doubt that this patient suffered from syphilis, but it is most probable she had also had gonorrhoea.

The next case shows the destructive effects of long obstruction:—

Case II.—A woman, aged 41, admitted under my care in May, 1907, for rectal stricture and the constant discharge of thin foul-smelling material. Twenty years ago, soon after her marriage, she had pain in the rectum accompanied by a brown discharge, a condition that has existed more or less ever since. The patient was wasted and ill, and in much suffering, and was constantly worried by the rectal discharge. The anus was surrounded by the usual hard pendulous masses, the finger could be passed for a distance of 2 in., and was then arrested; the surface between this point and the anus

14 Symonds: *Gonorrhæal Stricture of the Rectum*

was devoid of mucous membrane, and marked by hard ridges and pockets. The abdomen was somewhat distended, and there was some hypertrophy of the bowel.

On May 4 left inguinal colostomy was performed. Examination showed that the thickening and hardness extended up to the sigmoid, and there were adhesions between the broad ligaments, rectum and pelvic wall. The bowel was not opened. The next day the temperature rose to 100° F., the pulse to 104. On the third day it reached 108·6° F., and the pulse 130. There was some sickness but no distension. On the fourth day the bowel was opened and washed out. The material escaping was brown, thin, and foul smelling, and ran out continuously, the margins of the wound became black and the surrounding skin inflamed. The patient began to emaciate, the tongue, the mouth, and the skin became peculiarly dry, while the odour exhaled from the body was most disagreeable. The colon was irrigated with saline and various antiseptics without relief. On the sixth day the temperature was 95·2° F. to 97° F., and remained so to the end. The pulse fell to 88 and 98 by the tenth day. The amount of nourishment taken was small. She was frequently sick and was losing ground. On the twentieth day the appendix was opened, sutured to the abdominal wall, and the colon irrigated. Next day there was a marked improvement. The tongue was less dry, she looked less dried up and took more nourishment, and we had hopes of her recovery. The colon was irrigated through the appendix twice daily, on some occasions with hydrogen peroxide, but each day it seemed to cause more distress, and had to be abandoned. The green foul discharge disappeared, but the downward progress was steady. The skin round the artificial anus had sloughed from the contact of the acrid discharge. She died on May 31, just four weeks from the operation.

At the autopsy the mucous membrane was grey and cicatrized from the anus up to the colostomy opening. The lower 8 in. were marked by dense thickened masses, with intervening pockets. Further up, the wall was thickened, and though the lumen was not seriously contracted, there was general ulceration. The cæcum and colon did not show any ulceration as had been anticipated, nor was any other morbid condition found.

The fatal termination was unexpected, for the patient had been doing her ordinary household work up to the time of her admission. The rise of temperature immediately after the operation may have been due to the injury, slight as it was, to the wall of the bowel. The rapid emaciation after the bowel was opened, the inability to take food, the remarkable dryness of the tongue and mucous membrane of the mouth and of the skin, were no doubt due to absorption of toxins from the colon and edges of the wound. During this period it will be remembered that the temperature was 97° or under. The rapid change in the colour of the thin fluid escaping from the bowel—from brown to green—and the sloughing of the skin, together with the disagreeable odour of the body, were in part due to starvation, and diminished when nourishment was taken. The improvement following irrigation through the appendix was brief, and yet so definite that for a few days one could not but regret that it had not been adopted earlier. One surmised that, as in the previous case, the ulceration would have been found in the cæcum, and if so, probably greater benefit would have resulted.

This case exhibits the secondary effects of obstruction of long duration and points to the necessity for early colostomy. It would appear that the injury of the operation determined the onset of an infection, which, held in abeyance, only needed some such cause to set it alight. It is comparable to a case to be mentioned later, in which a digital examination determined profuse diarrhoea and death in twenty-four hours.

Case III.—A woman aged 40, the wife of a hotel proprietor, and therefore in easy circumstances, had a typical impassable stricture. She was in good health and well nourished and was relieved by a colostomy. She had had symptoms for some years and there was no evidence of syphilis.

Of the other cases one, as just mentioned, died after a digital examination. This was, I believe, an attempt to force the finger through the stricture, and caused some pain. The most profuse and continuous escape of thin, evil-smelling fluid followed, and death from collapse. In another the polypoid masses were removed, but whether a rectal examination had been made I cannot say. The result was disastrous, for death from acute septicæmia took place in a very short time. These two cases I saw while Surgical Registrar in 1880-82.

In two cases removal was attempted after colostomy. In one where the finger could reach healthy bowel the operation was completed and the colostomy wound closed; the anal control was fairly efficient. I may mention that an interval of six months was allowed to elapse between the colostomy and the excision, to permit of the subsidence of any existing periproctitis. In the other, after extending the dissection up to the peritoneum without reaching healthy bowel the operation was abandoned. The feasibility of excision can usually be ascertained at the time of the colostomy.

In all seven cases the stricture was of the character described in the earlier part of this communication.

What is the cause of this form of stricture? The appearances show clearly that it is the result of cicatrization following upon a deep and widespread ulceration. As to the cause of this ulceration there is no unanimity of opinion. Many of the museum specimens are labelled syphilitic, a nomenclature adopted, it would appear, more or less from tradition and without adequate investigation. It is a well established observation that this variety occurs almost exclusively in women, an incidence demanding a local explanation. Were it due to any constitutional condition, such as syphilis, it should be encountered with equal frequency in the male subject.

Let us first consider the question of sex. The seven cases were, as I have said, all women. Turning to museum specimens there are at Guy's Hospital thirteen available for classification, of these ten are from women and three from men. The youngest amongst the women was 22, two were aged 25, two 42, and the remaining five were between 30 and 40. Where the genitalia are attacked there is evidence of local peritonitis as is shown by adhesion of tubes and ovaries, a well recognized effect of the extension of a local infection usually attributed to gonorrhœa.

The three specimens from men show characters identical with those from women, including in one well-marked polypoid anal masses. In some of the cases from women there was in the body evidence of old syphilis, and while this association is not mentioned in the history given in the three cases from men, no particular importance can be attached to the omission one way or the other.

Death was directly attributable to the effect of the stricture in several of the cases, e.g., from peritonitis, septic extension, prolonged diarrhœa and exhaustion, lardaceous disease—in a man who passed 5 to 6 oz. of pus daily for two years—while others died from phthisis—one of these having tuberculous ulceration of the ileum—another from pneumonia. In one death was due to pyæmia, probably following the use of bougies.

I find similar specimens in other museums in London :—

- (1) In St. Thomas's Hospital: Two, both from women.
- (2) Middlesex Hospital: One, also from a woman.
- (3) St. Bartholomew's Hospital: Eight; seven from women and one without sex being indicated.

16 Symonds: *Gonorrhœal Stricture of the Rectum*

(4) London Hospital: Seven, four of which are from women, no sex being indicated in the other three.

(5) Royal College of Surgeons: Seven, three of which are from women and two from men. One of these latter is very typical, showing bridles and pockets. It came from a man of middle age who had used bougies. The other is a Hunterian specimen (2571), and shows ulceration and narrowing of the whole rectum and part of the colon.

There are thus in the museums examined thirty-eight examples, of which twenty-seven are from women, six are from men, and five are unspecified. If the seven cases seen during life be added, we get a total of forty-five cases, thirty-four of which are from women and six from men, a difference of incidence calling for some explanation.

In the descriptions in the catalogues of the museum specimens, are found such remarks as: "Suffered from syphilitic disease of the rectum"; "affected by what was supposed to be tertiary syphilis"; "supposed to have resulted from syphilis." Others are frankly called: "Syphilitic ulceration," or "destruction from syphilitic ulceration," or "the stricture was due to syphilitic ulceration"; again, "syphilitic stricture of the rectum"; others, and in appearance identical, included in my list are simply labelled "fibrous stricture."

Thus it will be seen that while some observers have been wisely doubtful, others have not hesitated, yet the stricture in all is of the same type. In a few there has been evidence of syphilis in perforation of the palate, a cicatrized gumma, and lardaceous disease. The occurrence of both syphilis and gonorrhœa in the prostitute class is to be expected, and not much evidence of constitutional disease can be adduced from a museum specimen in the absence of a clinical history: on the other hand, in those observed during life—five cases—there was no evidence of syphilis.

To establish the infective view information is required as to the early stage of gonorrhœal proctitis. Not much information is available, nor am I in a position to communicate any personal observations. My friend, Dr. Stebbing, of the Lambeth Infirmary, has been good enough to supply me with some notes of cases admitted under his care. He writes:—

"I find that I have brief notes of seven cases of definite gonorrhœal proctitis in which the diagnosis was confirmed by bacteriological examination, and in three of which proctoscopic examinations were made. Six of the cases are in women of the prostitute class, and one in a man who had at the same time gonorrhœal urethritis. One of the cases had already proceeded to the stage of stricture, and three of the cases showed 'Paget's cocks'-combs' well marked."

He tells me further that the mucous membrane showed numerous small ulcers, over all parts, and in one case these reached into the pelvic colon.

Few as these observations are, I think it will be allowed that they establish the existence of gonorrhœal proctitis. I by no means wish it to be inferred that the observation is new, or that the disease has not been described. The observations are valuable also from their completeness. There is a vagueness in many of the accounts one comes across. Writers on syphilis and gonorrhœa admit gonococcal proctitis in both sexes. Thus McDonagh says: "Owing to the proximity of the anus and vagina, gonococcal proctitis is more common in women than in men. It occurs in a higher percentage of cases than is generally thought." This author describes the mucous membrane as "red, swollen and granular, bleeding easily when injured, and there are usually several erosions, some of which may be covered with a sort of membrane. Only rarely does true ulceration occur, which is sometimes followed by stricture."

Does the occurrence in the male weaken the position and lean the balance in favour of a constitutional cause? First, as to the similarity of the stricture in men with that already described in women, I would call your attention to this specimen from the Guy's Hospital museum (fig. 2). The limitation to the lower 3 or 4 in., the involvements of the anal margin, the marked pendulous masses, the ridges and pockets are all present. There can be little doubt that it is due to the same cause. The other specimens from those referred to show more or less the same characters, and it will be remembered that there were six collected from the museums as against thirty-four clinical and museum examples from women. In the case of the man mentioned in Dr. Stebbing's note, ulcers were present, and he had definite gonorrhœal urethritis but no signs of syphilis, the Wassermann reaction also being negative. This case



FIG. 2.

establishes the occurrence of gonococcal proctitis, in the absence of syphilis, in the male, and emphasizes the view that the stricture under consideration is in both sexes due to this mode of infection. Writers on this subject, for the most part, accept a syphilitic basis for this form of stricture, describing, however, what appears to be an identical disease as "cicatricial stricture," without any causal relation. Others are less decided, thus Mr. Mummery writes, in his book on "Diseases of the Rectum and Anus":—

"While I am not in a position to deny that syphilis may be a cause of rectal stricture, personally, I have never met with a case of syphilis of the rectum (including, of course, condyloma of the anus and chancre), and at St. Mark's Hospital there is no reliable record of a case of tertiary syphilis of the rectum or of a syphilitic stricture."

And again he says:—

"It is well known that antisiphilitic treatment does no good in cases of rectal stricture."

A careful, and it seems to me, well-considered opinion is expressed in Thomson and Miles's "Surgery," and is as follows :—

"The commonest cause of non-malignant stricture of the rectum is chronic pyogenic infection, probably in most cases of gonococcal origin.

"The proportion of syphilitic cases has been exaggerated on account of the tendency to attribute to syphilis all strictures in which no other definite cause can be discovered."

With this view I fully agree, and it supports my plea that the nomenclature is largely a matter of tradition, and has been adopted without full investigation.

The following statement is made in the American "Treatise on Diseases of the Rectum and Anus," edited by A. B. Cooke :—

"We must record our conviction that gonorrhœal infection and syphilitic ulceration must be reckoned with in a great majority of cases.

"I have observed 100 cases of fibrous stricture in the rectum and 75 per cent. of that number was traced to syphilis, including five due to chancroidal ulceration."

This rather suggests that when syphilis existed the stricture was set down to this disease. One would require information as to the presence, recent or remote, of gonorrhœa before accepting so large a proportion as due to syphilis. Moreover, from the observations above related, one would be inclined to question the syphilitic origin of any of the cases.

The chief reason in favour of this form of rectal ulceration and stricture being determined by a local cause is its prevalence in women; clinically, I have never seen it in men. The easy access of discharges from the vagina to the rectum in women is obvious. I am not prepared to deny that the secretions from chancres do not determine the ulceration, but I would submit that the disease is due to local and not constitutional causes, otherwise it should occur as frequently in men as in women. Nor can it be asserted that syphilis is wholly absent and it is possible that local irritation may determine a syphilitic deposit. Gonorrhœa is, we know, a far more frequent infection than syphilis, and the well-known tendency to the formation of cicatricial contraction is seen in the male urethra. Again, the occurrence of adhesions of the Fallopian tubes and ovaries found in these cases supports the view that gonorrhœa has existed. A local infection in the male is not by any means impossible where inattention allows the discharge to collect on the clothing; and there is, I am informed, amongst a particular class, a more direct mode of infection. Since the public are better informed and better treated and take a more serious view of a malady that at one time was treated as a joke, rectal complications will, no doubt, be less frequent.

Whatever view we adopt, whether it be that of a pure gonococcal infection or one of a mixed character, I submit we cannot assign constitutional syphilis as the cause. It will be nearer the truth to speak of this form of rectal stricture as due to local infection from the labial and vaginal discharges, and as the most frequent disease is gonorrhœa, to call it "gonorrhœal stricture."

TREATMENT.

(1) *Bougies*.—Use of bougies in the early stages has its rightful place, but in more advanced stages I question the wisdom of this method, and would certainly limit it to those cases where healthy bowel can be reached by the finger. If employed where a limit cannot be reached, a perforation may be made with fatal consequences through a thin-walled pocket of ulceration. The danger attending a too rapid dilatation needs remark. The fatal result of a

single digital examination I have mentioned and the forcing of a bougie through a stricture may be followed by septic infection. As in urethral stricture, a bougie fitting loosely and left in some hours will do more good than one that fits tightly.

(2) *Division*.—The same limits should, in my opinion, be set to division, seeing that the disease may extend into the pelvic colon.

(3) *Excision*.—This again, I take it, is applicable to those cases in which a limit can be reached by the finger. I have been successful in one case, and have had to abandon another. What success has attended the endeavours of others with a larger experience?

(4) *Colostomy*.—This method occupies a foremost place in the management of these cases, and in the advanced forms constitutes our one and only means of relief. As a preliminary to excision I would consider it essential, and its performance before incision appears to be a wise step to take.

(5) *Cæcostomy*.—In long standing cases such as I have described at the beginning of this paper, in which from the character of the rectal discharge ulceration may be presumed to exist, cæcostomy as a primary operation would seem to be indicated. Had this been adopted in the first case in which the ulceration affected the cæcum and ascending colon, a better result would probably have resulted. And in the second, though the ulceration was in the ascending colon, the removal of fæcal infection could only have been beneficial.

(6) *Appendicostomy* was quite successful as a means of irrigating the colon in the one case referred to, and though marked improvement resulted at first, this was not maintained, and irrigation had to be abandoned on account of the pain it occasioned.

DISCUSSION.

MR. SWINFORD EDWARDS said that when he was a student most strictures of the rectum when not malignant were supposed to be syphilitic but this view had long been exploded. He personally had never seen a syphilitic rectal stricture though he had seen stricture in patients who were also the subjects of syphilis. Excluding carcinoma, tubercle, dysentery and traumatism, gonorrhœa was probably the cause of the remaining strictures, which meant that it was the origin of most fibrous strictures of the rectum, otherwise how could one account for the large preponderance in women, who suffered from gonorrhœal infection of the rectum much more frequently than men did? He was therefore in full accord with the conclusions of the President. Regarding the two cases which were illustrated by the epidiascope: he asked whether they might not have been tuberculous, for they much reminded him of some cases of this condition which had been under his care and which were undoubtedly tuberculous. As to treatment, he recommended hypogastric colostomy for those cases of tubercular stricture extending to the upper rectum, or even beyond. Excision was risky and nothing much to be gained by it. For strictures within three or four inches of the anus—posterior linear proctotomy was the operation of choice as it afforded good drainage and in this respect was a safer operation in Mr. Edwards's opinion than an internal proctotomy even when supplemented by division of sphincter. He mentioned the case of an officer with multiple tight fibrous strictures of the rectum and colon, due to poisoning by ground glass. The treatment consisted of appendicostomy and subcutaneous injections of fibrolysin. The patient made a good recovery and after some months was passing normal sized stools. Mr. Edwards heard afterwards that wishing to get rid of his appendicostomy he got admitted into a military hospital where an operation took place to which he succumbed.

DR. LAPHORN SMITH said that they were all very much indebted to Sir Charters Symonds for the very thorough and painstaking work shown in this paper which was the very last word on the subject. There were very few Fellows of the Society who would have gone to the trouble of visiting the museums of the great hospitals actually to find out for themselves what was to be learned from these specimens. He had shown that

this disease of the rectum was due to gonorrhœa and not to syphilis, which was of great importance when it came to the treatment. Dr. Lapthorn Smith had had under his care only one case of venereal stricture of the rectum and he had lost much precious time in treating it as a case of syphilis instead of gonorrhœa to which they now knew it to be due. This unfortunate patient, whom he attended nearly forty years ago, had a long thick stricture of the rectum composed of very friable tissue. If he were to have such a case to-day, with the knowledge which Sir Charters had placed in our possession, he would treat it with injections as a case of gonorrhœa anywhere else. If that failed he would do a colotomy and about six months later if the inflammatory thickening had not subsided he would remove the rectum and bring down the intestine and attach it to the sphincter. In one case he had done for cancer the woman had perfect control and lived in comfort for many years after. If after removal of the rectum for stricture the woman had control of the sphincter the colotomy might be closed subsequently. At one time he had used a great deal for various strictures, metal bulbs, to which was attached the negative pole of the galvanic current, with very good results, as the galvanic current seemed to favour the absorption of fibrous deposits. Perhaps this might be tried in gonorrhœal strictures before resorting to the major operation.

Mr. ASLETT BALDWIN said that about the middle of 1919 he saw a young woman who was very ill with intestinal obstruction of five days' duration. A year previously she had been in a hospital for four months, where she was treated for gonorrhœa associated with abscesses, which had been allowed to burrow extensively. There was a large mass in the left side of the abdomen, which was the loaded colon. There were several anal fistulæ, and one recto-vaginal fistula. About 2 in. up the bowel there was a stricture, which would not admit the end of the index finger. Eventually the bowel was cleared in a nursing home. Mr. Baldwin operated on the fistulæ and dilated the stricture to 22 Hegar. The patient had been passing bougies herself daily. Mr. Baldwin had seen her about every fortnight, and had passed bougies and given negative ionization. She had long been in excellent health. She ceased attending about a year ago. He (Mr. Baldwin) received a letter from her about a month ago, thanking him for all he had done, and saying she had been instantaneously cured in mind and body by Christian science.

Mr. LOCKHART-MUMMERY said he thought it unwise to label these strictures as being due to any one specific cause. He believed they resulted from chronic sepsis, due to any cause, and although the preponderance of cases among women suggested gonorrhœal origin, it was probably secondary septic infection, resulting in a stricture rather than a specific infection. Mr. Lockhart-Mummery said he had seen in all about three cases of gonorrhœal proctitis, but none of these had resulted in a stricture of this nature, which was probably due to the fact that they were properly treated. He had seen this form of stricture result from many different causes, such as operations upon the rectum followed by sepsis, glass poisoning, tubercle and scalds. One patient at a large hospital was given a boiling enema by a nurse, and a similar case occurred only recently in a nursing home, with stricture resulting; and stricture used to be common as a result of sepsis in St. Mark's Hospital before the days of antiseptics, and was then no doubt the result of infection from dirty enema nozzles, &c. His own belief was that these cases of stricture of the rectum resulted from neglected chronic sepsis due to any cause. Rectal strictures were certainly far less common at present than they used to be, this being doubtless due to the fact that they were now much more carefully treated in the early stages. As regards treatment, Mr. Lockhart-Mummery agreed with the President in thinking that the majority of cases were best treated by means of proctotomy where the stricture was strictly localized and did not reach above the peritoneal reflexion. He thought internal proctotomy would give excellent results, and he had had quite a number of cases in which this has been successfully done.

Mr. LIONEL NORBURY asked for an opinion as to the relative advantages of internal proctotomy and excision in cases of stricture of the rectum situated below the level of the peritoneal reflexion. He considered that treatment after internal proctotomy by means of bougies, &c., must needs be continued for a very long time, with much discomfort to the patient, and that such treatment was not always satisfactory. He was also of opinion that excision should be carried out when possible for strictures low down in the rectum.

Section of Surgery.

President—Mr. JAMES BERRY, F.R.C.S.

On Subcapsular Pyelotomy, with Remarks on the Origin and Treatment of Renal Calculi.

By W. SAMPSON HANDLEY, M.S., F.R.C.S.

IT is usually held that renal calculi form either in the pelvis or in the calyces. Sir J. Thomson-Walker appears to regard it as very doubtful whether a calculus can form in the renal substance. He says: "It is said that a calculus has rarely been found embedded in the substance of the kidney and unconnected with the calyces. I have not met with such a case."

From personal observation in two cases, I am convinced that the first step to the formation of a calculus may be the appearance of a cyst in the cortical substance. In this cyst a calculus is later deposited and it is likely later on that the cyst may rupture into a calyx in which accordingly the calculus may come to lie. I do not suggest that this mode of formation is of frequent occurrence, but unless it is recognized, the chance of overlooking a stone during operation is increased.

Observation I.—In a case under my care, renal calculi were successfully removed from an infected left kidney. A second operation for the removal of multiple calculi from a disorganized right kidney was fatal. The necropsy showed that all calculi had been removed from the kidney, but in its upper pole, unconnected with the calyces, was a small cyst containing turbid fluid and lined by a layer of gritty debris. Small gritty areas could be appreciated by the finger, though they could not be seen, in the substance of the left kidney.

Observation II.—After the removal of a group of calculi from a kidney by subcapsular pyelotomy, the two smallest calculi, one of them known to be of recent origin from a comparison between two radiographs of different dates, could not be detected. A probe was passed from the pelvis into the calyces but it failed to detect them. Needling of the kidney substance in the suspected region was equally unsuccessful. A finger cautiously introduced into the renal pelvis also failed to find them. On bidigital palpation between this finger and a finger laid on the convexity of the kidney slight indurations not hard enough to suggest a calculus were felt in the suspected regions. At these two points the cortical region was incised with the point of a knife. Each incision entered a small smooth-walled cystic cavity containing turbid fluid, within which lay a small mobile calculus about $\frac{3}{8}$ in. in diameter (*see* fig. 1, A and B). It is certain that these cavities did not communicate with the pelvis, for it was already freely open, and there was nothing to suggest their origin from the blocking of calyces.

One of these two stones was known to be the youngest calculus present in the kidney, a fact which suggests that the others, after originating in cysts in the same way, had passed secondarily by ulceration or rupture into the calyces and the pelvis.

Observation III.—In the Museum of the Royal College of Surgeons is a specimen numbered 3618 (now shown) of a kidney split open, and containing a calculus wedged in the pelvis. Near the middle of the convexity of the kidney and lying close under its capsule is an empty cystic cavity, ovoid in form, and measuring 1 in. by $\frac{1}{2}$ in. in section.

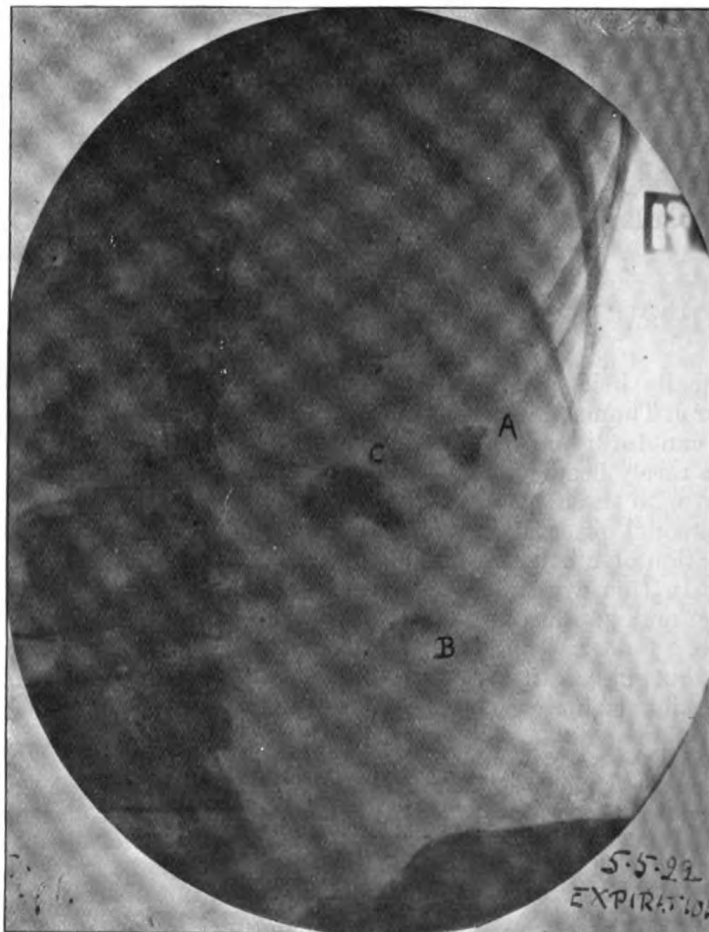


FIG. 1.—Bilateral renal calculus. Case of Mrs. H. Radiograph by Mr. W. A. Coldwell of the right kidney. C is a large calculus in the pelvis. A and B are two small calculi which were found lying in cysts in the renal cortex unconnected with the calyces. In a radiograph taken eight months earlier only calculi B and C were present.

This communicates by a narrow, oval opening about $\frac{1}{4}$ in. by $\frac{1}{8}$ in. in measurement, with a long dilated calyx, $\frac{3}{4}$ in. in diameter, which leads down to the blocked pelvis.

I suggest that the stone formed in the cystic cavity, which ulcerated into the calyx. The stone then passed into the pelvis and lodged there. The observation is from the nature of the case a suggestion only, but I believe the specimen represents a third stage in the history of a cyst-calculus. How otherwise can one explain the existence of a

globular cystic cavity at the blind end of the calyx, extending right to the surface of the kidney, and communicating by a constriction with the dilated calyx?

It is noteworthy in the descriptions of kidneys with renal calculi that the calyces are often described as extending almost to the surface of the kidney, even in kidneys the substance of which is not greatly atrophied. I suggest that in some cases the portion of these supposed enlarged calyces which lies in the cortex is the original cavity of a cyst in which a stone has formed, and that the communication with the calyx is secondary (*see fig. 2*). The normal calyces end at the apices of the pyramids and do not extend into the cortical substance.

I have not been able to form any idea as to the frequency with which a renal calculus originates in a cyst of the cortex, but my observations prove



FIG. 2.

FIG. 2.—(No. 3631, Path. Series, R.C.S. Museum.) A kidney, in which the pelvis and all its branches are dilated into large pouches, over which the glandular substance is spread out and atrophied. The dilated pelvis and many of the calyces are full of large calculi.

Note that there is no general atrophy of the renal cortex. It appears likely that the "large pouches," in which the calculi lie, and which extend nearly up to the capsule were formed, not by dilatation of calyces, but as cysts in the renal cortex.

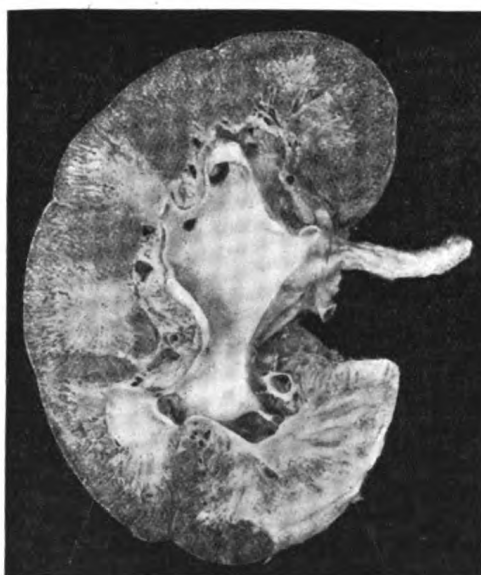


FIG. 3.

FIG. 3.—(No. 289, Anatomical Series, R.C.S. Museum.) Posterior half of injected normal kidney looked at from the front.

Note that the splitting of the kidney has failed to open up several of the calyces.

beyond a doubt that a calculus may originate in this way, and I suspect that this mode of origin is frequent. It is evident that, just as in the case of a sacculus of the bladder, the little stagnant pool of urine in a urinary cyst of the kidney supplies conditions very favourable to precipitation.

With these few remarks on the pathogenesis of renal calculi, I now pass to the more important part of my paper, a consideration of the best method of removing stones from the kidney.

NEPHROLITHOTOMY.

I maintain that the operation of nephrolithotomy, or splitting of the kidney, is an unscientific procedure to be regarded only as a last resort. It is an operation sometimes attended by dangerous bleeding and always by permanent injury to its secreting substance. Moreover, it may fail in its object of exposing all the calyces, for one or more of them may not lie in the place of the incision (*see* fig. 3). It should be reserved for cases of very large calculus such as one which I removed successfully, which measured 5 in. in length, and for cases in which the necessary exposure for an incision into the renal pelvis cannot be effected owing to a short pedicle or a deep loin. During the last ten years I have only twice been driven by necessity to do a nephrolithotomy in preference to a pyelolithotomy.

Thomson-Walker in his work on "Surgical Diseases and Injuries of the Genito-Urinary Organs," adopts, it seems to me, too detached an attitude in discussing the rival claims of nephro- and pyelolithotomy, and yields too large a territory to the former operation. But that his real preference is for pyelolithotomy is shown by his recommendation of this operation in the critical cases in which a stone has to be removed from a solitary functional kidney.

Cases Suitable for Nephrolithotomy.—Very large calculi, irremovable by way of the pelvis, can sometimes be extracted by splitting the kidney, without a nephrectomy. In cases with a short pedicle and a deep loin, pyelolithotomy may be impossible, and nephrolithotomy quite feasible.

RESTRICTED NEPHROLITHOTOMY.

Though "splitting the kidney" in a plane posterior to its mid-coronal plane has thus a very limited field in present-day surgery, there is something to be said for what may be called restricted nephrolithotomy. As Hartmann has insisted, if upon exposure of the kidney a stone can plainly be felt in its substance and near to its surface, it is best to cut down upon it directly through the renal substance if the radiograph has shown that the stone is single, and that therefore further exploration of the kidney is unnecessary. A small incision only is necessary, little damage to the kidney substance is done, and the operation has the merit of rapidity, directness and simplicity. If several calculi are present, and if each of them is to be plainly felt near the surface, a cut directly down upon each of them may be the best course to pursue, but only if the operator is certain beyond doubt that he can feel each of the calculi shown by the radiograph. After limited nephrolithotomy each of the incisions should be closed by one or more loosely tied catgut sutures. During the incision of the kidney and the extraction of the stones, the kidney pedicle should be compressed by the assistant's fingers.

BIPOLAR NEPHROTOMY (LEGUEU).

In cases in which the stone or stones cannot be palpated externally, some surgeons recommend that the kidney-pelvis should be reached by two incisions which follow respectively the upper and the lower thirds of the kidney-splitting incision, the uppermost and the lowest calyx being opened. In this way it is said that the whole kidney can be explored. I do not think, however, that free access to the pelvis can be obtained in this way. The communication of the upper calyx with the pelvis is often a very narrow and restricted orifice (fig. 4), and I found in a case of my own that it was useless for reaching and exploring the pelvis. The same objection applies to the lower calyx, though to a much less extent.

UNIPOLAR NEPHROLITHOTOMY.

Albarran recommends an incision into the lower pole, following the lower third of the convex border of the kidney, as a means of exploring the renal pelvis. The same objection applies to this operation. It does not open the main cavity of the pelvis, and affords very restricted access to that cavity. Though it is true the narrow opening of the calyx into the pelvis may be enlarged with scissors, it still seems undesirable and clumsy to approach a branching cavity such as the renal pelvis from the side where it breaks up into branches. Strategically, it should be approached from the opposite side, where it is a single simple cavity from which the branches diverge. From this point and from no other all the branches can be explored with ease. The plumber seeking a defect in a drainage system opens the manhole into which the accessory drains converge, and from which they are all accessible. He does not reach the manhole by splitting up one of the small drains and enlarging its orifice into the manhole, but opens the manhole directly. For reasons similar to those which guide the plumber, I believe lower pole

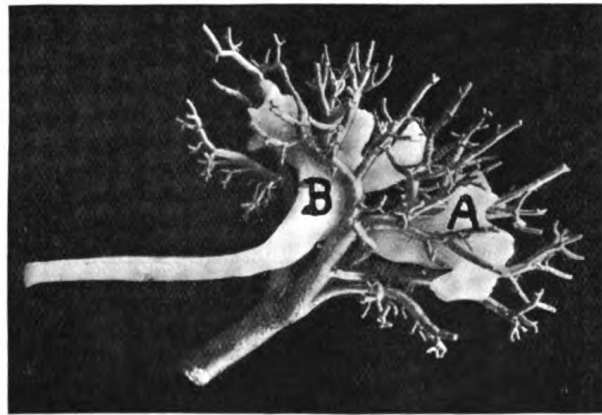


FIG. 4.—(Physiological Series, R.C.S. Museum.) Metallic injection of the chief veins of the kidney. Pelvis and ureter injected with wax. Prepared by Professor Wahby, of Cairo, and presented by the Egyptian Government in 1914.

This figure is inserted to show the narrowness of the communication between the upper calyx A and the body of the renal pelvis B in some kidneys.

nephrotomy to be an unsatisfactory operation, and that, save for the limited exceptions already made, the kidney should be explored for stone by way of the renal pelvis, for this cavity bears the same relation to the kidney as the manhole to the drainage system from the fact that all the accessory ducts converge to it and can be explored from it; and there is the further resemblance that the renal pelvis like the manhole can be easily opened and reclosed without injury to the drainage system.

PYELOTOMY.

At the present day there would probably be a consensus of opinion that stones known to lie in the pelvis should be extracted by pyelotomy. I would go further and maintain that pyelotomy should be the routine first step in the operative examination of a kidney for calculus, and that nephrolithotomy should be regarded only as a last resort.

The Technique of Pyelotomy.—The opening of the renal pelvis is an operation of comparatively recent introduction. I find for instance that in the 1904 edition of Cheyne's and Burghard's "Manual of Surgical Treatment" the operation is not mentioned. The accepted method of performing the operation is thus described by Thomson-Walker¹:—

"The kidney is drawn out of the lumbar wound. The organ is grasped in the left hand of the operator and turned forwards and upwards so that the posterior aspect of the pelvis is exposed. The fat covering the pelvis is removed with dissecting forceps. A posterior branch of the renal artery lying immediately within the renal sinus and irregular vessels must be avoided. If a stone is felt in the pelvis it is made prominent by pressure of the fingers on the front of the pelvis, and a longitudinal incision is made upon it through the posterior wall. The stone is then removed with forceps.

"If a stone is not felt, the kidney is given to an assistant to hold, and a longitudinal incision is made in the pelvis about $\frac{1}{4}$ in. in length, a fine catgut suture passed through each lip, and the wound held open by these sutures. A probe is now introduced and the pelvis and calyces are explored. If a calculus is now felt, the probe is held in position and a pair of forceps slipped along it, the stone grasped and removed.

"After removal of the stone, the edges of the wound in the pelvis are brought together by interrupted stitches of fine catgut. Over this a row of Lembert's sutures may be inserted.

"Since 1905 I have covered all wounds in the renal pelvis with a flap of the fibrous capsule turned down from the kidney and stitched in place. This has proved very successful in preventing the escape of urine and promoting primary healing. Mayo recommends a flap of fatty tissue for the same purpose.

"A drainage tube is placed behind the kidney and the lumbar wound closed. Usually there is no escape of urine, but occasionally some urine leaks for a few days. Rarely this continues for a fortnight or longer, and a urinary fistula may become established.

"The cases which are suitable for pyelolithotomy are those of small unbranched stones lying in the pelvis."

Some surgeons, notably Hartmann, of Paris, maintain that it is quite unnecessary to suture the incised pelvis. Hartmann records six cases of pyelolithotomy, all of them successful. No escape of urine from the wound occurred in any of these cases. Hartmann states that Mayo, in America, has also abandoned suture of the pelvis.

There seems to be no doubt that suture of the pelvis is unnecessary. I may remark that Sir J. Bland-Sutton has found suture of the common bile-duct after incision also superfluous. The insertion of a double layer of sutures in the pelvis is a tedious business, which increases the strain of the operation upon the patient's vital powers, while it must also tend undesirably to diminish the size of the pelvis. I cannot help thinking, however, that the firm, immediate and accurate closure of the pelvis after incision is an end desirable in itself if it can be attained quickly and simply. For the past ten years I have employed the method which I will now describe.

SUBCAPSULAR PYELOTOMY.

The kidney having been exposed by a lumbar incision is brought up to the surface intact in its capsule. All small bleeding vessels are clamped and tied. An incision about 3 in. long (fig. 5), and following the long axis of the posterior surface of the kidney, is now made through the fibrous capsule. It does not involve the substance of the kidney. Two pairs of forceps are clipped upon

¹ "Surgical Diseases and Injuries of the Genito-urinary Organs," 1914, p. 273.

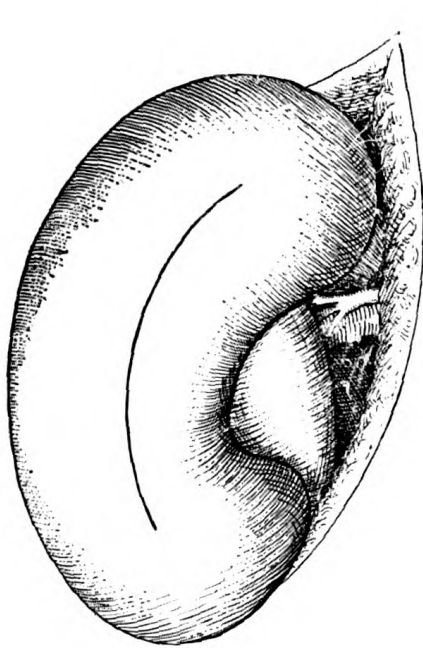


FIG. 5.

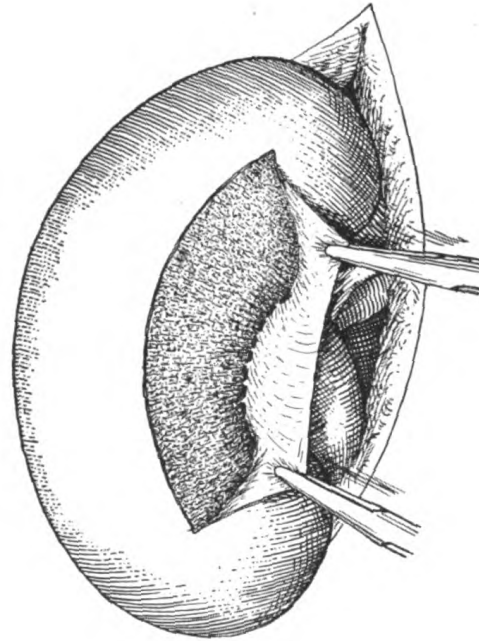


FIG. 6.

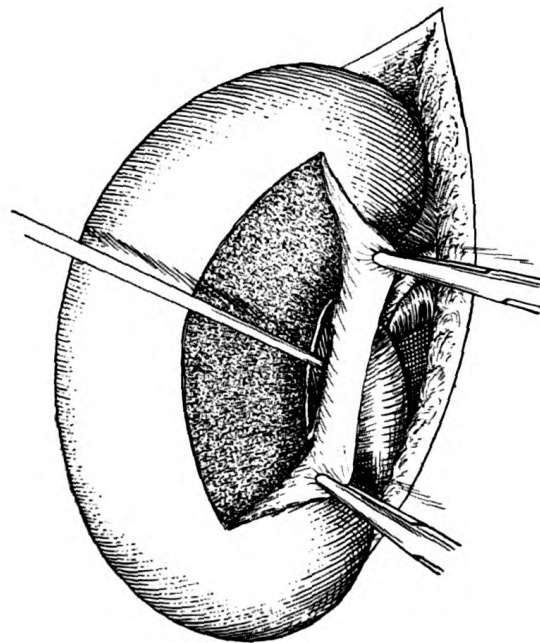


FIG. 7.

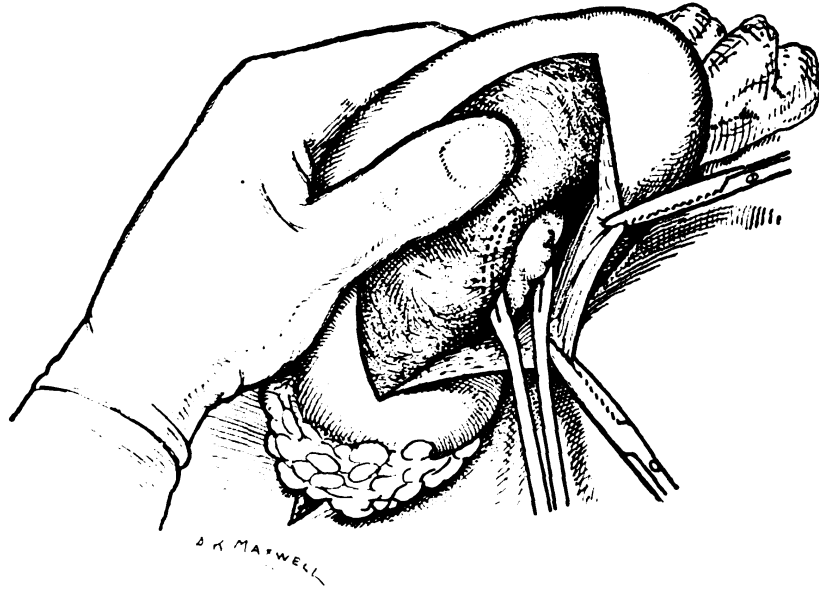


FIG. 8.

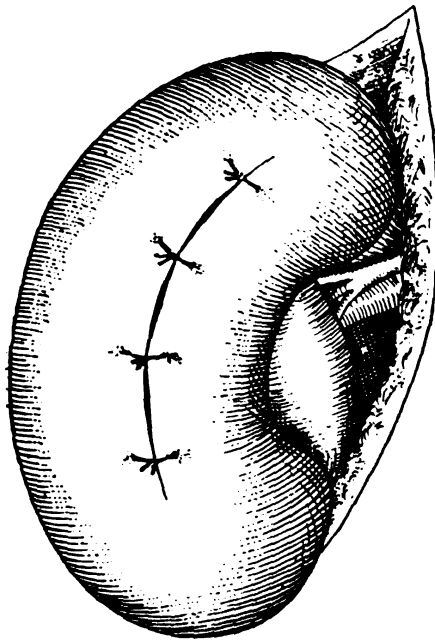


FIG. 9.

the posterior edge of the incision in the capsule. While these are steadied, a pair of dissecting forceps is insinuated beneath the capsule in the direction of the pelvis until the entrance to the hilum of the kidney is reached (fig. 6). Here the capsule, or one layer of it, leaves the kidney and passes on to the posterior surface of the pelvis. The forceps or blunt dissector enlarges the pocket formed beneath the capsule until at its bottom the line of entrance of the pelvis into the substance of the kidney is reached. A knife is now taken and with its point the posterior surface of the pelvis is cautiously incised at the bottom of the capsular pocket in the long axis of the kidney. Forceps (fig. 7) or, if necessary, the finger can now be introduced into the pelvis, stones present in it removed (fig. 8) and its recesses explored.

When the opening in the pelvis has served its purpose it can be completely and rapidly closed by three stitches uniting the edges of the original incision in the capsule on the posterior surface of the kidney (fig. 9). No suturing of



FIG. 10.—(Physiological Series, R.C.S. Museum.) Injection with fusible metal of the arterial system of the kidney, by Professor Wahby, of Cairo. Note the posterior branch of the renal artery.

the renal pelvis itself is necessary and the duration of the operation is correspondingly shortened. In the case of an infected kidney, the chances of infection of the perirenal tissues are much reduced because the flap or pocket of capsule prevents septic fluid from escaping into them from the incised renal pelvis. The security against a urinary fistula appears to be absolute.

Complications.

I have performed this operation, supplemented when necessary by cortical extrusion of stones, in perhaps nine unilateral cases of renal calculus, and in two bilateral cases. In all the unilateral cases, the calculi were small and lay in the pelvis or in calyces from which they could be extracted by way of the pelvis without incision of the kidney substance. In none of the unilateral cases did any complications worth noting present themselves. In two or three

cases a discharge of slightly urinary odour was present for a few days. In one case a stone, after extraction, was lost in the perirenal tissues.

There is a theoretical risk in pyelotomy (*see* fig. 10) of cutting a branch of the renal artery which passes over the upper edge of the pelvis to reach its

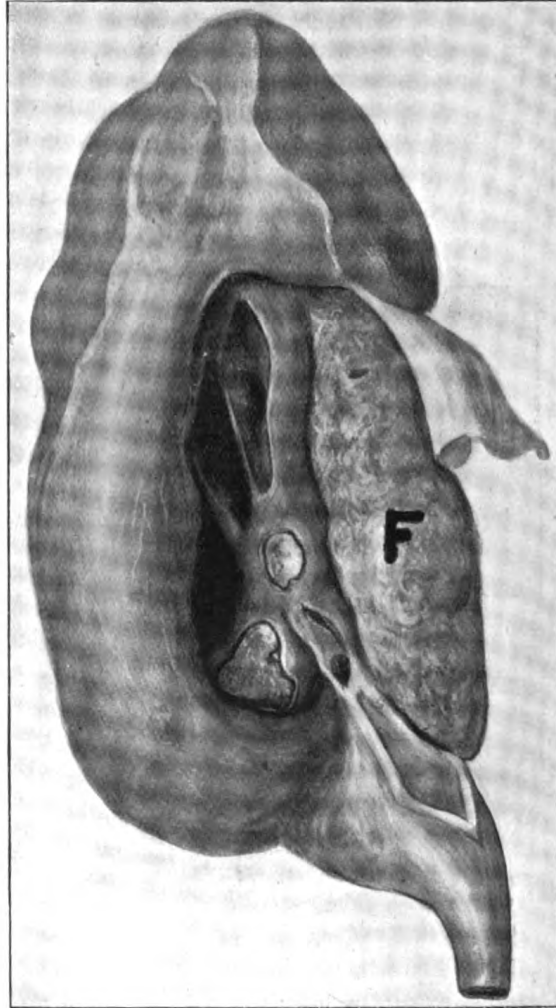


FIG. 11.—(No. 3618 Path. Series, R.C.S.) A kidney the pelvis of which is nearly filled with a calculus. The pelvis was surrounded by a large quantity of fat F, which has been dissected away from one side of it.

The calyces are dilated and the substance of the kidney is so much atrophied that some of the dilated calyces nearly reach its surface. The surface is granulated, and, in some situations, seamed and drawn in as if cicatrized after ulceration or loss of substance.

Note that all the branches of the pelvis lie within the cavity of the renal sinus, and that a subcapsular pyelotomy would open the main pelvic cavity.

posterior surface, but in ten years I have had no trouble from this source. I believe that reflection of the fibrous capsule in the subcapsular operation must push this artery out of danger—an additional advantage of the method. I have avoided carrying the incision in the pelvis right to its upper border, so as to

minimize this possible risk. The re-suture of the capsular flap secures efficient hæmostasis of any small vessel in the divided edges of the pelvis.

It might be thought that the operation of pyelotomy would be difficult or impossible in cases of compound branching pelvis. It will be found, however, that the branches of such a pelvis lie right within the sinus of the kidney and that an incision made in the way directed, just in the line of the hilum, enters the main cavity even of a compound branching pelvis. A specimen from the Royal College of Surgeons Museum (fig. 11) illustrates this point. I may add that I have not yet failed to find the cavity of the pelvis when it is incised in the way described.

The suture of an incision in a compound branching pelvis might well prove to be impossible without producing a dangerous stricture of its cavity. But if the subcapsular method of incision be adopted the problem does not arise.



FIG. 12.—(No. 241.2, Physiological Series, R.C.S. Museum.) Kidney with compound branching pelvis. A human kidney, injected and divided longitudinally to show the chief points in the structure of the kidney with pelvis of the multiple type.

The ureter dilates a short distance external to the hilum to form a common pelvis which divides within the "sinus" to form two main branches that in turn break up into minor branches or infundibula each related to one or occasionally to two mammillæ, through which the uriniferous collecting tubes of a pyramid open.

The blood-vessels pass through the hilum into the sinus and enter the kidney substance in the interspaces between the infundibula of the pelvis. The fat in which in nature the vessels are embedded has been removed. The separation of the kidney into cortex and medulla is clearly marked by the rich arterial supply to the cortex and by the radial striation of the medulla due to the direction of the collecting tubes and the position of the veins between them.

In the kidney represented in fig. 12 it is possible that instead of opening the main pelvis two separate infundibula might be entered. But their common attachment to the flap of capsule would ensure the reconstitution of the pelvis and minimize the risk of a subsequent fistula.

Bidigital Exploration of the Kidney.

In cases in which a probe introduced into the calyces fails to detect a calculus known to be present, time should not be wasted in needling the kidney.

The incision in the pelvis should be enlarged cautiously to an extent sufficient to permit the gentle introduction into the pelvis of the gloved forefinger of the left hand in the case of the right kidney, and vice versa. I am aware that some surgeons object to the introduction of the finger into a deep wound as vitiating asepsis. With this objection I can only agree if the word "unnecessary" be inserted before the word "introduction." It is unwise to erect a general principle into a fetish. For certain purposes in surgery the gloved finger gently

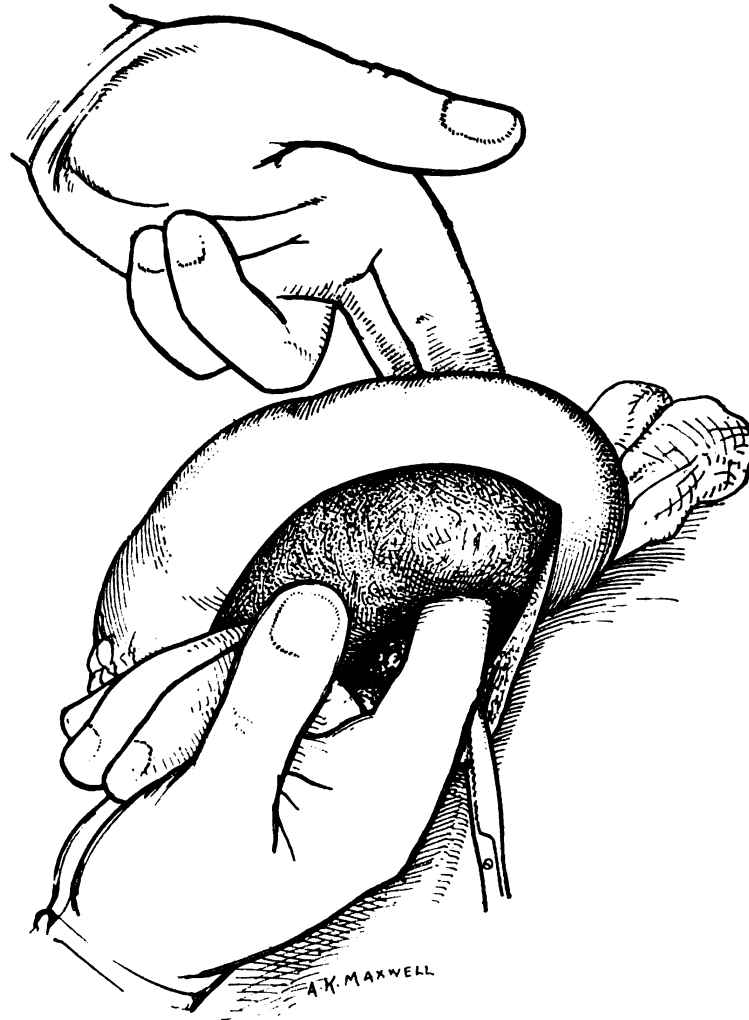


FIG. 13.—Bidigital examination of the kidney.

used, and with due precautions as to asepsis, is not only the safest, but the only instrument which will supply the necessary information, since it is the only instrument the outer surface of which is in direct connection with the observer's brain. When the finger has been insinuated into the pelvis it will go no farther. The calyces are inaccessible to it. They are, however, not palpable. If a finger of the other hand be used to make counter-pressure on the convex surface of the kidney (fig. 13), it is, I think, most unlikely that any small induration

produced by a calculus in a calyx can escape observation. Even if the small calculus be mobile within a cyst, as in one of my cases, a vague induration will be felt by this bidigital method and will invite exploration. In the case of a stone lying in a calyx a gentle impulse given by the internal finger will be felt by the external finger as a definitely localized "lift" imparted to the external finger over a small and well-defined area of the kidney surface.

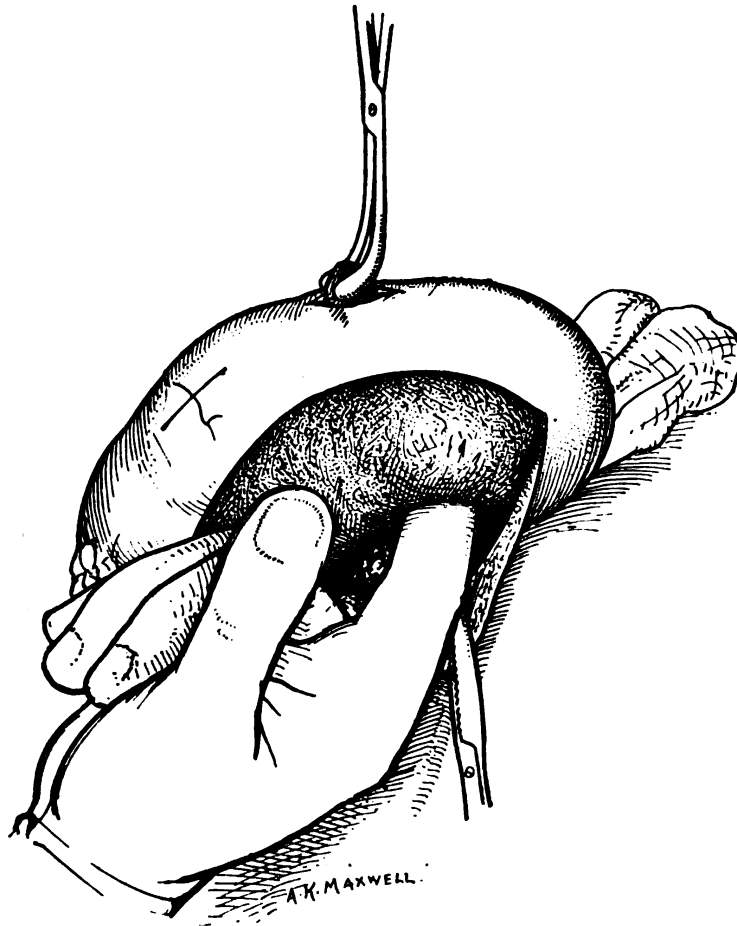


FIG. 14.—Extrusion of a renal calculus through a small incision in the cortex.

Extrusion of Calyx Calculi through the Renal Cortex.

When a stone has been located in a calyx, the finger within the pelvis may be withdrawn and an attempt may be made to seize the calculus by a forceps introduced by way of the pelvis. Frequently, however, a constriction between the calyx and the pelvis will render the attempt unavailing. It is probably better as a rule not to make the attempt, but to retain the internal finger within the pelvis. The finger is used to press the calculus gently towards the cortex and, with the point of a knife, the calculus is cut down upon through the cortex by an incision hardly longer than the diameter of the calculus. This step is

rendered quite easy by the efficient counter-pressure of the internal finger. A little extra pressure and the calculus can often be made to protrude on the surface of the kidney through the small incision, or if this does not happen it is easy to extract it, fixed as it is by the internal finger, by introducing suitable forceps into the little incision, which is subsequently closed with a single catgut suture (fig. 14). Search is then made for other calculi which, if present, are removed in the same way, each through a separate small incision. The efficiency of the search which can thus be made is illustrated by the kidney shown, from which more than a dozen calyx calculi were removed. The operation was a severe one and the case is the only fatal one of my series. After death no residual calculi were found in the kidney.

Thomson-Walker states that "in an exploration of a kidney for a stone which cannot be felt in the pelvis, pyelotomy . . . is looked upon as inferior to nephrotomy. In a single large pelvis (ampullary pelvis) Legueu looks upon the two operations as being equally efficient." If, however, there is a branched pelvis, Thomson-Walker regards nephrotomy as the better operation. It is here that I cannot agree with him for the reasons already stated. At the entrance to the hilum, the line in which a pyelotomy incision is made, it will usually be found that even a branching pelvis presents a single cavity which can be incised sufficiently to admit a finger. The branches of a branched pelvis lie in the deep concavity of the hilum (fig. 11), and from an operative point of view are merely elongated calyces. At any rate I have not yet met with a pelvis which was not amenable to pyelotomy.

I am indeed prepared to admit that in a branched pelvis it might be quite impossible to extract by way of the pelvis, calyx calculi or even calculi in a main branch of the pelvis. This, however, I do not regard as an argument for nephrotomy. The difficulty can be overcome, without risk of hæmorrhage or injury to the kidney, by the adoption of the bidigital method of exploration and by extruding the stones by means of small multiple incisions in the renal cortex. This may indeed be called a limited nephrotomy, but in practice there is the widest difference between this method and the usual splitting of the kidney.

The most complete splitting of the kidney may fail to hit off the plane of one or more of the calyces, so that nephrotomy does not really afford so much security against missing a stone as is given by the bidigital method.

It is usually stated that urinary fistula occurs more frequently after pyelotomy than after nephrolithotomy, a view endorsed by Thomson-Walker. Schmieden's percentage of fistulas after pyelotomy was as high as 22. There would here appear to be a real advantage in favour of nephrotomy, but I believe that pyelotomy by the method I have described to-day is never followed by a urinary fistula. At any rate the complication has not yet occurred in my experience, and it is on this account as much as by reason of its simplicity of technique that I venture to commend subcapsular pyelotomy combined when necessary with bidigital examination and cortical extrusion of calyx calculi, as the best plan of dealing with most cases of renal calculus.

Case Records.

I append the records of three cases. One of them is chosen as a typical example of early unilateral renal calculus—the common type. The other two are cases of bilateral renal calculus already referred to.

Case I.—F. L., female, aged 27, admitted to the Middlesex Hospital under my care on December 15, 1914.

History: She had complained of an intermittent gnawing pain in the right side for two years. An appendectomy had been done without benefit. The pain was worse after exertion, but eased by lying down. Since September, 1914, when she had a severe attack of pain and felt sick, the urine had been much darker. Similar attacks have since recurred twice or three times a week.

On admission the temperature was normal, and the urine free from blood and albumin. The kidneys could not be felt.

Operation: On December 31 the kidney was exposed by a lumbar incision and delivered. The pelvis was opened by subcapsular pyelotomy and a rough stone the size of a large pea was removed from it with a spoon. The capsule was sutured, the kidney returned, and the wound closed. Six days later the urine still contained much blood and albumin. The following day the drainage tube was removed. No urinary discharge had occurred. On the thirteenth day there was no blood in the urine and the patient left the hospital on the twenty-sixth day with the wound soundly healed.

Case II.—Patient, a male, C. L., aged 27, admitted to the Middlesex Hospital under my care on November 17, 1922.

History: About fifteen years previously this patient had a calculus removed from the right kidney. Five months before being admitted to hospital, he had pain in the back on the left side, and noticed that his urine had a reddish tinge. Pain has been continuous, but patient has been able to do his work. The pain is less when he lies down. On October 2 the pain became acute, he vomited the following morning, and the attack passed off. Another attack began on October 10, and lasted until October 14, preventing him from doing his work. On October 15 pain came on again, and has been continuous ever since. The pain has always been in the back, and has never radiated down the thigh or to the testicle.

Condition on admission: Temperature 102°2' F., pulse 112, respiration 24. Patient looks ill, and complains of pain in the loin on the left side. On palpation the kidneys cannot be made out, but there is tenderness on the left side below the twelfth rib, and external to the erector spinæ muscle. There is slight tenderness on the right side in the same region. Urine 1012, acid, contains albumin and blood.

X-ray report: Group of large calculi in right kidney. Two calculi present in left kidney.

Blood urea test report: "Blood urea, 54.0 mg.; protein nitrogen, 53.76 mg. These are dangerously high figures."

Bacteriological report: "Many pus cells. Streptococci or coliform bacilli seen in direct films and grown in cultures."

The acute symptoms slowly subsided with rest in bed, and his temperature became normal.

October 28, 1922: Patient has been sitting up for one to two hours each evening since October 24, and the excretion of urine has gone up from 48 to 76 oz. Was given boric acid 10 gr. t.d.s. from October 24.

October 30, 1922: Blood urea, 28 mg.; non-protein N., 36.5 mg. (normal figures); temperature, normal; pulse 68.72.

November 7: *Staphylococcus aureus* vaccine No. 1, ½ minim given.

November 9: *Staphylococcus aureus* vaccine No. 1, 1 minim given.

November 11: *Staphylococcus aureus* vaccine No. 1, 2 minims given.

November 18, 1922, operation by Mr. Handley: Lumbar incision on the left side. The kidney was delivered partially and with great difficulty; it was found to be greatly enlarged, having a short pedicle and its surface showed lobulation. The capsule was incised in the posterior surface near the pelvis, but it was not found possible to do a subcapsular pyelotomy on account of the shortness of the pedicle and the elongation of the patient's thorax. The two stones which could easily be felt in the lower pole were therefore extracted by direct incision of the renal cortex over them; one was the size of a small plum and one the size of a cherry. The opening in the kidney substance was closed with capsule sutures and the capsule was closed. The kidney was then replaced in the abdomen and vaseline gauze packed round the incision in it (on account of tendency to ooze). Wound closed with catgut sutures. A

drainage tube was left in. The end of the vaseline gauze protruded through the incision.

November 14: Blood-stained discharge on gauze dressing this morning; drainage tube shortened and about 8 in. of vaseline gauze removed.

November 15: Remainder of vaseline gauze removed this morning. Blood-stained discharge on dressing. No leakage of urine since; no effervescence is obtained on pouring a solution of sodium hypobromite upon the dressing.

November 16: Dressing soaked with fluid of ammoniacal odour.

November 20: Dressing still soaked with urine. Dressing changed and dry dressing applied.

November 22: Stitches removed. Tube shortened.

November 23: Eusol dressing. Discharge slightly purulent.

November 29: Flavine dressing. Very little discharge and patient getting stronger.

December 7: Some fluid discharged from wound, not smelling of urine. A few days later the wound was healed completely, and the patient rapidly recovered his strength.

December 18: Prepared for operation. Temperature normal in the mornings, rising to 99° F. at night. Pulse 84 as a rule.

December 18, operation by Mr. Handley: Patient was placed on the left side and an incision was made in the right loin. The kidney was exposed and numerous capsular adhesions found and divided. The kidney was withdrawn. One stone could be felt close to its surface at the upper pole and was removed through the cortex, an opening over it being made with Spencer Wells forceps. It was about the size of a walnut. Another stone could be felt through the same opening and was removed in like manner: it was larger than the first. The pelvis of the kidney was now opened under cover of the capsule, and five stones were removed with forceps through this opening. Four other stones were found at the lower pole of the kidney and were removed through the cortex, one finger of the surgeon being in the pelvis of the kidney to push the stone towards the surface. The kidney was irrigated with flavine through the cortical wounds. After this several small stones were removed, and finally one blocking the entrance to the ureter. The cortical incisions were sutured to control the very considerable amount of hæmorrhage that occurred. Wound sutured with catgut and dressings applied.

Patient was returned to bed in fair condition, showing signs of shock. He continued fairly well, but at 3.20 a.m. of the following day he collapsed suddenly, and died. No evidence of external bleeding on the dressings.

Post-mortem: Small amount of blood round right kidney, not sufficient in itself to account for death. No residual stones were found in the kidney. Cerebro-spinal fluid gave no evidence of uræmia. Death was probably due to cardiac failure.

It would have been wiser, I think, to send this patient home for three months before doing the second operation.

Mr. E. C. Dodds, who kindly analysed the calculi, reported that they were composed of calcium oxalate and urate, with an outer coating of sodium phosphate.

Case III.—Mrs. H., a delicate woman, aged about 40, was sent to me by Dr. Joseph White, of Clapham, in July, 1921. In October, 1914, she had an attack of cystitis, and in March, 1915, blood for the first time appeared in the urine. For a fortnight in October, 1917, she had pain in the left kidney. In June, 1921, she noticed hæmaturia without any other symptoms. The urine was smoky rather than bright, and she did not consult a doctor. In July, 1921, she again had pain in the left kidney, with frequent micturition and hæmaturia. The following day she had a temperature of 100° F. and vomiting. She was slightly delirious and complained of headache. On pelvic examination the left ovary was found to be enlarged and very tender, but this swelling may have been connected with the ureter. There was much blood in the urine, and the pain of the day before was now relieved. The signs pointed to a stone in the left ureter now passed into the bladder. There was undue resistance and fullness in the left loin, but

the kidney could not be felt. There was some increased resistance of the left anterior abdominal wall, but none of the muscles in the left loin. A skiagram of both kidneys and the pelvis was advised. The X-ray photographs, taken by Mr. Coldwell on September 16, 1921, showed a large calculus in the left kidney lying low down close to the ilium. In the left ureter, just below the sacro-iliac joint, there appeared to be a small calculus. In the right kidney there were two calculi—one large, and a small one lower down.

It was decided to operate on October 21, 1921, upon the left kidney first. The kidney was much enlarged. It was cut down upon in the loin, and the pelvis was opened by the subcapsular method. A large stone, firmly adherent to the kidney substance, was abstracted from the lower end of the pelvis. The skin incision was now prolonged downwards and inwards, and search was made for the stone in the ureter below the rim of the pelvis, but it could not be found.

It appears likely that the ureteric calculus had passed five days previously to the operation, on the day when a second skiagram was taken, which showed the ureteric calculus still in the ureter. On that day, on returning home, the patient became "frightfully ill" with sickness and pain in the lower iliac region at 4 p.m., and at 7 p.m. the pain suddenly disappeared. These facts were only elicited after the operation.

When the operation was over there was no escape of urine from the lumbar wound, although a urinary smell could be detected on the first day. There was some pyrexia reaching to 102° F. on the third and fourth nights, but this subsequently subsided. Slight hæmaturia on the first day only. The wound healed without suppuration, and the pain was relieved. On October 5 the left kidney was decidedly smaller, and the patient's condition was very satisfactory.

In September, 1922, it was decided to operate upon the right kidney. An incision was made from the angle between the twelfth rib and the erector spinæ downwards and forwards towards the anterior superior spine. The kidney was brought to the surface, and the posterior surface of the capsule was incised for about 3 in. near the convex border of the kidney, and the pelvis was opened with the point of a scalpel cautiously introduced beneath the capsule flap. The opening was then enlarged with a pair of forceps until a curved pair of forceps could be introduced, which easily gripped a large stone free in the pelvis. A sound was then introduced into the calyces in all directions but failed to find any sign of another stone. No induration could be felt in the kidney substance on palpation. A gloved finger was now carefully introduced into the pelvis, but still no stone could be felt. However, upon bimanual palpation between the finger in the pelvis and an external finger, two areas of slight induration corresponding to the two small stones in the radiograph (fig. 1) could be detected in the substance of the kidney. Short incisions about an inch long, through the kidney substance at its convex border, entered a cystic cavity at both points where induration was felt. Slightly turbid fluid escaped from the cysts, which were evidently not in communication with the pelvis of the kidney. Two small calculi were found severally free in the interior of each cyst. The upper calculus was of recent origin, for it was not shown in the skiagram of the right kidney taken eight months before the operation. The kidney substance was sewn up, the capsular flap was replaced in position and fixed by two or three stitches, a small drainage tube was introduced, and the skin incision sutured. No urinary discharge occurred during convalescence. A little pyrexia was present for a few days, and subsequently the patient made an uninterrupted recovery.

In July, 1922, the patient's husband reported that she was "marvellously well."

DISCUSSION.

Mr. FRANK KIDD said that the question of the pathology and technique of the removal of stones from the kidney was one of great complexity and difficulty, so that only the fringe of the subject could be touched upon at a discussion like this. For instance, an infected kidney gave rise to stones of a peculiar character and had then to be approached and treated in a very different manner to a stone-containing kidney which was not infected; and again, wisdom might dictate a nephrectomy rather than a nephrolithotomy in difficult unilateral cases and might not only be an operation attended with less risks, but might also ensure freedom from recurrence.

Confining his remarks to the question of stones lying in a non-infected kidney he emphasized the point that complete splitting open of the kidney as at a post-mortem examination was a highly dangerous procedure and one that should seldom, if ever, be carried out. If a stone was so large and branched as to necessitate the splitting of the whole kidney it often meant that nephrectomy was preferable to nephrolithotomy, nephrectomy being a less dangerous procedure and ensuring freedom from relapse. What was the danger of splitting the kidney? It was hæmorrhage. Immediate hæmorrhage from the slitting of large arteries might be so severe that it could not be controlled, with the result that the kidney had to be sacrificed straight away. On the other hand, it might be possible to control the bleeding by catgut stitches and yet on the eighth to the tenth day the kidney digested these stitches and severe secondary hæmorrhage occurred, for which nephrectomy in a hurry was the only remedy if life was to be saved. He had had only one such case in his own practice—a patient with a very septic kidney operated on in a military hospital under poor conditions—for he had hardly ever needed to split the kidney, and indeed had carefully avoided doing so as he knew the dangers. But he had noticed in hospital records that quite a number of cases of secondary hæmorrhage occurred, following nephrolithotomy, at the general hospitals. He had pointed out the dangers of splitting the kidney in an article he wrote for Treves's "Surgery," giving diagrams of the arrangement of the blood-vessels. Nevertheless this did not mean that pyelolithotomy should be adopted as a routine procedure. It also had its dangers of hæmorrhage, as he would show. Whenever possible one should endeavour to make an incision through the pelvis and remove a stone or stones through that incision as it was the safest and simplest method of extracting stones of moderate size from the kidney, but one should not make an exclusive practice of this procedure. If there was a single nose-stone lying in the pelvis itself this should be lifted out at once without chipping bits off. The opening in the pelvis should be explored by the gloved little finger, which was better than any probe, and the whole kidney and every calyx palpated in turn bimanually. If further stones were known to be present and were detected gripped in the neck of a calyx it would be found in nearly every case that the cortex was thinned out into a cyst superficially to these stones, in other words a local hydronephrosis of a single calyx. For many years he had taught that knowledge of this fact of the thinning of the cortex over a stone in a calyx was the key to successful and conservative stone surgery. It was a very simple matter to make a small incision through the thin cortex when a dimple could be felt over each stone and to pull out each stone through this. The incision need not be more than half an inch long, so that there would be no serious bleeding. Each wound was stitched up with catgut and neither leaked urine nor gave rise to secondary hæmorrhage. This procedure was far less dangerous than to attempt to drag a stone through the narrow neck of a calyx. Large arteries and veins usually surrounded the necks of calyces, and by the dragging of a stone through the narrow neck these vessels became liable to be torn, and bits of stone were also chipped off and left behind. By thus combining pyelotomy with small incisions over individual stones through the thinned cortex it was usually possible to avoid gross splitting open of the kidney.

If it was decided the kidney should not be sacrificed and yet that it should be split to get out a large branched stone, in the first place the pedicle should be temporarily controlled, and in the second place an incision should be made through the "bloodless

line" of Brodel which he (Mr. Kidd) had described in Treves's "Surgery," and not through the outer border of the kidney as performed in the post-mortem room. The stone should be grasped with the greatest gentleness so as to avoid fragments being left behind. If bits of stone were chipped off the kidney should be thoroughly washed out with several pints of lotion until the operator was satisfied that no chips were left behind. In all cases a bougie should be passed down the ureter as far as the bladder.

Every case of stone in the kidney or kidneys should be made a study in itself. It was not possible to lay down universal rules. Judgment and experience were the keys to success. X-ray pictures should be taken at several days' intervals on at least two occasions in every case. No case should ever be operated on after one X-ray examination only, nor should any case be operated on simply because the X-rays showed up a stone. Adequate indications such as undue pain or evidence of renal damage must be present to warrant an operation. With good X-ray pictures it was usually possible to map out the number and position of all the stones present and to make up one's mind as to the type of operation likely to be required. The final decision however should not be made until the kidney had been exposed and was in the hand of the operator. It was always advisable to get the kidney right out on to the loin before exploring it for stone, as in this position it could be better explored, and hæmorrhage, if it arose, could be better controlled. This was more often possible through the type of posterior incision he (Mr. Kidd) employed than through the lateral incision usually adopted in this country. The technique of the operation was then decided upon after a combined study of the X-ray plates and of the kidney in hand.

He (Mr. Kidd) had lately returned from America, where he had had the opportunity of seeing the Mayos at work. Off and on for many years kidney operators had endeavoured to apply X-rays by means of the fluorescent screen to kidneys at operation, but so far with little success. He (Mr. Kidd) was interested to see that the Mayos were now employing a new type of machine with a small Coolidge tube by means of which they were able to screen the kidney exposed at operation. The experience of the Mayos had been that a great many stones were left behind in the kidney. They stated that if the kidney was cleared of stones completely relapse was not likely. They showed one kidney which was supposed to have been cleared of stones, but the X-ray investigator maintained that a small stone was still present. The kidney was eventually taken out and was opened up by a pathologist, who stated that he could not find any stone. The kidney was X-rayed again and the radiographer stated the stone was still present. The kidney was then sliced up into small pieces and the stone was found. The experience just recorded showed the difficulty of making sure at operation that all stones had been removed and he thought that very shortly it would be necessary to employ an X-ray apparatus at any operation for stone in the kidney. As yet the apparatus was rather dangerous to the operator, but it was being perfected, and he (Mr. Kidd) hoped to make use of it shortly.

The risk of fistula after pyelotomy was a bugbear based on the experiences of the early operators. Personally, he had never seen such a complication, and he had carried out more than one hundred pyelotomies. Fistula could only occur if little pieces of stone were chipped off and left behind, preventing natural drainage of urine from the stitched pelvis. There need, therefore, be no fear of doing a pyelotomy because of the risk of a fistula.

During the last year he had operated on four cases of bilateral renal calculi and in each case he had done both kidneys at one sitting. It always seemed to him very hard on patients with bilateral calculi that they should have to undergo two operations. In the first place a large number of cases with bilateral calculi did not need any operation at all. Cases of bilateral calculi should not be operated on simply because the X-ray pictures showed stones, they should only be operated on if they had urgent or dangerous symptoms. If he considered operation necessary he preferred to operate on both kidneys at one sitting. He found he could deal with both kidneys in about an hour and twenty minutes, the patient lying on the face so that there was no need to move him between the two operations. In cases in which he had done this the patients had had no more shock than after an ordinary operation and had done extremely well.

Surgeons should not undertake to operate on stones in the kidney unless they had made a special study of the minute anatomy of the pelvis and calyces of the kidney, and the relation to them of the arteries and veins. A good knowledge was also necessary of the numerous common types of abnormality of these structures.

Finally, he would warn them against being too ready to adopt Mr. Sampson Handley's line of incision in pyelotomy. He understood Mr. Sampson Handley had only carried out this particular operation in about nine or ten cases. If he were to do it in one hundred consecutive cases in quite a number he would strike a large aberrant artery which ran down from the renal artery posteriorly to the pelvis. Very often it would be just under the kidney substance in the hilum of the kidney. It was not always an easy matter to correct a wound of this artery, and the necessary ligatures cut off the blood supply of a large area of kidney substance. For that reason he thought that the line of incision Mr. Sampson Handley recommended should not be widely adopted. It was much safer to make an incision from the ureter longitudinally upwards, and then, if desired to enlarge the incision, to carry it outwards and downwards into the kidney substance for a short way so as to avoid the artery he had mentioned. In that way quite a large opening could be made. Incisions in the renal pelvis should always be sutured, if possible. It only took a few minutes to suture them and then cover them with a flap of fat; nevertheless, if inexperienced operators found difficulty or need arose for rapid closing of the wound then it was safe to leave them unsutured; but the wounds took longer to heal up and were more liable to septic complications. He always expected to get first-intention healing without any leak of urine at any time from his pyelotomy wounds. Nevertheless it was a wise precaution to leave a small drain in the wound for a few days.

Mr. J. SWIFT JOLY said that he endeavoured in each case to select the operation which would do least damage to the kidney. No rigid rule could be prescribed as to the relative merits of nephrolithotomy and pyelolithotomy. Much depended, to begin with, upon the patient's physique. In a fat person, with a thick abdominal wall, the kidney perhaps could not be got sufficiently out of the loin to enable pyelolithotomy to be done with safety. Again, the relative size of the stone to the area of the renal pelvis was a factor to be taken into account. If a stone was definitely in the renal pelvis not extending up into the calyces a pyelolithotomy could almost always be done, providing the kidney could be brought out of the loin. There was no real limit to the size of the true pelvic stone which could be got out through the pelvis. Recently he had removed two stones from a kidney pelvis through a pelvic incision, the larger stone being about the size of a hen's egg and the smaller about the size of a cherry; but the patient had a dilated pelvis, and it was easy to get them out. But when, owing to branching, the stone involved calyx as well as pelvis it was not always possible to get it out through the pelvis, and to attempt that operation was to do more damage to the kidney than would be done by making a clean cut through the kidney itself. When there were stones both in the pelvis and in the calyces the line of conduct would be governed chiefly by the amount of dilatation of the pelvic-calyx system. If the kidney was dilated and the pelvis and all the calyces were dilated, these stones could be got out through the pelvis quite comfortably; but if, as often happened, though the pelvis was dilated, there was a very narrow opening between pelvis and dilated calyces, he thought it wrong to try to enlarge that opening either by dilatation or by cutting through the pelvis. It was much safer to cut through the cortex in such cases, and while taking the pelvic stone out through the renal pelvis to take the smaller stones out by way of the cortex. He went on to consider the methods of exploring the kidney in those uncertain cases in which the X-rays showed a small shadow, whose exact position was uncertain, and nothing was very palpable. Generally the X-rays gave a good idea of the locality of the stone, and this greatly simplified the operative technique. But if quite in the dark as to where the stone was the rule he followed was first to look at the renal pelvis. With a big pelvis, extending well out from the kidney, he opened it and passed in his little finger and explored the calyces one by one. If he could find nothing there he made an incision over the lower third of the kidney and cut down on the lower calyx.

One was given a much more direct line of action from the cortex downwards than from the pelvis upwards. This plan of exploration could be continued, but it was rarely necessary nowadays, for usually the surgeon was able to have a map of the position before he started.

Mr. R. H. JOCELYN SWAN said that it was usually taught that renal calculi were only formed in the renal calyces, but he had shown a case and operated upon it before a meeting of the Section of Urology in which a calculus was firmly embedded in the cortical substance of the kidney. This case was also of interest from the position of the calculus in the radiograms; in the antero-posterior view the shadow seemed to be too lateral to be in the kidney and suggested a gall-stone, but in the radiograph taken in the lateral plane, the shadow was opposite the neural arches of the vertebræ and not opposite the centre of the vertebral bodies as was usual with renal stones.

With regard to the operation to be performed for renal stone, he thought that each individual case must be separately considered. He much preferred to extract a stone through an incision in the posterior wall of the pelvis if this were possible rather than through any incision in the renal substance, and it was surprising how large a stone could be removed in this way. In some cases he had enlarged the incision upwards, actually incising the margin of kidney overhanging the pelvis, any bleeding being easily controlled by suture. In some cases in stout patients or with very large or branched calculi direct incision had to be made in the renal cortex, or again when a second operation had to be performed for stone on a kidney previously operated upon. Most cases were obvious when the kidney was exposed, when the best means of approach to the stone could be determined, but he would always attempt a pyelolithotomy if the conditions permitted. He had experienced difficulty with small calculi in a long calyx; in these cases he advised that an opening should be made in the pelvis and the little finger introduced to feel the stone. Pressure could then be made on the latter towards the renal surface, when, the calculus being maintained in position with the finger in the pelvis, it could be easily removed by a very small incision in the renal tissue.

As stated by another speaker he had a dread of secondary hæmorrhage occurring after a nephrotomy incision and had had on more than one occasion to remove the kidney as a life-saving measure for severe hæmorrhage after this operation. In one case under a colleague, severe hæmorrhage had occurred after a nephrolithotomy and upon the evacuation of the clot from the bladder a catgut suture used in sewing the renal cortex came away. He thought that Mr. Handley's operation was ingenious, but it would be applicable only in those cases in which pyelolithotomy could be done or where the posterior surface of the kidney could be freely exposed. He was sorry to think that Mr. Handley had recommended it as a safeguard against fistula resulting from a pyelolithotomy, as in his (Mr. Swan's) experience this was rare, in fact in his own series of cases he had not seen it. He did not think it even necessary to do the elaborate suture of the renal pelvis or to turn the fascial covering over the incision as he was advised by Sir John Thomson-Walker, but he was content with a few sutures or even one fine suture to approximate the edges of the incision in the pelvis. He had more than once left the incision unsutured, after ureterolithotomy for instance, without any fistula resulting.

Professor HOBDAY stated that calculi of the urinary tract were of comparatively common occurrence in animals, and although found in all varieties were especially met with by the veterinary practitioner in the long-lived species such as the horse and the dog. They were, however, occasionally met with in the sheep, the cow, and the pig, but generally found by the meat inspector in the slaughter-house. In the horse he had met with instances in which the calculus had been of such a size that nothing but the capsule of the kidney was left, and he illustrated jagged shapes which some of these calculi took by showing on the screen, by means of the epidiascope, several sketches taken from actual specimens in the kidney of the dog.

Mr. HANDLEY (in reply) said he was pleased to find that a large measure of agreement existed as to the superiority of pyelotomy over pyelolithotomy save in exceptional

cases. He hoped that Mr. Frank Kidd would give a trial to the subcapsular method, for the danger of wounding the posterior branch of the renal artery appeared to be a theoretical one. He believed that during the elevation of the capsule this arterial branch was pushed out of danger with the capsule. Mr. Swift Joly's remarks confirmed him in the view that it was usually best to extrude calyx calculi through the cortex with a finger in the pelvis, rather than to attempt their extraction through the pelvis. He was very interested in Mr. Jocelyn Swan's case, in which a calculus was found imbedded in the cortex. Evidently a calculus might originate in the cortex in at least two ways, either in its solid substance or in a cyst. Professor Hobday stated that calculi were more frequent in long-lived animals. If so, they should be common in the crocodile; he (Mr. Handley) understood that this reptile might live for one hundred and fifty years.

Section of Surgery.

President—Mr. JAMES BERRY, F.R.C.S.

A Case in which an Adenoma weighing 2 lb. 3 oz. was successfully removed from the Liver: with Remarks on the Subject of Partial Hepatectomy.

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THE title of this note, for which I alone am responsible, is not sufficiently explicit. A statement of the weight of the tumour does not suggest the principal feature of the case. It ought to have been made clear that the tumour was sessile, and could only be removed by incisions carried far into the substance of the liver and that it had unusual pathological features. Even the weight is by no means a record. Peck¹ recently recorded a case of angioma which he removed, weighing 3 lb. 14 oz., and refers to one operated upon by Pfannenstiel and reported by Langer, in which the collapsed tumour weighed 5 lb., and was estimated to have been twice that weight when occupied by blood.

My patient was a boy, I. A. (Reg. No. 18496), aged 13, the only child of middle-aged parents and somewhat precocious. He was referred to me by Dr. Christian Kapp, of Gateshead, and admitted to the Newcastle-upon-Tyne Royal Victoria Infirmary under my care on February 26, 1921, complaining of a lump in the right side of the abdomen. This was first noticed after an attack of pain which had come on suddenly whilst doing his homework ten days previously. Up till this time, he had never suffered from abdominal pain and had enjoyed good health, playing cricket and football, and taking part in the school gymnasium. The pain was at first very severe and "doubled him up," so much so that when he walked, he had to bend forward to ease it. There was no vomiting. The severity of the pain diminished during the same evening and he was able to sleep, but it was present more or less until four days before coming into hospital when it went away gradually so that he was free from pain on admission. His appetite had not been impaired during his illness, and the bowels had been regular, there being no alteration in the colour of his stools. There was no difficulty with his "water," he had never noticed blood in it, and only on one occasion had it been cloudy.

Condition on admission: The patient stated that he felt quite well, but had been told that a tumour was present. He was a normal looking boy with dark hair, pale face, and sallow complexion, and of about 5 st. in weight. The temperature was 98° F. and the pulse-rate 80 per minute. The respiratory and cardiovascular systems were normal. The urine was acid, contained a trace of albumin, and showed a deposit of urates and phosphates. On examination a large lobulated mass could be seen and was readily palpated on the right side of the abdomen, just below the costal margin

44 Grey Turner: *Adenoma removed from the Liver*

and extending almost to the middle line in front and as low as the level of the umbilicus. On bimanual examination, the mass appeared to extend from the right kidney behind to the abdominal wall in front, and could be moved slightly antero-posteriorly in a "to and fro" manner. On percussion, there was dullness over this mass continuous with that of the liver above and of the kidney behind. There was no tenderness, no other tumours were discovered, ascites was absent and there was no evidence of dissemination. Neither cystoscopic examination nor X-ray were employed.

The symptoms, physical signs and negative evidence led to the diagnosis of a rapidly growing unilateral renal sarcoma—a condition which is recognized as occurring in young subjects with but few symptoms and signs beyond the presence of a mass.

An exploratory operation was decided upon, and this was performed on March 1, 1921. General anæsthesia was induced by chloroform, and maintained with "open ether."

A vertical incision was made through the outer third of the right rectus abdominis muscle, extending from the costal margin to about an inch below the level of the umbilicus. On opening the peritoneum, a large lobulated tumour was at once seen apparently arising from the right lobe of the liver. It was a matter of remark that there was no ascites. The tumour was covered by the omentum which was adherent all over its anterior surface and to the parietes. These adhesions carried some enormous vessels, and in order to make fuller investigation, it was necessary to ligature and divide them. This enabled the mass to be partially delivered from the wound. It was then found to consist of a large tumour occupying almost one half of the right lobe of the liver. The left extremity of the tumour reached as far as the notch for the gall-bladder, but this viscus was not involved. The remainder of the organ appeared to be normal, and nothing suggestive of a primary growth would be found in the abdominal cavity, so that the condition was regarded as being of a purely local nature.

The mass appeared to be too limited to one region of the liver to be merely a cirrhotic change, it had not the appearances suggestive of a gumma, and in the absence of a primary growth, and considering the age of the patient, it was thought to be possibly of a simple nature. While the matter was under consideration the report arrived on a frozen section which had been quickly made from a small portion of the mass removed earlier in the operation; this stated that it was an unusual type of tumour composed of liver cells and probably not of a very malignant nature. On these grounds it was decided to attempt its removal.

A critical examination showed that this could be carried out by a V-shaped resection, with the apex just opposite the commencement of the cystic duct, but without encroaching on the hilus. The gall-bladder had to be removed with the tumour and the isolation and ligature of the cystic duct and vessels was first carried out. The affected portion of the liver was drawn out of the incision as far as possible, and the stomach and colon were well packed away with large gauze pads.

A light, bow-shaped stomach clamp with jaws 4 in. long, such as I use for gastro-enterostomy, was applied on the tumour side of the proposed incision and was slowly tightened until it had a firm hold. This provided a convenient handle and helped materially in the subsequent manipulations. With a large fully curved intestinal needle threaded with No. 3 chromic catgut, a series of sutures were then introduced into the liver substance on the proximal side of the proposed incision and parallel to it. These sutures were passed as deeply as possible into the liver tissue and almost reached the under surface. Four were required: each was locked to its fellow and they were all separately tied. The liver substance was then gradually incised between the line of sutures and the clamp. A sharp knife aided by scissors was used, there was some hæmorrhage, but it was not alarming.

Branches of the hepatic artery were recognized either before or just at their division, by the fibrous capsule accompanying them. Three or four of these branches were caught in ordinary artery forceps and were subsequently ligatured with fine catgut. Some venous oozing from portions of the liver which had escaped the hæmostatic sutures was easily controlled by the pressure of a gauze swab. An

exactly similar proceeding was carried out on the right side of the tumour, but there was not so much tissue to be divided, that side of the V not being quite so long. After the incisions were made, the parts fell asunder, and left a very large formidable looking gaping wound. It was found that the sides of this gap could be easily approximated, and it was closed by a series of four catgut sutures passed with the same needle on the proximal side of the hæmostatic sutures which had been first

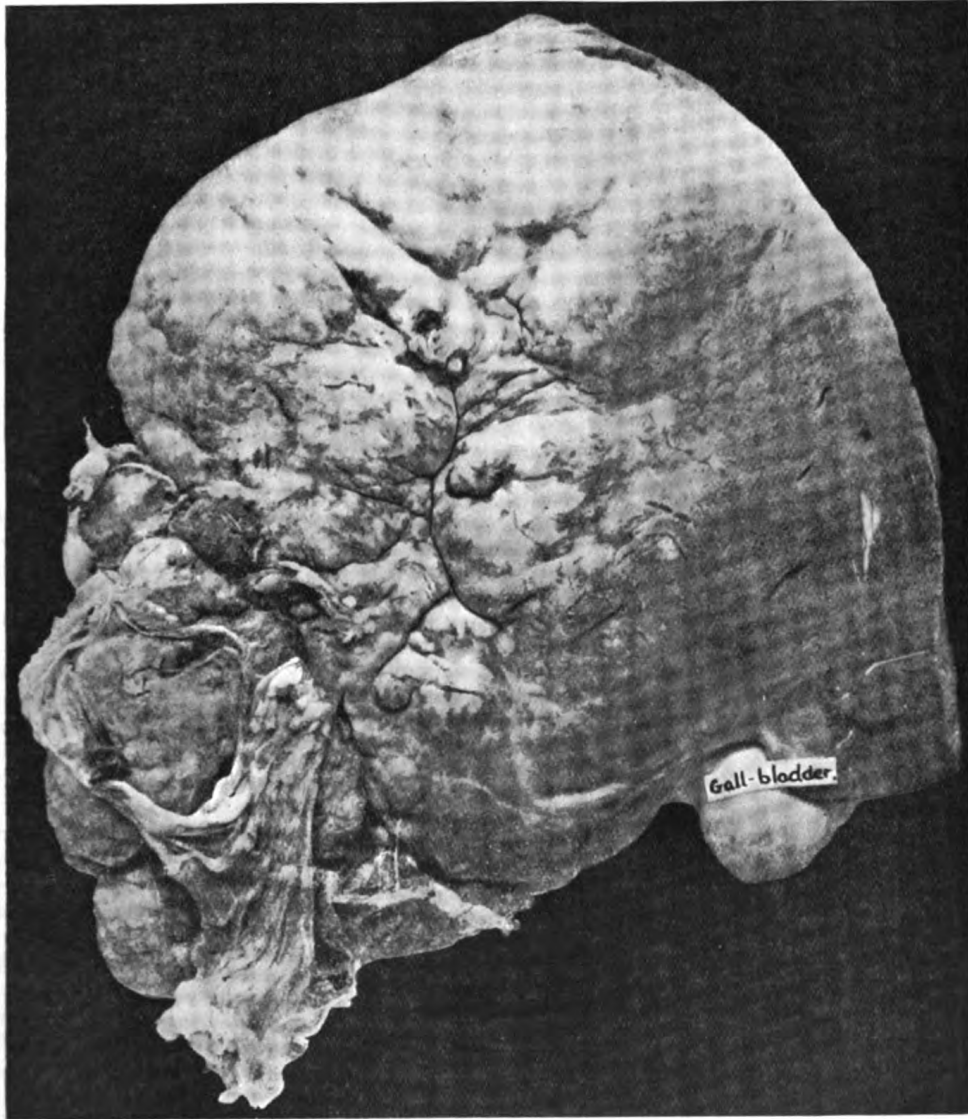


FIG. 1.—The portion of liver containing the tumour excised in the case of J. A. The whole weighed 2 lb. 3 oz. Remains of omental adhesions are well shown.

introduced. This brought the surfaces well together, but a few additional interrupted sutures were required for the exact approximation of the edges.

After the completion of the suturing, there was still some oozing from the under surface of the posterior part of the liver wound. This might have been controlled by further stitches but it was thought wiser not to prolong the operation, and therefore

gauze was packed over the area and was protected from contact with the hollow viscera by a strand of rubber tissue. A small tube was brought from the stump of the cystic duct as is my routine in cholecystectomy.

It is difficult for me to give a word picture of all that happens at an operation of this sort. The conditions are highly unusual, and experience is lacking as to the effect of such a proceeding on the patient, and there is naturally rather an artificial atmosphere of apprehension and excitement. I had one assistant, a student house surgeon (Mr. George Mason who is present this evening) to whom I was accustomed, and in whom I had complete confidence, but my own anæsthetist was unable to be present.

The operation lasted about an hour and a half, and in the aggregate there must have been a considerable blood loss. Towards the end of the operation, the blood-pressure had fallen considerably, and there were other evidences of grave shock which caused us anxiety. It was therefore considered wise to introduce 10 oz. of gum saline solution (Bayliss) into the veins, and this was followed by considerable improvement. While the infusion was being carried out, the abdomen was closed with a continuous catgut suture for the peritoneum and through-and-through silkworm sutures passed figure of eight fashion for the other layers. The patient left the table in quite good condition. Six hours later, the gum solution was repeated.

After the initial shock was overcome, recovery was uneventful except for the occurrence of a little sepsis along the track of the gauze, which cleared up after the removal of the latter on the fourth day.

The patient left the hospital twenty-four days after the operation in good condition and with the wound healed except for a small granulating area at the drainage tract. He seemingly suffered very little from the removal of such a large portion of an important organ.

Pathological.—The specimen is preserved in the Museum of the University of Durham College of Medicine, and the photograph was kindly made for me by Mr. H. B. L. Dixon. The part removed (fig. 1) was mainly composed of tumour growth but included a narrow strip of liver tissue covering the posterior and left lateral surfaces of the growth and the gall-bladder. The whole weighed 2 lb. 3 oz. (35 oz.). (the normal weight of the liver in the adult is from 50 to 60 oz. and in a boy aged 13, about 40 oz.). The surface of the growth is lobulated and irregular as the result of deep branching furrows which traverse its surface. The lower half is also covered with the remains of adherent omentum. There is no definite capsule and the tumour appears to be directly invading the liver substance, though the line of demarcation is plainly seen. The predominant colour is a pale yellowish green, but there are areas of hæmorrhagic staining here and there.

The tumour has been very carefully examined by Dr. A. F. Bernard Shaw of the Pathological Department of the University of Durham College of Medicine, and a detailed account of his investigations will be published in the next issue of the *Journal of Pathology*. A summary of the microscopic characteristics is as follows: "The appearances here are those of a true liver-cell tumour of unusual type. While the tumour cells are obviously of hepatic origin, they are atypical in structure and arrangement. There is evidence of both direct and mitotic division and multi-nucleated forms are seen. Though the tumour cells are actively secreting bile, no bile-ducts are found in the substance of the growth. No evidence of tumour cell emboli were seen either in the vessels in the growth nor in those of the adjoining liver tissue. Sections of the lymph gland, obtained from near the gall-bladder, show no metastases. On the whole the appearances point to an unusual type of hepatic adenoma in which however the cells are more aberrant than it is usual to find in an adenoma of simple type."

The drawing of the microscopic appearances (fig. 2) was made from a section at the junction of tumour and normal liver tissue.

The boy returned on October 28, 1921, for examination, and stated that "he had never felt so well before," and was now regularly attending school, although not playing games. He looked better in every way, and the incision was quite healed, showing however a little keloid change. The right lobe of the liver was not palpable.

but the left was easily felt slightly below its normal level. A radiograph of the abdomen (without oxygen injection) showed what appeared to be a diminution in size of the liver shadow on the right side. Dr. Crawford of the Pathological Department examined the fæces and urine, and reported as follows: Urine: Specific gravity 1027; reaction, acid; albumin, *nil*; bile, *nil*; diastase, 20 units; urea, 3 per cent.; deposit, a few crystals of calcium oxalate. Fæces: Bulky brown stool; reaction, alkaline—urobilin content, normal; microscopic examination shows a few soap crystals, but otherwise nothing abnormal. Thus the examination did not suggest any interference with the hepatic function.



FIG. 2.—The microscopic appearances at the junction of the tumour with the normal liver substance.

The patient was seen again in January, 1923, one year and ten months after the operation. He was in excellent health, working regularly in an office, and without complaint of any kind whatever. He has developed very much since the operation and is heavier (7 st. 12 lb.) and more healthy in appearance than ever before (see fig. 3). Dr. George Clark of the Physiological Department of the University of Durham College of Medicine kindly investigated the hepatic function, making detailed analyses of the urine and fæces on two separate occasions, and the result of the analyses indicates that the liver is functioning normally.

The patient's father refused to allow a sample of blood to be taken for estimation of the blood-urea.

RESULTS OF ANALYSES OF URINE AND FÆCES TWENTY-TWO MONTHS AFTER
HEPATECTOMY.(a) *Urine Analyses.*

(1) Twenty-four hours' sample :			
Urea (in terms of nitrogen)	0.775 grm. per cent.
Ammonia (in terms of nitrogen)	0.048 " "
Amino acids (in terms of nitrogen)	0.017 " "
(2) After dinner sample :			
Urea (in terms of nitrogen)	0.949 " "
Ammonia (in terms of nitrogen)	0.076 " "
Amino acids (in terms of nitrogen)	0.042 " "
(3) Further sample obtained a fortnight later :			
Urea (in terms of nitrogen)	1.072 " "
Ammonia (in terms of nitrogen)	0.074 " "
Amino acids (in terms of nitrogen)	0.0077 " "

(b) *Fat Analyses of Fæces.*

(1) Total fat per 100 grm. dried fæces	32.4 "
(2) Further sample obtained a fortnight later, and after a meal of a known quantity of fatty food.			

No increase of fat in the fæces.

The "lævulose" test (*Lancet*, 1921, ii, p. 1360) is only indicated where there are widespread or acute degenerative changes, and was not employed.

At this stage, I will briefly refer to the only other case in which I have had occasion to resect any considerable portion of the liver:—

The patient (Reg. No. 4885) was a woman, aged 33, who was admitted to the hospital in October, 1911. For twelve months, she had complained of pain in the left side of the upper abdomen, followed by the development of a "lump." The pain came on in attacks, often just after food, and she stated that the lump got larger during a seizure. She had never vomited. The patient did her usual household duties until six weeks before admission.

On admission she was found to be a thin, anæmic looking woman, the mother of five healthy children, but had thrice miscarried. In the right hypochondrium there was a large pear-shaped swelling extending downwards from the costal margin towards the umbilicus and movable from side to side. It therefore resembled a distended gall-bladder, and that was the diagnosis provisionally made. At the operation it proved to be a localized growth springing from the left lobe of the liver. It was suspiciously like a malignant tumour, and as there were no other deposits and no signs of a mother growth, it was looked upon as a primary carcinoma of the liver. After careful examination, it was decided that it ought to be removed, and this operation was carried out. The notes state that "the liver was clamped, divided and sutured with catgut. There was a little oozing, for which a gauze strand was left *in situ* and was removed four days later." I much regret that the record is not more explicit and that I have no clearer recollection of the exact technique employed, but fortunately, I do vividly remember that the large tumour was removed and the gap in the liver repaired without much difficulty and that the patient made a smooth and satisfactory recovery, leaving the hospital three weeks after operation.

The mass was the size of a closed fist and weighed 8½ oz. (fig. 4). Its surface was nodular, puckered and inflamed, and there were some tags of adhesions about it. The cut surface showed an appearance at once suggestive of gumma. The centre was caseating, but the periphery was of solid pearl-coloured nature. A lymphatic gland from the small omentum presented the same appearance. Microscopic examination confirmed the gummatous nature of the mass. While in hospital a Wassermann test was done, but with a negative result. The patient had a course of antispasmodic treatment after leaving the hospital, but salvarsan was not employed. Since the operation she has had two normal children and one miscarriage, and is now, eleven years later, in good health.

Cases in which removal of a considerable portion of the liver may be looked upon as a legitimate surgical enterprise are few indeed. Up to the end of 1922 I had performed 14,923 operations. Of these 4,935 have been abdominal, with about 550 on the liver and its excretory apparatus, and the

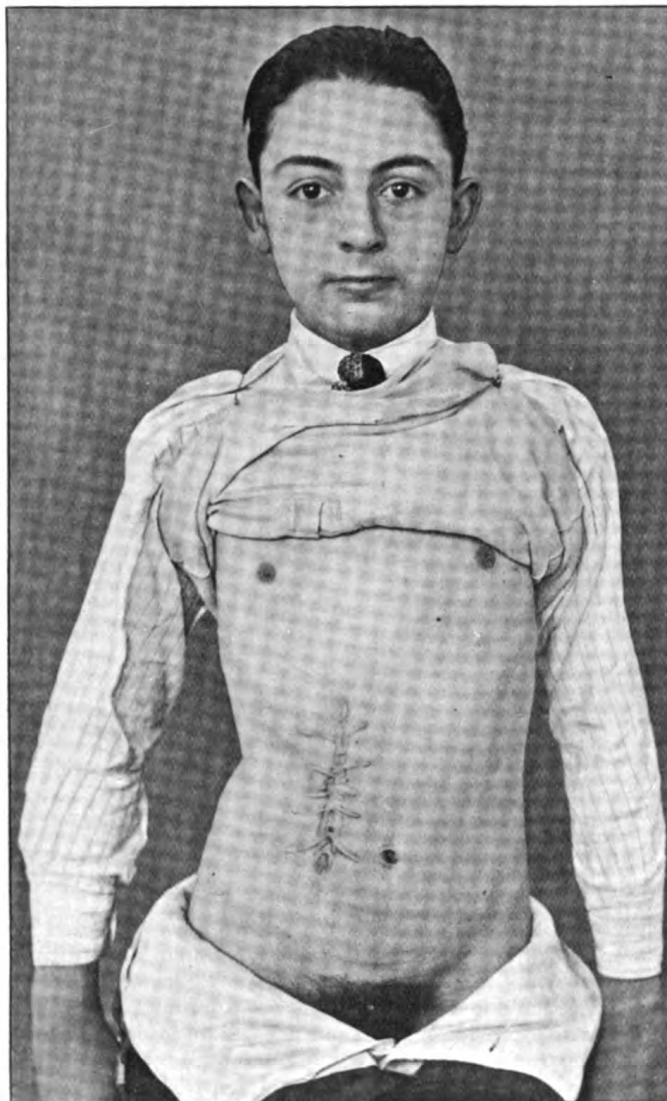


FIG. 3.—Photograph of the patient one year and ten months after the operation. It shows the scar of the incision employed and incidentally the good condition of the boy.

instances of resection of the liver which I have quoted are the only cases in which I have felt it justifiable to attempt this operation. Yet, during the whole of my surgical career I have been alive to the possibilities of partial hepatectomy, for my interest in the subject was first aroused nearly twenty-

50 Grey Turner : *Adenoma removed from the Liver*

five years ago. At that time I assisted the late Dr. George Halliburton Hume, one of the surgeons to the Newcastle Infirmary, at an operation for the removal of a cancerous mass secondary to malignant disease of the stomach.

The patient was a woman, aged 48, on whom Dr. Hume had performed pylorotomy for cancer in June, 1897. She made a good recovery and remained well for eighteen months when she noticed a lump in the epigastric region. This development was followed by complaint of pain in the left side, worse after food, and accompanied by nausea but no vomiting. The lump gradually increased, and when she finally summoned up courage to return to the hospital in December, 1899, it was the size of a tangerine orange. It lay midway between the ensiform process and the umbilicus in the central line. There was no evidence of distant dissemination and no signs of dilatation of the stomach. On exploration, the mass was found to be a malignant growth in the margin of the liver just to the left of the gall-bladder. There were several omental adhesions, but after their separation the parts could be fully explored, and as there was no evidence of any other growths either in the liver or stomach it was decided to

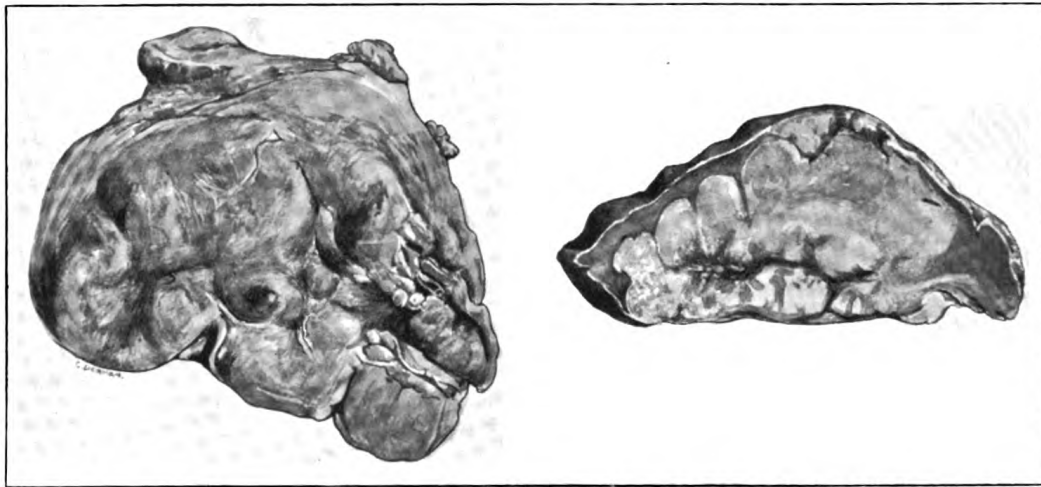


FIG. 4.—Portion of gummatous liver weighing 8½ oz. excised from the second case described in the text. The cut surface is shown on the right.

remove the recurrence. This was done by withdrawing the affected portion of liver from the abdomen, transfixing its base with a couple of knitting needles, and surrounding the pedicle thus formed with an elastic ligature. The portion of liver bearing the growth was then cut away, and the stump treated extraperitoneally. The patient made a good immediate recovery, and was able to leave the hospital about eight weeks after the operation. Six months later she appeared in good health, but was losing weight, and as far as I can recollect, she died within twelve months from the operation with local recurrence and evidence of secondary deposits in the liver.

I have now and again removed a portion of the edge of the liver during the performance of partial gastrectomy or cholecystectomy, and have come to look upon this as a procedure which can be carried out with impunity.

My experience of this limited type of excision began with the first gastrectomy that I ever performed in the case of a patient, J. H., aged 37 (Reg. No. 1062). The growth, a pyloric one, was adherent to the liver edge, but this was clearly a direct extension, and there was no evidence of dissemination. The liver was clamped well

beyond the affected portion, and the latter was cut away, the gastrectomy being completed by the method then known as Billroth No. 11. The clamp was removed from the liver on the third day. To my great disappointment, the patient died eight days later from a duodenal leak, but examination showed that the liver incision was healing satisfactorily and had in no way contributed to the unfortunate result. That case greatly encouraged me for it showed how direct invasion as opposed to dissemination need not prove a bar to successful operations for cancer of the viscera.

Though the indications for resection of any part of the liver are undoubtedly few, the surgeon may be called upon to perform it in the course of some operation in which interference of this kind has not been contemplated, and therefore we should be prepared with a suitable technique which may be used when the occasion does arise.

In spite of the fact that the opportunities are few, the total number of operations recorded is comparatively large, probably because almost every instance has been published. The operation has been carried out for a whole variety of conditions. Many surgeons hold that it should be part of the routine operation for malignant disease of the gall-bladder, and they recommend that a wedge-shaped portion of the liver should be excised whenever the removal of such a gall-bladder is undertaken. Others limit the interference with the liver to those cases in which there is evidence of a direct extension to that organ. My experience has taught me that when malignant disease of the gall-bladder can be recognized by the naked eye, no operative interference—whatever the type—can be expected to lead to permanent cure, and that the operation of excision is only a palliative measure. Under these circumstances I should not consider it justifiable to incur any great additional risk in dealing with it.

Direct extension of malignant disease from the stomach or colon may render removal of a portion of the liver necessary, and I think this will become one of the most frequent indications, especially as it usually only involves the removal of a slice or wedge from the edge of the organ. In cases of this kind, such an extension to the liver need not in itself be a bar to successful operation, but of course it would be useless to carry out this portion of the operation if the extension of the growth in other directions rendered its removal unprofitable.

The operation has been extolled, by some writers, for hydatid disease, but I am satisfied that this is never an absolute indication and that it should only be carried out when the conditions are such that the resection of a tongue-shaped portion of liver bearing the hydatid (or it may be a simple cyst, as in Sir John Bland-Sutton's case) is simpler and safer than the other available methods of dealing with such cysts.

Tumours of uncertain nature have often led to this operation being carried out, and that was so in my own cases. Many of these have turned out to be angiomas and some have been solid adenomas, and both of these may, in my opinion, be legitimately dealt with in this way, provided the local conditions will admit of a clean excision without approaching too near the hilus.

In other cases, tumours regarded as localized primary malignant disease, or as localized secondary masses, the primary growth having been dealt with, have also furnished examples of the cases in which the operation has been successfully carried out; and I should look upon these conditions as perfectly justifiable indications, considering the doubt that most often exists with regard to the naked-eye diagnosis, and the very hopeless outlook when nothing can be done.

Several times granulomas, and especially gummas, have been removed from the liver, always under the impression that the surgeon was dealing with malignant disease. Of course, this is an error which is less likely to occur at

the present time, and I have learnt that gummata, even when localized, may usually be distinguished by the naked eye. They are usually smaller than malignant nodules. Their surface is rounded and not umbilicated, and there is much more perihepatitis than is the case with new growths. If real doubt exists, I should not hesitate to remove a portion for section, and either perform a secondary operation or carry out the operation of excision there and then on the evidence furnished by a frozen section, provided that some pathologist, in whose opinion I had confidence, would take the responsibility of giving a definite opinion. I must say that in practice it usually means that when there is doubt, it is better to wait until paraffin sections can be made and examined.

Riedel's lobe has been excised. This is undoubtedly sometimes a temptation, because the pedicle which connects the latter with the liver is often thin and very easily dealt with. However, I question very much if the mere presence of the lobe is ever the cause of symptoms, and the surgeon should carry out a most careful investigation before deciding that the lobe can of itself have caused the symptoms and therefore be justifiably removed. Of course, there may be some cases in which the persistent dragging of such a lobe, or its mere presence, has become an obsession to the patient, but usually I believe there is some other explanation of any discomfort, mental or otherwise, from which they may suffer.

Finally, the subject is intimately associated with the question of injuries of the liver for the treatment of lacerations of the organ, and the removal of some portion which has been nearly separated by the traumatism presents many of the same technical difficulties.

The great problem in dealing with resection of the liver has always been the control of hæmorrhage, not only at the time of the operation, but permanently. Let us review briefly the methods which have so far been employed.

Formerly, it was a question of whether or not the tumour was sufficiently pedunculated to allow of its being brought outside the abdomen and the pedicle treated extraperitoneally. This method is now seldom, if ever, employed, but it should be borne in mind, for it can be carried out with success if necessary. The indications for its use may be some well pedunculated tumour with a very vascular pedicle such as is found in some of the angiomata; or else the inexperience of the operator. In actual practice, the tumour is brought outside the abdomen and the pedicle is transfixed by two stout needles. Wyeth's pins or thick knitting needles will serve the purpose. An elastic ligature (such as a piece of fine rubber drainage tube) is then wound tightly round the pedicle on the proximal side of the needles and the remainder of the parietal incision is closed. The needles lie on the abdominal wall, pads of gauze intervening. As a last step the tumour may be cut away, or this may be deferred for 48 hours until the pedicle has become adherent to the abdominal wall. In either case the knife or cautery is employed, and the stump is dressed antiseptically. In about a fortnight the pedicle will have become firmly adherent and the process of its isolation will be well advanced so that the needles can safely be removed, but it is usually many weeks before the stump heals over.

But the ideal plan is to carry out the resection and to repair the wound in the liver by immediate suture with closure of the abdominal wall. For this type of operation to be carried out, means must be provided both for the temporary and permanent arrest of hæmorrhage. So far as the temporary arrest is concerned, the methods available are :

(1) That originated by Hogarth Pringle of Glasgow, in which the blood-vessels entering the liver are either caught and held, or are temporarily clamped. This is carried out by the fingers or by a special clamp (Baron's) compressing the structures in the free edge of the lesser omentum. This plan has many possible advantages, and has perhaps not received the consideration it deserves.

(2) The second plan is again the manual control of the tissue to be divided, an assistant taking a firm grasp of the organ on either side of the proposed incision. The liver is very plastic and can endure quite firm pressure without harm, provided that it is applied slowly and steadily. This method is efficient, but it is irksome, and the hands of the assistant are very often in the way. Except, therefore under special circumstances, it is not a method which can be universally relied upon.

(3) Clamps may be used, and are often effective because of the plasticity of the liver substance, but they undoubtedly crush the tissue severely and are not suitable for holding the thicker portions of the organ. There need be no hesitation in using them for grasping the tissue which is to be removed, but for use on the proximal side there are many disadvantages, except in those cases where the liver tissue is thinned out to an unusual degree. In some of the cases of pedunculated tumour, they have been used with great success.

(4) A tourniquet may be applied in certain circumstances, and with a proper introducer, an elastic ligature can be passed right through the substance of the liver and tied in such a way as to control the blood supply to a particular area, but this method rather interferes with the subsequent suture and permanent arrest of hæmorrhage.

(5) Sometimes the thermocautery has been used to actually make the incision and at the same time staunch the bleeding, and this is a method which has proved satisfactory in the hands of Keen, of Philadelphia, who removed the greater part of the left lobe by this method, 20 to 30 minutes being required to cut away the mass. I would certainly adopt this plan, probably in association with other methods, when the line of section has to be made in close proximity to a malignant growth.

The next question is the permanent arrest of hæmorrhage and the closure or other treatment of the liver wound. For the permanent arrest of hæmorrhage, the branches of the hepatic artery may be caught and tied. The big venous channels may similarly be surrounded by a purse-string of catgut, which must be tightened slowly and carefully, as it may easily cut through the friable liver substance and fail in its purpose. For the rest, the mere apposition of the surfaces will usually be enough to stay any further hæmorrhage.

The great difficulty about the suture of the liver is the friability of its substance and the readiness with which sutures cut out or cut through its substance. It is only the tough capsule which ever enables the sutures to hold. For this reason various devices have been introduced. Special needles are recommended. Those introduced by Kousnetzoff are blunt, and are flattened from side to side so as to cut the liver substance as little as possible. Payer has used magnesium plates in imitation of the old button stitch. Various complicated methods of chain suture have also been devised and extensively used in experimental work on animals. For these sutures various substances have been employed—silk, catgut, kangaroo tendon, strips of fascia and ribbons of gauze.

In my view, we should have at our command some method of liver suture which is simple, and which does not require any special instruments or suture material. Invariably, special apparatus which is only very seldom required is

mislaid or is out of order when wanted. I would, therefore, especially direct your attention to the method of liver suture which I employ, and which has the advantage of fulfilling the indications of both the temporary and permanent arrest of hæmorrhage, while greatly facilitating the application of sutures. It was suggested to me by a paper published in the *Annals of Surgery* for November, 1907, by Van Buren Knott, and entitled "A New Liver Suture." The plan, as described by Knott, consisted in passing a stout catgut suture parallel to the line of the proposed incision, which served to give a hold to the sutures of apposition which are passed and tied over this buried strand. The passage of this suture requires a special needle, and as originally described, it did not provide a means of arresting hæmorrhage until the sutures of apposition were tied (fig. 5). I have modified the method as shown in the diagram, and in the specimens which I have brought with me. Instead of burying a single strand longitudinally, I pass a series of loops of catgut into the liver substance parallel to the proposed incision. For this purpose I use an ordinary large size

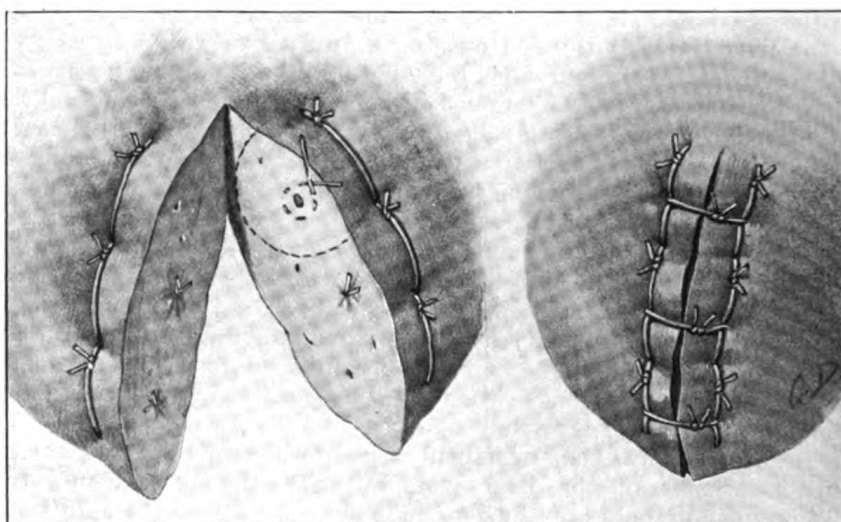


FIG. 5.—Illustrating the method of liver suture employed. A couple of hepatic veins are shown surrounded by purse-string sutures of catgut.
(Reproduced by kind permission of Messrs. Cassells.)

half circle round needle with a circular eye threaded with No. 3 chromic catgut. Each loop is tied as passed, and serves to arrest the hæmorrhage in the area of liver in its grasp. Some vessels will escape, but they may be caught separately and tied or surrounded with a purse-string of finer catgut. The sutures used for the apposition of the liver are then passed over the longitudinal loops which serve to prevent them cutting out, and provide quite a sufficient support for the apposition of the cut surfaces, even when the latter are a little reluctant to lie together. The exact apposition of the edges can be secured by a few additional interrupted sutures passed here and there as required.

This method answered most admirably for dealing with the large tumour which I have brought before your notice this evening, and I can recommend it with confidence. The method is much easier in its application than to describe, but I trust that the drawings and specimens will make it intelligible.

In dealing with an actual case where hepatectomy is necessary the various steps required are as follows: (1) Adequate exposure; (2) exploration and decision; (3) temporary control of hæmorrhage; (4) the actual excision of the involved area; (5) permanent arrest of hæmorrhage and repair of the liver wound; (6) toilet of the peritoneum and closure of the abdominal wound.

The parietal incision to be employed will depend upon the situation of the tumour and the build of the patient. The exposure must be sufficient, and for most cases, some type of vertical incision will be best, but the surgeon must never hesitate to make a cross cut if additional room is required. If the tumour is far back on the right lobe, then a long oblique incision 1 in. below the costal margin will be the best to employ. It may be possible to obtain much help in exposure from the use of the reverse Trendelenburg posture, the elevation of the lumbar region and the division of the triangular ligament. Gauze packed over the dome of the liver as described by Masson may also be an aid.

As soon as the abdomen is opened, the condition must be carefully explored in order to determine the feasibility of its removal. For this purpose, adherent omentum may have to be separated or adhesions to the parietes and the other viscera divided. This may be done without hesitation, for some of the most favourable cases are associated with such adhesions, and their separation may bring an apparently hopeless looking condition within the range of safe surgical enterprise. The greatest advance in abdominal surgery to which I have been witness is this preliminary examination of pathological conditions under the guidance of the eye, and the more thorough mobilization, by separation of adhesions and unimportant peritoneal folds, avascular ligaments, &c.

If there is any question of malignancy, a careful search must be made on the one hand for a primary focus, and on the other for further deposits in the liver. In illustration of the latter there are the not uncommon cases of malignant disease of the gall-bladder with direct extension to the liver, which could easily be removed by a moderate wedge-shaped excision, but in which a secondary deposit present on the dome of the liver renders excision valueless. It is most important to determine the relation of the tumour and the incisions that will be necessary for its removal to the hilum, as this part of the liver must not be encroached upon. The gall-bladder and cystic duct can of course be removed if necessary.

Whenever possible the part to be excised should take the form of a V, but to avoid going too near the hilum it may be necessary to leave the point of the latter rounded or rectangular. When the part removed is not too large, the gap can be brought together and sutured, but if this is not possible, the apex may be dealt with in this way and the remainder left gaping.

Every effort should be made to stay oozing. A good deal of the oozing may be due to venous engorgement, and will stop when the liver is allowed to fall back into position. In spite of great care it may be necessary to leave in a gauze pack, and in order to exert pressure on the bleeding area, one or two sutures may be passed through the liver on either side, so that the edges of the gap may be drawn together over the gauze. It will not often be possible to remove the gauze with safety sooner than ten or twelve days after operation. In all cases, drainage should be provided lest there be bile leakage.

In dealing with angiomas, great care must be taken to cut wide of the actual tumour, and to avoid puncture of the latter, as most serious hæmorrhage may result.

In conclusion, I suggest :—

(1) That the method I have described is suitable for dealing with every kind of wound necessary for resection of portions of the liver.

(2) That small resections of the liver edge can be safely dealt with by cutting into its substance obliquely so that the edges of the wound fall together like the flaps of an amputation, and can be then readily sutured by any simple method.

(3) That when the section of the liver must be made in close proximity to a malignant growth the actual division of the liver should be made with the cautery.

(4) That the method of extraperitoneal resection after transfixion of the pedicle and elastic ligature may occasionally be peculiarly applicable and need not be despised.

(5) That the after-history of the cases presented shows that a considerable portion of the liver tissue may be removed without interfering with the subsequent health of the patient.

Primary Carcinoma of the Liver excised by Operation.

By GARNETT WRIGHT, F.R.C.S. (Manchester).

THE occasions on which any one surgeon can meet with liver tumours which are operable must of necessity be limited, and this is especially so in the case of malignant neoplasms. It is, therefore, useful to pool our experiences of such cases, and this is my object in reporting the following case :—

The patient was a man, aged 60, whom I saw for Dr. Marsh, of Macclesfield, on June 21, 1920. His illness dated back for some six weeks; during that time he had suffered from griping pains in the upper abdomen, shooting through to the back and to the right shoulder. During this period a lump had formed which had increased in size. There was nothing further of note in the clinical history, except that for some months he had had increasing constipation and occasional passage of bright red blood, which he attributed to piles. He had passed no mucus and had had no diarrhœa. He also had some nocturnal frequency of micturition.

On examination he was found to be in fairly good general health. In the abdomen there was a large oval mass on the right side of the umbilicus reaching to the costal margin. It was not fluctuant and moved freely with respiration. It was dull all over on percussion and the dullness was continuous with the liver dullness. The mass could not be pushed back into the loin. I thought I had probably to deal with a much enlarged and adherent gall-bladder.

Operation was performed on July 8, 1920. The abdomen was opened through the right upper rectus muscle, when the mass was seen to consist of a large spherical tumour in the right lobe of the liver. It was not pedunculated but was deeply embedded in the liver substance and was about the size of a Jaffa orange. The gall-bladder was lying stretched over the left margin of the growth and there was a small secondary nodule in the liver substance, about the size of a pea, lying near the sharp margin of the liver close to the cleft for the falciform ligament. No further growths could be found in the liver, and as no primary tumour could be discovered anywhere in the abdomen, I decided that I had to deal with a primary carcinoma of the liver, and that I would attempt to excise it. I first divided the cystic duct, as the gall-

bladder was so closely applied to the tumour that it was necessary to remove it. At this stage I noted that the hepatic artery was much enlarged and tortuous. I began by attempting to prevent hæmorrhage by mass ligatures through the liver tissue, but I soon abandoned this and cut through the liver tissue. In the deeper parts the tumour began to shell out, so that I completed the separation by shelling out the tumour from the liver. My assistant controlled the bleeding by pressure of hot swabs, and when the growth had been removed all large veins and spurting arteries were caught with forceps and ligatured. An attempt was made to close the gap in the liver by means of mass sutures but too much had been removed to allow of complete closure. The wound in the liver was therefore packed with gauze and the abdominal wall was closed. The gauze pack was removed at the end of three days and the patient made a good recovery except for an attack of double glaucoma for which Dr. McNabb did a double iridectomy. He is still alive and in good health at the present time.

The specimen removed was hardened and then cut across and it shows a well encapsulated tumour with a very varied appearance, being bile-stained in some places and showing hæmorrhages and degenerations in others. The small secondary nodule can be seen quite separate from the main growth.

The tumour was examined microscopically at the time by Professor H. R. Dean and recently by Dr. Ryrie and Professor Shaw Dunn, and they all three came to the same conclusion that it was a primary carcinoma of liver cells. Dr. Ryrie has kindly given me a detailed pathological report as follows:—

“The tissue submitted for examination consists almost entirely of new growth, pale in colour and often slightly bile-stained, with occasional areas of hæmorrhage. It is well defined from the small margin of liver tissue remaining, but in this liver tissue are several small pale, spherical nodules apparently quite separate from the primary mass.

“*Microscopic Examination.*—The structure varies considerably in different parts. Most sections show a solid mass of closely packed tumour cells devoid of any definite histological arrangement. Blood-vessels are scanty, but there are large sinus-like blood spaces, with only an imperfect endothelial lining separating the tumour cells from the blood stream. In these spaces tumour cells can sometimes be seen among the blood corpuscles. Elsewhere the tumour cells are arranged in thick trabeculae separated by fine strands of connective tissue and small blood-vessels, and frequently the cells are arranged in tube-like structures in the lumina of which a brownish stained material occurs, apparently a product at least related to bile—mingled with products of cell degeneration.

“There are areas of hæmorrhage. In some areas the tumour cells appear to have undergone atrophy, leaving a vascular cellular stroma where connective tissue cells are proliferating. Areas of necrosis also occur distal to the vessels, so that in these areas the tumour has a definite perithelial arrangement.

“In the sections the main tumour mass appears encapsuled. Encapsulation of the secondary nodules is less perfect, so that the tumour cells are sometimes in close opposition to liver cells along an irregular frontier. Tumour cells are seen in vessels distal to those secondary tumours. The liver immediately around the tumour is cirrhotic, secondary to the tumour process, but away from the tumour the liver tissue is not cirrhotic.

“There is extraordinary variety in the size and shape of the cells of the tumour and many show degenerative changes, vacuolation, and coarse granularity. Nuclear variations are many. Nuclei of great size and very irregular shape are common, and binucleated and multinucleated cell masses are frequently seen. Great increase in size and number of nucleoli constitutes a prominent feature. Mitotic figures are present, but not numerous.

“The very atypical structure of the tissue, the aberrant types of cell, nuclei and nucleoli, the presence of secondary growth in the liver, the imperfect capsulation of

these growths, the presence of tumour cells in the vessels, and the areas of necrosis, are findings suggestive of a malignant growth. Nevertheless, clinical malignancy in the sense of capacity to cause generalized metastases is not a necessary corollary of even extreme anaplasia in these tumours, and it is doubtful whether the cells seen in the vessels are viable outside the liver. The non-committal term 'hepatoma' has on these grounds tended to replace the term 'liver cell carcinoma.' "

The epithelial elements in the liver from which a primary carcinoma may arise are: (1) the epithelium of the bile-ducts, and (2) the liver cells.

These tumours have been classified into three varieties: (1) the large solitary carcinoma; (2) the nodular variety, and (3) multiple carcinoma. The last named variety is the kind found complicating cirrhosis of the liver. The nodular variety consists of a collection of malignant nodules, one of which has probably been the primary growth, the others being metastatic. The tumour for which Keen excised the left lobe of the liver was of this nature.

The large solitary growths, of which my case is an example, are interesting because they do not seem to be so malignant as the other varieties, and are said not to produce metastases outside the liver. Some indeed deny that they are malignant and the name hepatoma has been given them. Microscopically the evidence of malignancy is quite definite. The irregular character of the cells, and their nuclei; the occurrence of binucleated cells and the presence of mitotic figures in the nuclei all point in that direction. In addition, one section I show you demonstrates the presence of tumour cells in a vessel beyond the periphery of the tumour, and although the tumour is for the most part well encapsuled, in some places the tumour cells are in close contact with the liver cells. Finally there is the presence of a secondary nodule quite separate from the primary tumour which can be seen in my specimen.

From the clinical side, however, there seems to be no doubt that the malignancy, if any, is of a low grade. In my case operation two and a half years ago, of a somewhat incomplete nature, has not been followed by recurrence. This is in sharp contrast to the results obtained in carcinoma of the nodular variety, where recurrence has been the rule.

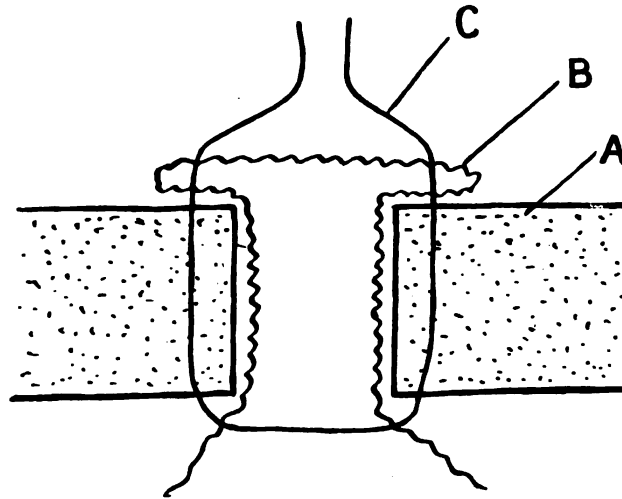
The technique of partial resection of the liver has been chiefly concerned with the control of hæmorrhage, and various methods have been adopted to isolate the operation area, such as interlocking ligatures, or elastic tourniquets round the base of the tumour, kept in position by knitting needles passed through the substance of the liver. The drawback to all these methods is that while they are easy to apply to small and pedunculated tumours they are very difficult to use in the case of larger tumours which are deeply embedded in the substance of the liver. From my experience in this case, and in some cases of excision of the gall-bladder with a wedge of liver tissue, I think the simple method of cutting boldly through the liver tissue is quite safe. It is easy for an assistant to control the hæmorrhage from the cut surfaces by pressure with a hot swab. When the excision is complete the swab is removed gradually, the vessels being picked up with forceps and ligatured in the usual way, or by under-running them. The cut surfaces of the liver should then be drawn together by deep sutures if possible. If not, then packing should be resorted to.

In any future case I would certainly excise more boldly and widely than I did in this instance.

A Case of Resection of the Liver for Malignant Disease spreading from the Gall-bladder.

By CLAUDE FRANKAU, C.B.E., D.S.O., F.R.C.S.

A WOMAN, aged 40, came under my care in April, 1913, for recurring attacks of gall-stone colic. The history and signs were typical and there was no reason to suspect any complication, but at the operation it was found that in addition to stones there was malignant disease of the gall-bladder with evidence of infiltration of the liver and at one point a definite secondary nodule on the convex surface of the liver. I decided to attempt to eradicate the disease by resection of the affected portion and accordingly with a scalpel excised the gall-bladder together with a wedge of liver measuring 3 in. wide at its base, $3\frac{1}{2}$ in. deep, and rather over 2 in. thick at the deepest part of



A, Liver ; B, Interposed omentum ; C, Suture.

the incision. The line of incision was well clear of any visible growth. Hæmorrhage was brisk for the moment but was readily kept in check by digital compression of the liver on either side of the incision and by a gauze pressure. I then attempted to suture the cut edges together by means of stout catgut ligatures on round-bodied needles passed deeply through the liver substance: this failed as the stitches cut out at once. As I was anxious to suture the gap rather than trust to gauze plugging I brought up a large fold of great omentum in a double layer into the liver incision and overlapped both cut edges with it. The sutures were then passed through omentum and liver as shown in the diagram (*see figure*): they held firmly and good coaptation of the cut surfaces with complete hæmostasis was readily obtained. The portion of omentum interposed was then detached from the main omental sheet by ligaturing it off: and the abdomen was closed, a tube drain being left in leading down to the ligatured cystic duct.

Recovery was uneventful and the patient was alive in August, 1914, and apparently well; unfortunately she could not be traced after that date, but as she had survived for fifteen months the result justified the risk taken at operation. The diagnosis of carcinoma was confirmed by microscopical examination.

I have used the method of interposing omentum between cut or torn surfaces of liver in a number of cases since and have found it satisfactory.

Case of Excision of an Adenoma of the Liver, which had ruptured spontaneously, causing Internal Hæmorrhage.

By PHILIP TURNER, M.S., F.R.C.S.

I have only once removed a primary growth of the liver but that was in the most remarkable abdominal emergency with which I have ever had to deal. The patient, a married woman, F. C., aged 29, was admitted to Guy's Hospital on November 16, 1913, for abdominal pain and vomiting. The pain, which was very severe, had come on suddenly about ten hours before admission and the vomiting had been frequent. There was no history of injury; she had never before had any severe attack of abdominal pain, and there was no history of any digestive troubles.

On admission the abdomen was moderately distended and there was general pain, rigidity and tenderness: signs of free fluid were found. There was a marked degree of collapse; the pulse was very weak and rapid, the skin cold, and the patient was very pale. Menstruation was regular and normal.

No definite diagnosis was made before operation, but the pallor, condition of the pulse, and the presence of free fluid, suggested an internal hæmorrhage, most probably the result of a ruptured ectopic gestation.

When the abdomen was opened the peritoneal cavity was found to be full of blood, but both the tubes and the ovaries were normal. On the abdominal cavity being searched for the source of the hæmorrhage a large dome-shaped tumour was felt projecting from the inferior surface of the right lobe of the liver. The incision was prolonged in an upward direction to bring this into view, and a rent, about 3 in. in length, from which blood was freely escaping, was found in the most prominent part of this tumour. Attempts to close the rent and check the bleeding by sutures were not successful, as the stitches, owing to the friable character of the surrounding tissue, which had the consistency of placenta, at once cut through. For the same reason an attempt to plug the rent with gauze led to further laceration and increased the hæmorrhage. Removal of the tumour appeared to offer the only prospect of stopping the bleeding and this was done by Pacquelin's cautery. The tumour did not extend deeply into the liver substance: there was no pedicle, though the base was slightly constricted, and the charred area of the liver left after removal of the tumour was about the size of the palm of the hand. There was no bleeding from this surface.

The condition of the patient was very grave: She was infused but died about an hour after the completion of the operation. At the post-mortem examination the stomach and intestines were normal: the stomach was full of bile-stained fluid. On the under surface of the right lobe of the liver there was irregular bruising and superficial laceration over an area the size of the palm of one's hand, corresponding with the site from which the tumour had been removed. There was nothing found in the post-mortem room to indicate the nature of the tumour. It appeared to have been attached to the liver rather than to have arisen in its substance and there were not any metastases to indicate that it was malignant. Macroscopically one would have judged that the whole tumour had been removed. Histological examination of portions of the liver which had been in contact with the growth showed normal hepatic tissue. The other viscera were normal.

Owing to its friable nature the tumour did not make a satisfactory museum specimen and it has not been preserved, but a coloured drawing made to scale at the time gives a good idea of its appearance. Its length and greatest diameter were both about four and a half inches.

The tumour was examined histologically by Dr. G. W. Nicholson, who reported as follows :—

“The sections represent part of a large adenoma of the liver. It consists of irregular trabeculæ of somewhat small hepatic cells and contains no ducts or portal systems. It is entirely without a capsule. Parts of it have undergone hæmorrhagic necrosis. Others show the result of old hæmorrhages in the shape of a fibrinous deposit between the remains of the epithelial cells.”

At the operation the growth was thought to be either a sarcoma or possibly an angioma. No history of injury could be obtained from the patient and careful inquiry of her relatives failed to produce any account of an accident. Presumably, however, some slight injury, possibly even muscular action, must have caused the hæmorrhage. Doubtless the tumour was much smaller before the hæmorrhage occurred and its friable character was due to its being infiltrated with blood-clot. Removal by the cautery was rapidly carried out and there was no hæmorrhage from the cut surface. Death was due to the hæmorrhage rather than to shock from the operation.

Case of Primary Tumour of the Liver removed by Operation.

By FRANK KIDD, M.Ch.Cantab., F.R.C.S.Eng.

CASE history : Female, aged 57, admitted to the London Hospital on July 5, 1911. Sudden onset twelve days ago, aching pain in right iliac fossa, burning pain, accompanied by diarrhœa, no vomiting. During the last few days has been passing urine more frequently, has had to get up five or six times at night and then passes only a little at a time. She had a similar attack of pain two months ago, but at that time there was no trouble with the urine.

Examination : Temperature 99·5° F., pulse-rate 94 to the minute. Abdomen moves satisfactorily; no rigidity; tenderness all over the right flank. A large smooth rounded swelling can be felt in right flank.

Operation : This patient was admitted during the evening. I thought she was a case of subacute appendicitis with abscess formation, as I had to operate on her in the middle of the night, having already done a number of acute appendicitis cases consecutively. I turned the right rectus inwards and on opening the peritoneum found that the swelling was not an abscess of the appendix, but a tumour the size of a cricket ball growing in the substance of the right lobe of the liver towards its anterior margin. A rapid exploration of the abdomen revealed no other lesion, and particularly nothing in the nature of a primary growth to which the tumour could be secondary. I came to the conclusion that I was dealing with a primary tumour of the liver and decided to remove it. I was accustomed to use Cullen's blunted needles for stitching the kidney substance and always had some at hand. I therefore passed splinting sutures of stout catgut some 2 in. away from the growth through the liver substance with Cullen's blunted needles, each suture taking a bite of about 2 in. and overlapping its fellow. Having inserted these sutures and tied

them I cut out the tumour enclosed in a wedge-shaped area of liver substance. I picked up one or two large arteries that were spurting, with artery forceps, and tied them off. I then placed catgut sutures threaded on the Cullen's needle outside the splinting sutures on each side and drew the sides of the wedge together. The cut in the liver came together very neatly and the splinting sutures stopped all the venous hæmorrhage. I was surprised to find how little difficulty there was in the whole procedure.

Progress: As regards the operation wound the patient did well, but there was an extraordinary heat wave that week, the temperature rising each day to 105° F. in the shade. She died from heat stroke on July 10, 1911, in company with many other victims of the heat lying in the wards of the hospital.

Necropsy, July 11, 1911: There was a little blood in the peritoneal cavity. The wound in the liver was healing satisfactorily. There was a little fibrinous peritonitis in the region of the wound of the liver. Not a sign of any neoplasm could be found in any organ of the body, though examination of the head was not permitted. Nothing was found as a cause of death, the organs appearing normal.

Dr. Hubert Turnbull at that time reported the tumour to be a Grawitz tumour. On section it presented to the naked eye many areas of fatty degeneration and looked exactly like a Grawitz tumour of the kidney. He has recently made a further investigation and reports that it is an adenoma of the liver. He states:—

"The cells are polygonal and are arranged in solid masses, which are separated into somewhat lobular areas by capillaries and capillary veins. The cells, with few exceptions, are completely vacuolated. This was probably due to glyconic infiltration, but unfortunately no sections were prepared to demonstrate fat or glycogen. Largely owing to this change in the cells the tumour was originally reported to be a Grawitz tumour. Now, in the first place, the term 'Grawitz tumour of the liver' is absurd, because the tumours described by Grawitz (Virchow's *Archiv*, 1888, xciii, p. 39) were in the kidney. In the second place, I am now satisfied that it is not a 'heterotopic, hepatic hypernephroma' (Birch-Hirschfeld, 'Grundriss der allgemeinen Pathologie,' 1892), but is a tumour composed of hepatic cells—a rare form of hepatic adenoma."

Mr. A. J. WALTON.

My own experience of hepatectomy is based on three cases. They were all examples of a spread of carcinoma to the liver from the gall-bladder. In each case a relatively large portion of the liver was removed, and although at the time one was struck with the ease and simplicity with which the operation itself was conducted, and with which the hæmorrhage was controlled, I am very doubtful whether operations of this nature are justifiable, for my three patients all had recurrence either in the peritoneum or the liver within a few months, the longest period of apparent freedom being six months. For less malignant conditions the operation holds, however, a very distinct place, and may be found useful in one of the following conditions:—

Adenomata.—These tumours, although extremely rare, form localized and sharply-defined tumour masses, generally in the right lobe, and will cause symptoms only from their size and local pressure, so that if removed completely they will probably not be followed by any recurrence.

Primary Carcinomata.—In certain cases carcinomata of the liver, as in some of those shown to-night, will form a localized tumour mass which is capable of removal. The prognosis, however, is much less likely to be satisfactory, for it is almost impossible to determine whether or no there are small outlying nodules in the rest of the substance of the liver.

Carcinoma Secondary to the Gall-bladder.—Many of these cases appear to be quite localized and capable of removal, but, as my own cases have shown, it is probable that when once the growth has extended into the liver substance there has been a microscopic spread either into the lymphatics in the gastro-hepatic omentum or round the head of the pancreas, or that there are other minute secondary growths in the rest of the liver and the peritoneum. Even if it be considered justifiable to remove such a growth, it is well to point out that care must be taken to avoid two errors. In the first place, chronic cholecystitis may closely resemble a carcinoma, and a large wedge of the liver may be taken

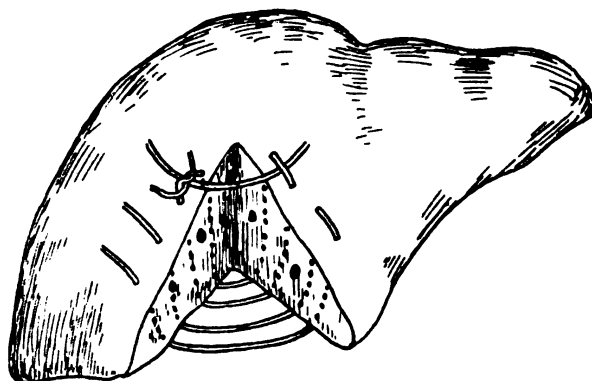


FIG. 1.

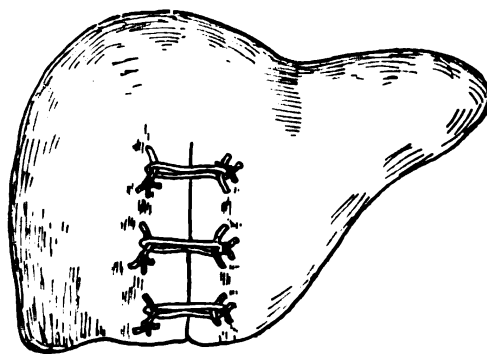


FIG. 2.

away in order to remove an inflammatory change which certainly would have cleared up after a simple cholecystectomy. The other error is to regard as a primary carcinoma of the gall-bladder one which is secondary to a relatively small growth elsewhere, such as in the liver or colon, for now and again large masses of secondary growth appear to be almost wholly limited to the gall-bladder, and thus it is easy to mistake this condition for the primary growth.

Cysts of the Liver.—The ideal treatment for cysts, whether they be hydatid or retention cysts, is certainly excision, but, owing to the enormous size which they may sometimes reach, such an operation is rarely feasible. When,

however, the mass is situated at the edge of the liver, and is the only focus, an operation of this sort is easily performed.

Hæmangiomata.—In certain cases a large tumour mass may arise in the right lobe of the liver, which appears to be quite localized, and to be capable of excision. It is, however, very doubtful whether a partial hepatectomy will be successful in such case, for an examination of those cases which have come to post-mortem show that not infrequently hæmangiomatous masses in the liver are not only multiple, but may be associated with similar conditions within the spleen.

As regards the actual technique of the operation, my own experience has been that it is relatively simple, and that the complicated technique which has been so often described is rarely necessary. Owing to the wide wedge shape of the liver, clamps are rarely satisfactory, and it is almost impossible to control the deeper vessels with their help. It is far better that an assistant should evert the liver and strongly grasp it on either side of the tumour with his hands. By this means the hæmorrhage is controlled. The tumour can then be removed, being in part freely excised and in part enucleated. This step is best carried out with scissors. It is in the method of suture of the resulting wound that so many different steps have been advocated. Much stress has been laid on the friability of the liver substance, and a great many devices have been introduced for the purpose of suturing the rent. The majority of these are unnecessary. If wide mattress sutures of stout catgut are passed through either side of the wound they will be found to grip the liver substance firmly. However, when they are tied, the wound tends to gape on one side. This is easily overcome by passing further strands of stout catgut under the loops of the mattress sutures before they are tied (*see* figs. 1 and 2, p. 63). After the mattress sutures are tied, these accessory loops are tightened, and will be found to give perfect apposition of the cut surfaces, and to control all hæmorrhage completely.

MR. CYRIL NITCH.

The specimen I am showing is that of Carcinoma of an Adrenal "Rest" in the Liver.

This specimen contains a lowly lobulated mass 2 in. in diameter and sharply marked off from the hepatic tissues. Microscopically, the growth presents the appearance of a typical adrenal carcinoma. The patient was a female, aged 54, who had suffered from attacks of abdominal pain and vomiting for many years. For three months there had been a dull aching pain in the right side of the abdomen and progressive loss of weight.

On examination: There was a hard, nodular mass in the upper part of the right iliac fossa which appeared to be connected with the cæcum. The liver dullness extended to the upper limit of the growth.

Operation, July, 1910: The growth was situated in a lingual lobe of the liver and looked exactly like a metastatic deposit. Every abdominal organ was carefully examined (including the kidneys), but no primary growth was discovered. Partial hepatectomy of a portion of the right lobe of the liver successfully carried out. Recovery uneventful.

In August, 1911, she was in good health and had gained weight, but she died in March, 1912, of cachexia with ascites and a large, hard, nodular liver.

Section of Surgery.

SUB-SECTION OF PROCTOLOGY.

President of Sub-section—Sir CHARTERS SYMONDS, K.B.E., C.B.,¹/₂M.S.

A New Method of treating Ischio-rectal and other Abscesses.

By J. P. LOCKHART-MUMMERY, F.R.C.S.

THE treatment of an ordinary subcutaneous abscess does not make much appeal to the modern surgeon, who generally leaves such cases to his assistant. The treatment is summed up in a few words: Open the abscess as aseptically as possible and put a drain into it.

At first sight this treatment does not seem capable of any improvement, yet few things are really incapable of improvement, and the treatment of abscesses has undergone many vicissitudes in the past.

It is interesting to trace the history of the treatment of abscesses so far as this is possible. In most of the early treatises on surgery it was advised that phlegmons or aposthumes, as abscesses were generally described, should be treated by the application of sedatives to drive the evil humours out of the painful part, and by some form of dietary, which varied according to the fashions of the time and the fancy of the author of the particular treatise. Later poultices in some form or another were recommended, and the materials from which these were made included various substances such as cow-dung, earth, bread, oatmeal and linseed. This treatment by poultices is still unfortunately much too frequently employed in the shape of so-called hot fomentations, although the modern fomentation, except from the point of view of cleanliness, compares most unfavourably as regards effectiveness with the cow-dung poultice of our ancestors.

Even as recently as in the time of our great grandfathers abscesses were generally treated by freely bleeding the patient and loosening his teeth with mercury.

From comparatively early times, however, it was recognized by the bolder spirits in our profession that the best method of all was to make an opening into the abscess and evacuate the humour, or pus, which it contained, and with the advent of anæsthetics, which did away with the greatest objection to this method of treatment, namely, the pain which it involved, it became a recognized practice, although even at the present time it is not done as often, or as soon, as it should be.

It was soon recognized that to make an opening only was not sufficient, as although this at once relieved the more urgent symptoms, the opening tended to close and the abscess to refill. To overcome this difficulty various foreign bodies, called drains, were introduced into the wound to prevent its closing prematurely. Drainage tubes were at first made of pewter for the proletariat

[February 14, 1923.]

and silver for the nobility and gentry, gold, I presume, being reserved for royalty; but with the advent of democracy india-rubber tubing soon became universal. At first the tube used to be introduced to the bottom of the abscess, but it is now generally recognized that it should only just reach the abscess cavity.

In my student days it was customary to treat abscesses by scraping them out and packing the cavity with gauze. I have never been able to ascertain who invented this barbarous practice, or what possible object it was expected to serve. It resulted in an extremely painful form of dressing, and tended to delay healing of the abscess to the maximum extent. Quite apart from this it seems foolish to make an opening into an abscess and then block it up again. In the case of ischio-rectal abscess, this form of treatment invariably resulted in producing the most classical kind of fistula-in-ano.

I have for many years been striving to discover a method of treating ischio-rectal abscesses by means of which the subsequent formation of a fistula might be avoided: a fistula was almost invariably the sequel under early methods of treating these abscesses. I soon found that if a fairly large-bore drainage tube was stitched into the opening and the end only just allowed to reach the cavity of the abscess, healing took place in quite a number of cases without the formation of a fistula. It was difficult, however, to keep the tube in place, and I have now entirely discarded drainage tubes in favour of the following method: The abscess is opened with a knife by a crucial incision in the usual way and the pus allowed to flow out. The skin covering, or forming, the outer wall of the abscess, is then completely cut away so as to leave a large opening of an inch or more in diameter, the interior of the abscess being left entirely alone and untouched. A large flat dressing of moist antiseptic gauze is applied all over the parts and covered with a protective so as to keep it moist and prevent it sticking to the edges of the wound and interfering with drainage. The pad should be large enough to absorb all the discharge for at least twelve hours. The patient should be nursed lying on his back as much as possible, so that the pressure is towards the opening in the abscess. When the dressing is changed it will be found that the abscess cavity has completely vanished, and that there is only a flat shallow ulcer, which may take ten days or a fortnight to heal, but does not result in a fistula once in twenty times. I have seen surprisingly large ischio-rectal abscesses, containing nearly a pint of pus, heal when drained in this manner without any fistula resulting.

The advantages of this method of draining an abscess are: (1) That it is supremely simple; (2) that it is not in the least painful, once the opening is made; and (3) that healing is very rapid and is not followed by a residual abscess or fistula. It might be thought that very bad scarring would result, but this is not the case, the ensuing scar being quite flat and surprisingly small. It is essential that the opening should be amply large, and I have not hesitated to remove up to a couple of inches of skin in some cases.

I have used the same method in treating abscesses in other parts of the body with equally satisfactory results, and should now only employ drainage tubes where the anatomy of the part prohibited the cutting away of the external wall of the abscess.

Incidentally I may mention that quite recently I have treated an abscess of the breast in this way, in which a large part of the breast was infiltrated with pus. A large piece of skin was cut out and no drainage tubes were used. The wound was completely healed in a little over a fortnight without any attempt at the formation of a residual abscess, without painful dressings and with the minimum of scarring.

Case which was clinically one of Inoperable Carcinoma of the Rectum treated by Colostomy and Subsequent Injections of Cuprase-Collosal Selenium and Collosal Cuprum for over Two Years, with Disappearance of the Growth.

By LIONEL E. C. NORBURY, F.R.C.S.

PATIENT, a female, aged 47. Seen by my colleague and myself in the out-patient department of St. Mark's Hospital in October, 1920. Diagnosed as extensive carcinoma of the rectum, probably inoperable. Extensive growth within easy reach of the finger but extending up the bowel. Fixed posteriorly.

Symptoms: Constant desire to have bowels opened. Diarrhœa, but no good result without aperients. Discharge of mucus but no blood.

Admitted to hospital, October 23, 1920, under the care of Sir Charles Gordon-Watson.

Abdominal exploration, October 28, by Sir Charles Gordon-Watson: Growth fixed posteriorly; uterus enlarged; enlarged mesenteric glands; liver normal.

Growth considered inoperable. Colostomy performed.

November 8, 1920: Intramuscular cuprase, 3 c.c. November 11: Intravenous selenium, 5 c.c. November 16: Intramuscular cuprase, 3 c.c. November 18: Intravenous selenium, 5 c.c. November 22: Intramuscular cuprase, 3 c.c.

November 29 to January 4, 1921: Collosal selenium, 5 c.c., intramuscular, every seven days.

January 11 to February 28: Collosal cuprum, 3 c.c., and then every fourteen days until May 24, 1921, and then every twenty-one days until June 28. Collosal selenium every twenty-one days till September 26, 1922. Then collosal cuprum every twenty-one days up to the present.

There has been practically no discharge from the rectum for the last two years and she has steadily put on flesh.

Present state: No growth or ulceration can be seen or felt in the rectum. Much narrowing of the pelvic colon just beyond the recto-sigmoidal junction, apparently the result of the colostomy. General health excellent. Vaginal examination: Nothing abnormal.

Unfortunately no microscopical examination was made of the growth and so the diagnosis could not be definitely established, but clinically the case is undoubtedly one of carcinoma.

I have employed colloidal preparations in a large number of cases of inoperable carcinoma and in my opinion such treatment tends to diminish the amount of the discharge from the growth and renders it less offensive.

A Specimen of Colon, showing Multiple Perforations resulting from Dysentery.

Shown by PERCIVAL COLE, F.R.C.S.

THIS specimen was removed post-mortem from a female patient aged 28, who was taken ill with pain in the lower abdomen and with the frequent passage of small, liquid, blood-stained stools. She had recently returned from a holiday on the continent—Germany, Switzerland, France—but this was her first visit abroad.

Before admission to hospital, sigmoidoscopy showed soft cedematous, papillomatous masses protruding into the lumen of the bowel, and commencing at a distance of 12 cm. from the anus. After admission to hospital the diarrhoea continued. Temperature ranged between 100° and 103° F.; the pulse varied from 120 to 160, and the patient's condition steadily became worse.

Emetine was administered hypodermically, although pathologically no evidence could be obtained to support a diagnosis of dysentery. Ten days after admission to hospital she became rapidly distended, and began to vomit.

Laparotomy showed that the lower abdomen was filled with a mixture of fluid faeces and pus, and she died the following day.

The following pathological report has been kindly furnished by Dr. Fry:—

PATHOLOGICAL REPORT ON A CASE OF MULTIPLE PERFORATIONS OF THE COLON, RESULTING FROM DYSENTERY, BY DR. FRY.

(I) *Clinical Pathology.*

Total leucocyte count, 9,500; polymorphonuclears, 73 per cent.; large hyalines, 4 per cent.; lymphocytes, 21 per cent.; eosinophils, 2 per cent.

Fæces: Loose watery stools, tinged with blood with shreds of mucus. Numerous epithelial cells, a few pus cells, and many red cells. No amœbæ, cysts or flagellates found. No enteric or dysenteric organisms isolated.

Urine: No albumin or sugar. A few urinary epithelial cells. A few red and white blood cells. Streptococci and *Bacillus coli* isolated.

Blood: Blood culture sterile. Agglutinations for *Bacillus typhosus*, Paratyphoid A and B, Gaertner, Aertrycke, Shiga, Flexner, V W X Y Z, all negative.

(II) *Morbid Pathology (partial post-mortem only).*

Peritoneal cavity contained a large amount of brown faecal exudate and the intestines were matted together. Omentum drawn down over intestines and firmly adherent round descending colon. After separation of adhesions faeces exuded from large intestine, which showed numerous perforations, forming a lattice work.

Large intestine: Interior of descending colon and sigmoid covered by a number of plum-coloured masses arising from mucous membrane. The mucous membrane between the polypoid masses appeared little affected. Other viscera not examined.

Microscopic sections show a considerable infiltration of the submucosa by mononuclear cells and cedema of the submucous coat. The mucous membrane, except where it has sloughed away, is less affected. In the vessels and lacunæ of the submucous coat can be seen a number of relatively large amœboid bodies with a well-marked nucleus and nucleolus. They are rather distorted by fixation but exhibit the general appearance of *Amœba histolytica*.

Mr. PERCIVAL COLE also showed a Specimen illustrating Vesico-colic Fistula resulting from Carcinoma of the Pelvic Colon.

Section of Surgery.

President—Mr. JAMES BERRY, F.R.C.S.

The Technique of Resection and Anastomosis of the Colon for Tumour.

By J. P. LOCKHART-MUMMERY, F.R.C.S.

CONSIDERABLE improvements have taken place during the last twenty years in this department of surgery, and it will be useful to revise our knowledge and practice, and to compare our results in this important field of abdominal surgery in the light of recent experience.

Resection and anastomosis of the colon has interested surgeons considerably during the last few years. There was a discussion upon it at the Medical Society of London on December 6, 1920, and it was one of the subjects dealt with last September at the Congrès français de Chirurgie, in Paris, at which I was present.

Much of our advance in this branch of abdominal surgery has been due to improved methods of diagnosis. Until comparatively recently stricture and growths in the colon were seldom detected except when the surgeon was exploring the abdomen for the cause of acute obstruction. As a result most of the operations performed were designed to deal with the obstruction which was present rather than with the tumour itself. Colostomy or Paul's operation held the field.

The mortality of resection was high, and anastomosis was generally performed extraperitoneally by some such operation as Greig Smith's well-known method. Even when a definite two-stage operation was planned the surgeon was invariably handicapped by the presence of a fæcal fistula, or colostomy on the abdominal wall, generally quite close to his operation site, and this necessarily controlled to a large extent the type of operation which could be done.

Many of the results were good, but the resections which were possible under these types of operation were very limited, and infection of the wound was the rule rather than the exception, so that good results were only obtained after long and tedious illnesses. Improvement in methods of diagnosis have altered these conditions, and it may safely be said that now most tumours of the colon are diagnosed before acute obstruction occurs, or at least their presence is so strongly suspected that the surgeon is called upon to explore the abdomen.

The modern surgeon has learnt much of his surgery, and most of his diagnosis, upon the operating table rather than in the post-mortem room, and

as a result is able to suspect the presence of a tumour in the colon long before it is palpable, and before serious obstruction has occurred. Aided further by the sigmoidoscope, by the chemical and microscopical examination of the stools, and by the use of X-rays after a bismuth meal or enema, a more or less accurate diagnosis is generally possible.

The fact that the surgeon is now able to operate upon patients with tumours in the colon before serious obstruction has occurred has entirely altered the type of operation that is performed, and greatly improved the results. It will be generally agreed that to attempt to resect or anastomose the large bowel in a patient suffering from any degree of obstruction is wrong, and that the proper treatment is first of all to relieve the obstruction and only to attempt to resect or anastomose the bowel after all obstruction has been relieved. Further, most surgeons now agree that cases of obstruction in the large bowel are best treated by draining the cæcum.

I would go further, and say that cases of obstruction of the colon should be treated by simple drainage of the cæcum without exploratory laparotomy, and that the exploratory laparotomy should be postponed until after the obstruction has been entirely relieved. The adoption of this course does away with the great danger of exploring the abdomen while the intestines are distended and the patient is suffering from fæcal vomiting.

The simple tube method of performing cæcostomy, which was described in the *Lancet* (November 25, 1922) can be easily performed under local anaesthesia with the minimum of risk. Within a few days the patient's toxic symptoms have disappeared and the abdomen can be explored under the best possible conditions. Cæcostomy performed in this way is a clean operation, as everything drains into a bucket through the tube, and there is consequently no danger of infecting the wound.

Before an attempt is made to resect the colon, I think it is very important that the patient should, if possible, be carefully prepared. The bowel should be cleared out, preferably by the use of castor oil, to which belladonna and opium should be added to relieve spasm and to enable the aperient to act more efficiently in the presence of the stricture, which is necessarily present. At the same time as the castor oil is administered a methylene blue pill, or teaspoonful of charcoal, should be given by the mouth and a careful note kept of its first appearance in the stools. This is a rough, but efficient test as to whether the bowel has really been cleared out. The charcoal should appear within twenty-four or thirty-six hours; if it fails to do so the bowel should be further cleared. At the same time kerol, dimol or some other intestinal antiseptic should be administered in full doses, or if sufficient time is available, a good preparation of Bulgarian bacillus may be administered in milk in order to modify the intestinal flora.

It is necessary to spend some time over the preparation of the patient, and as soon as the bowels have been satisfactorily cleared the patient should be put on large doses of liquid petroleum in order to liquefy everything within the colon and prevent the formation of scybala. During the preparatory treatment the patient should be fed with ordinary solid food, and on no account should he be starved. At least a couple of days should intervene between the administration of the purgatives and the operation. On no account should the patient be purged just before the operation on the colon. This preliminary treatment is a most important factor in securing good results, and the expenditure of a week or even more time upon it is fully justified.

THE INCISION.

This must, of course, depend to some extent upon the situation of the lesion, and it is not always possible to be certain which part of the colon is affected. The great majority of growths, however, occur in the sigmoid flexure, descending colon and splenic flexure, the next commonest situation being the cæcum.

For growths on the left side I believe the diagonal incision to be the best. It should begin close to the midline, $1\frac{1}{2}$ in. above the pubis and be carried outwards into the angle beneath the last rib (*see fig. 1*). It is the same incision, in fact, which is used in exposure of the left ureter. The rectus sheath is opened up and the rectus muscle itself freed, so that it can be drawn well over to the right side.

The advantages of this incision are that if the patient is turned slightly on the right side and is supported with sandbags under the buttock and left shoulder-blade: (1) It gives splendid exposure to the whole of the left side of

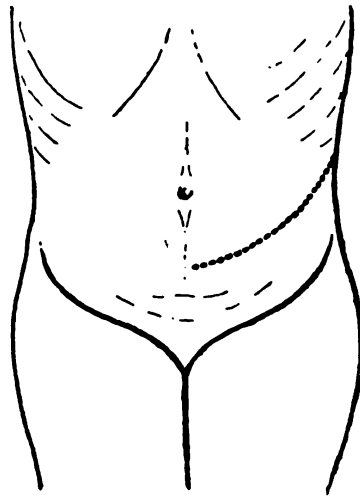


FIG. 1.

the colon; (2) the remainder of the intestines fall away and do not get in front of the colon during the operation; and (3) practically the whole of the incision lies through thick muscular structures which readily heal; further (4) no more than one nerve is likely to be divided; (5) the results, both as regards subsequent healing of the wound, and as regards access to the colon, are admirable; (6) this incision enables the surgeon to remove the splenic angle with the greatest ease. When there is doubt as to the locality of the lesion a midline incision is usually advisable.

The extent of colon removed must vary with the circumstances of the case, and with the opinion of the surgeon operating, some surgeons preferring to remove considerable lengths of colon, others preferring merely to remove that portion which is necessary; but as a general rule it may be stated that in the case of a tumour in the cæcum, or at the hepatic or splenic angles, it is better to resect the entire angle rather than to attempt local resection.

METHODS OF JOINING THE COLON AFTER RESECTION.

Twenty years ago the popular method of joining together two portions of intestine in order to obtain an anastomosis, was by means of some apparatus aided by stitching. Murphy's button, Allingham's bobbin, Senn's plates, Mayo-Robson's bobbin and Laplace's forceps all had their advocates, and any surgical text-book of that time contains numerous illustrations and descriptions of the various methods of using these appliances.

The most popular instrument was Murphy's button, which for a long time held the field owing to the ease with which it could be used. But the not infrequent fatalities attending its use, the fact that the button was not always passed but had to be removed by secondary operation, and the trouble from stricture which often resulted from the small stoma, led to its disuse.

With increased experience and improved technique surgeons soon found that plain stitching gave better results both as regards time and subsequent safety, and no surgeon of the present day would think of using any of these instruments for the purpose of anastomosing the large bowel.

When stitching first took the place of mechanical appliances lateral anastomosis was used in preference to axial anastomosis. At the present day we find that while axial anastomosis is always used in dealing with the small intestine, from what other surgeons have told me, and from contemporary writings, it would appear that lateral anastomosis is still the popular method of joining the large bowel.

The disadvantages of lateral anastomosis are obvious: (1) It requires a much greater length of bowel. (2) It requires either more extensive freeing of the colon, or less extensive removal of the diseased part as compared with axial union. The operation takes much longer to perform, as in addition to the actual anastomosis two ends of colon have to be closed. (3) The subsequent anatomical condition is not perfect, and the blind end of the proximal portion is apt to give trouble. I know of several cases in which there has been an abscess and ulceration in this portion of the blind bowel. The fact that it is still popular in spite of these objections shows that there must be some distinct advantage.

Though actual anastomosis in the small intestine has given universal satisfaction, it was found that axial anastomosis in the colon was liable to result in leakage, and that either peritonitis or fæcal fistula resulted. It was supposed that this was due to the more solid nature of the contents of the colon as compared with that of the small intestine, the line of suture being liable to give way from this cause. This, however, is not the real reason. The failure of axial union of the large intestine has nothing to do with the contents of the bowel, but is entirely the result of the anatomical arrangement of the blood-vessels in the wall of the colon, which differs widely from that of the small intestine. In the small intestine there is a very free anastomosis between the vessels feeding the bowel wall. In the colon, however, the arteries pass round the bowel in a circular direction from the mesenteric side parallel to each other. There is a free lateral circulation along the marginal artery, but the anastomosis in the bowel wall is not very free. It is obvious, therefore, that in performing a resection, if the bowel is cut across transversely and sewn together in this position, there is considerable risk of the stitches joining the edges of the bowel on the mesenteric side constricting the vessels, and so damaging the blood supply of the edge of the bowel opposite to the mesentery. As a matter of

fact, when leakage occurs after axial union of the colon it will generally be found that the leakage is on the side opposite to the mesentery, and is due to the sloughing of the edges of the bowel where they are stitched together, rather than to faulty suturing.

In performing lateral anastomosis there is no such danger of damaging the blood supply, as the arteries themselves cannot be caught up in a suture. All that is required in order to get a good result in axial union, is to see that the bowel is cut at an angle of 45° from the mesentery outwards. That is to say, that a larger amount of bowel is removed on the free than on the attached side. While this ensures a good blood supply to the whole of the sutured edge, it has the additional advantage of increasing the lumen of the bowel at the point of union, and so compensating for the narrowing produced by the turning in of the edges through the suturing. If this method of joining the colon axially is adopted, giving way of the suture line is no more likely to occur than in lateral anastomosis. I have used this method for years, and have had very few cases in which the result was not perfect. I suggest, therefore, that successful anastomosis in the large bowel wholly depends upon the surgeon ensuring a good blood supply to the joined ends of the bowel.

AXIAL ANASTOMOSIS.

I do not propose to take up time describing the aseptic technique of operations upon the colon. Careful protection of all parts of the wound and

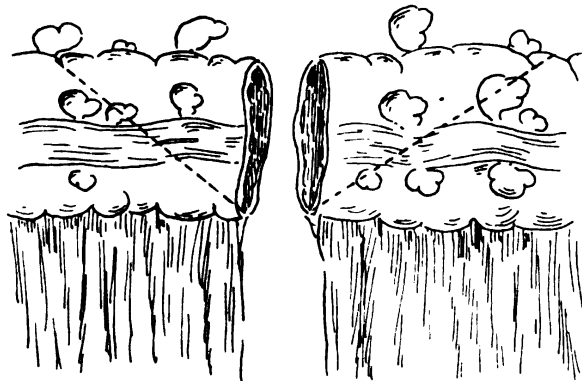


FIG. 2.—The dotted lines show how the bowel ends should be divided before performing end-to-end union. (From "Diseases of the Rectum and Colon," by the author.)¹

of the abdomen by means of swabs and towels is now the universal practice, so also is that of changing the gloves and instruments after dealing with the interior of the colon, before closing the wound.

I shall first briefly describe the method I prefer of axial union, and then other methods, which have their own special advantages and applications.

The part of the colon which it is proposed to resect is first drawn up into the wound and the mesentery is divided and any vessels secured. Rubber-covered clamps are next applied well above and below the area which it is proposed to resect. The bowel is then cut through at an angle of 45° to its

¹ For the loan of the blocks illustrating this paper (figs. 2-8B) the author is indebted to Messrs. Baillière, Tindall and Cox.

74 Lockhart-Mummery: *Resection and Anastomosis of Colon*

transverse diameter, both above and below and the resected portion is removed, together with its attached mesentery (fig. 2). If the clamps are controlling the blood-vessels in the mesentery they should be momentarily released, particularly the lower clamp, to make certain that there is a good blood supply to the bowel ends, as it is very easy to damage the blood supply to the lower ends of the colon when tying off the mesentery, more especially in fat persons. The two ends of the bowel are then brought in contact and caught together by two pairs of toothed forceps, or if preferred, guide sutures, the forceps being applied on the lateral aspects of the bowel, so that the two mesenteric edges come straight together (fig. 3). If there is excessive bulging of the mucous membrane some of this may be cut away. A stitch of fine catgut with a short straight needle is now started from one pair of toothed forceps and carried across to the other side of the bowel. This stitch takes up all the coats and is locked about every four or five stitches. It should be drawn sufficiently tight to ensure controlling any bleeding, and it is just as well to slack off the clamps occasionally to make sure that this has been done. When this stitch reaches the opposite pair of forceps they are removed and

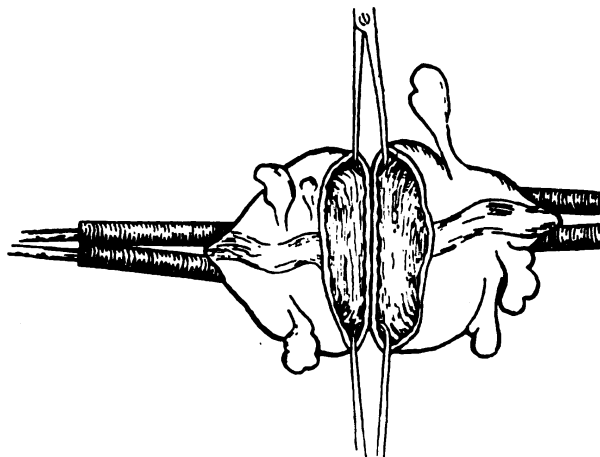


FIG. 3.—Shows the application of the forceps or guide sutures at the sides of the bowel halfway between the mesenteric attachment and the free border. (From "Diseases of the Rectum and Colon," by the author.)

it is continued right round until it reaches the point where it started, where it is tied off (fig. 4). The clamps are then released, the dirty towels and swabs removed, and the gloves changed, the bowel itself being first of all gently washed. A fine catgut peritoneal suture is now started on the mesenteric attachment on the outer side and carried right round the bowel over the first suture and down the gap in the mesentery so as to close it. I prefer to use ordinary through-and-through stitches and to use catgut entirely. Fine catgut on Souttar needles is very good for this purpose (fig. 5).

Lastly I like to have an omental graft over the anastomosis. One great advantage of the graft is that it prevents the adhesion of other structures to the line of anastomosis and gives much greater security should there be any leakage in the neighbourhood of the stitches. I do not think it matters very much whether it is a live, or a detached, graft. Where the omentum is large and long a live graft is very satisfactory, but detached grafts appear to me to

do equally well and there is not the risk of fixing the omentum and so possibly of producing bands. Finton and Peet's experimental work shows that detached omental grafts remain alive when wrapped round the intestine. I generally suture the omentum lightly round the junction with catgut sutures and then divide the omentum afterwards if it is considered necessary. This operation can be very quickly performed and has given excellent results.

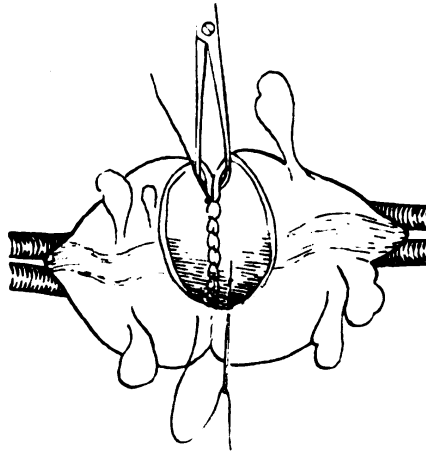


FIG. 4.—When the suture reaches the opposite side of the bowel, the toothed forceps are removed and the stitch is continued around until it reaches the point where it started. It is then tied off, great care being taken to keep all knots on the inner surface of the bowel. (From "Diseases of the Rectum and Colon," by the author.)

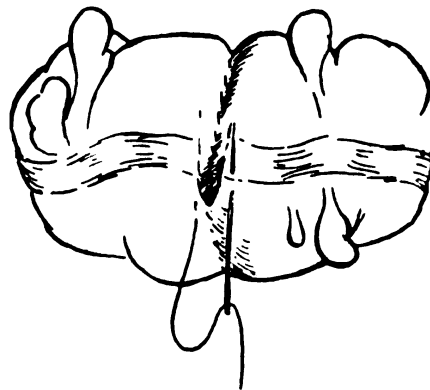


FIG. 5.—Showing the continuation of the fine-thread peritoneal stitch until it meets the mesentery on the opposite side of the bowel, where it is continued down the mesentery itself so as to close the gap. (From "Diseases of the Rectum and Colon," by the author.)

For some years now I have made it a practice always to drain the cæcum as a routine in all cases of anastomosis of the colon. I am sure that it renders the operation much safer, and has been one of the chief means of reducing the mortality of these operations. I used to use the appendix for this purpose, but for the last few years have been using the tube method, which as previously mentioned in this paper, was described in the *Lancet* (November 25, 1922).

76 Lockhart-Mummery: *Resection and Anastomosis of Colon*

After the abdomen has been closed a small wound is made over the cæcum and a knuckle of the anterior wall of this organ is pulled out and a puncture is made into this with a knife. A piece of rubber tube about 8 in. long and $\frac{1}{2}$ in. in diameter is pushed into this opening through the cæcum for about 2 in. Catgut stitches are then passed through the cæcal wall and through the drainage tube in three or four places so as to invaginate a cuff of cæcal wall into the lumen of the cæcum itself. A purse-string suture is then passed round the tube in the cæcal wall so as to invaginate the cuff more completely and it is then tied on to the tube. The cæcum is then drawn up against the inner surface of the abdominal wall, and the abdominal wound stitched up round the tube (fig. 6). After the patient has returned to bed a long collapsible rubber tube is attached to the tube in the cæcum and its other end passed over the side of the bed into a bucket. This completely drains the cæcum without anything being soiled, and in practice it is not found necessary to change the dressings for three or four days. The tube remains tight for about eight to nine days, and then comes out when the stitches dissolve. As a rule there is no leakage from the cæcum after the tube is removed. This is a much more convenient method than the performance of ordinary cæcostomy, which



FIG. 6.—Tube cæcostomy. (From "Diseases of the Rectum and Colon," by the author.)

invariably causes soiling, and sometimes results in a secondary operation to close the opening.

There is practically no after-treatment. The bowels can be moved at any time by giving a mild aperient, and ordinary food can be administered from the outset.

OTHER METHODS OF AXIAL UNION.

Some surgeons prefer to use clamps for sewing the ends of the colon together, in the same way as in the performance of gastro-enterostomy with clamps. A very ingenious clamp has been designed by Dr. Abadie, of Paris, in which the clamp blades revolve on each other in order to facilitate approximation of the edges of the bowel while passing the posterior stitch.

METHODS OF PERFORMING ANASTOMOSIS WITHOUT EXPOSING THE MUCOUS MEMBRANE.

Several very ingenious ways of joining the colon together have been designed by means of which the interior of the bowel is neither seen nor exposed at any time during the operation. Some of these methods deserve description.

The following method was first described by Gudin. The colon, at the point where it is desired to divide it, is crushed by a powerful pair of crushing forceps. Four narrow-bladed, tapered forceps are then placed on the crushed segments of bowel and the colon divided either with a knife or a cautery, between the clamps. The resected portion of colon with its two clamps having been removed, the remaining two clamps are brought together, and the ends of the colon are sewn together with stitches over the clamps, the stitches taking up the muscular and serous coats only. When the stitch which has been started at the handle end of the clamps comes back to the same spot again the clamps are removed and the two ends of the stitch are tied together. The colon is now joined together, but there is no open lumen as the crushed ends are still shut. By invaginating the finger into the bowel above the line of union the lumen can again be re-established.

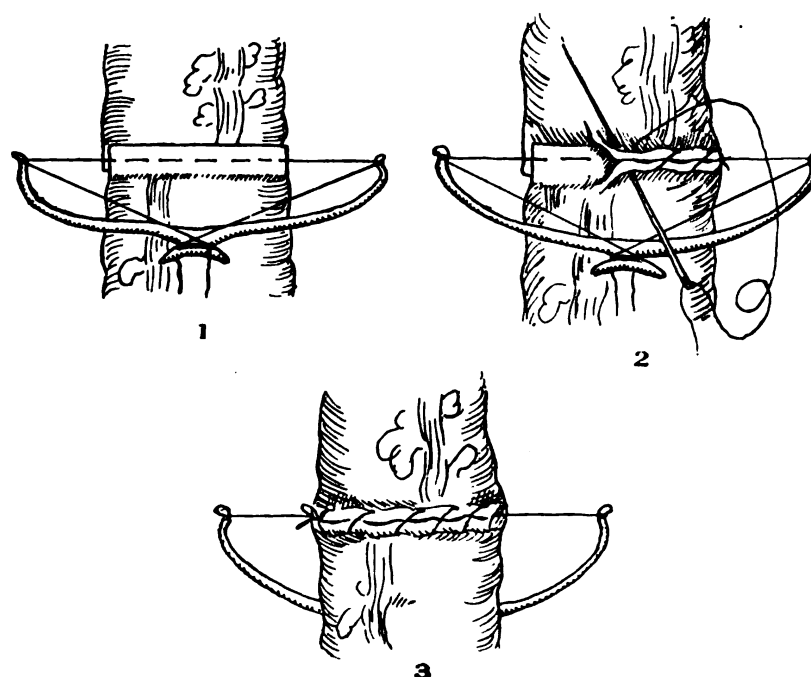


FIG. 7.—Martel's method of performing anastomosis. 1, ends of colon held in position by temporary stitch; 2, anastomosing stitch being inserted; 3, anastomosing stitch completed, temporary stitch is now withdrawn. (From "Diseases of the Rectum and Colon," by the author.)

An ingenious modification of this method has been devised by Martel. After the colon has been crushed and divided, the clamps are removed and the two crushed ends of the bowel are placed together one end on the top of the other. A stitch is then passed through both the crushed ends so as to fix them together and prevent them getting out of place. The ends of this stitch are attached to the extremities of a steel bow and fixed (as shown in the drawing) (fig. 7). The permanent stitch, which of course only takes up the peritoneal and elastic coats, goes all round the bowel and is tied off. After this is completed the steel bow is removed and the straight stitch is pulled out. The lumen of the bowel is subsequently re-established in the same way as in the previous method.

78 Lockhart-Mummery: *Resection and Anastomosis of Colon*

A very ingenious clamp has recently been sent me by Dr. Martel which enables the colon to be joined in a similar manner. There are three small steel clamps, which fit into a hinge. These are passed over the colon and the open ends of the clamp are then seized with a specially made handle and forced together. Small bolts then turn up and hold the steel clamps firmly together. The handle is now removed, the hinge detached and the centre clamp is opened and released. The bowel is then burnt through between the two remaining clamps and a subsequent anastomosis can be done in the same manner as in either of the previous methods described. This clamp is a most useful implement, and will certainly save a great deal of time in the resection of any large portion of the colon, as it can be left attached to the portion which is to be removed and will obviate the closing of two ends of bowel—namely, the ends of that portion of bowel which is going to be resected. I have also found this clamp useful in resecting the rectum.

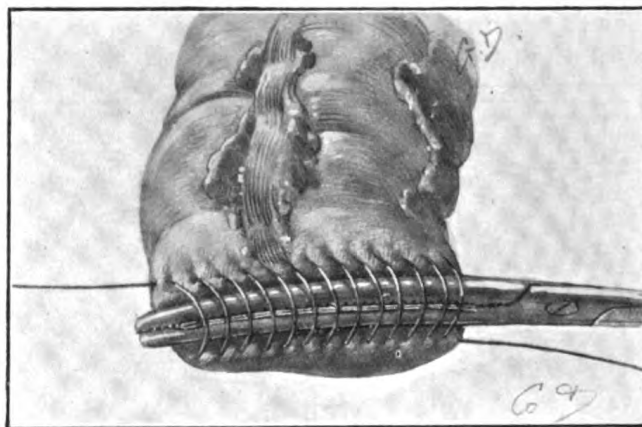


FIG. 8A.—Method of closing the end of the colon by sewing over a clamp. (From "Diseases of the Rectum and Colon," by the author.)

LATERAL ANASTOMOSIS.

I do not propose to describe this method of anastomosis, as it is too well-known, and in my opinion should be given up in favour of axial anastomosis.

METHODS OF CLOSING THE ENDS OF THE COLON.

This closure is necessary in operations on the large gut, such as colectomy or hemi-colectomy, abdomino-perineal excision of the rectum, and in lateral anastomosis. There are several good methods:—

(1) That which I prefer is a Mikulicz stitch of catgut passed backwards and forwards over a tapered crushing clamp after the bowel has been burnt through. This stitch, after the clamp has been removed and the ends of the stitch pulled tight, invaginates the crushed end into the bowel lumen (*see* figs. 8A and 8B). If the ends of the stitch are then tied together further security is obtained. It can still further be reinforced by a mattress stitch if thought desirable. This method has the advantage of being simple, and requires no handling of a septic surface.

(2) Another simple method, suggested by Abadie, consists in crushing the bowel and dividing it, and then seizing the extremities of the crushed end in two pointed forceps, and rolling the forceps in opposite directions inwards. A ligature is then placed round the crushed portion and tightened as the forceps are withdrawn.

(3) Klapp's method consists in rolling the crushing forceps round so as to make a sort of Swiss roll of the end of the bowel, and then inserting a few stitches to prevent the end from unrolling.

(4) There is yet another method—that of crushing the bowel with a tapered clamp, then sewing round and round the clamp, the clamp being removed before tightening the stitch. A supporting purse-string suture is certainly desirable in this latter method, as the crushed portion of bowel is left on the peritoneal aspect.

When the two ends of the colon are not of the same size, I think the simplest plan is (*a*) to slit up the smaller end of the bowel, along its dorsum,

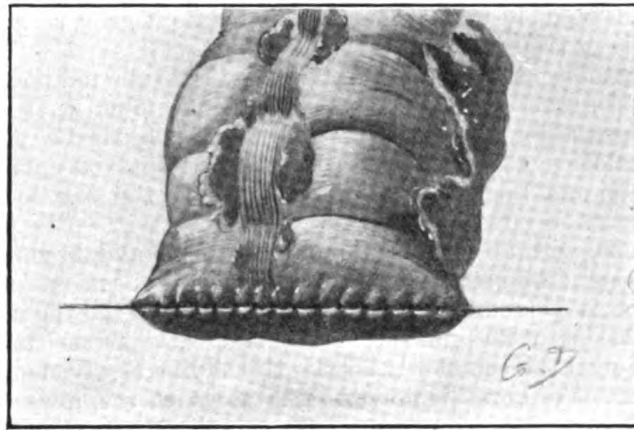


FIG. 8B.—Closure completed by withdrawing the clamp and pulling on the ends of the stitch. The ends are afterwards tightened. (From "Diseases of the Rectum and Colon," by the author.)

that is, its free edge, until the two ends are approximately equal in calibre. Another method (*b*) is that of cutting one end diagonally and one transversely. This is, in my opinion, objectionable, as the blood supply may be bad on the side of the transversely divided bowel. Lastly (*c*) there is the method of anastomosing the two ends and closing up the portion left in the large end, after the anastomosis is completed.

ANASTOMOSIS OF ILEUM TO LARGE GUT, OR ILEO-SIGMOIDOSTOMY.

This can quite well be done by axial union if the small gut is divided along its dorsum. The more usual method practised, however, consists in implanting the small gut into the sides of the large intestine, after closure of the end of the latter. This is the usual practice in ileo-sigmoidostomy.

So persuaded, however, am I of the importance of the ileocæcal valve in human beings, more particularly in preserving the anaërobic condition of the small bowel, that I intend in future, when in the performance of an ileo-colic anastomosis, as for instance when resecting the large bowel, to divide the

*

cæcum itself, and join the cæcum to the colon. Such a procedure does not seem to present any serious difficulties, and will have the advantage of preserving the ileocæcal valve intact. The advantages of preserving this valve appear to be more important than the slightly longer time this procedure may involve.

ANASTOMOSIS NEAR THE LOWER END OF THE PELVIC COLON.

The resection of a growth situated just above the recto-sigmoidal junction can be carried out under full use of the Trendelenburg position, and by free incision of the peritoneum to the outer side of the sigmoid, but considerable difficulty may be experienced in dealing with the bowel afterwards. Three possible procedures suggest themselves:—

(1) To close the upper end of the rectum and bring the stump of the sigmoid out of the abdominal wall as a permanent colostomy. This, while the easiest and safest method, deprives the patient of the use of a perfectly normal rectum.

(2) To remove the entire rectum by abdomino-perineal excision and bring the stump of the sigmoid to the anus. This, while it gives an excellent result, is a very serious procedure.

(3) To anastomose the ends of the bowel by the tube method. I described this method in the *Lancet* in 1908,¹ but subsequently found that Mr. Rutherford Morison, of Newcastle-upon-Tyne, had previously published a similar method in some gynecological reports. It has since been improved upon by Balfour of the Mayo Clinic, and his modification is certainly the one to adopt in performing the operation now.

A rubber tube, with an internal diameter of $\frac{1}{2}$ in. and having a lateral eye near the end, should be used. After the bowel has been resected, the open end of the tube is passed into the lower segment of bowel until it can be reached by an assistant and pulled out of the anus. The assistant then draws down the tube until the top end is level with the line of division of the bowel. The upper segment of bowel is now brought down to the lower and a rough end-to-end anastomosis performed. This completed, the tube is pushed up the bowel until its upper end with the lateral eye is some 6 to 8 in. above the line of union. Next, a stout catgut suture is passed through the colon wall and the rubber tube some 2 in. above the line of union, and tied so as to fix the bowel firmly to the tube. The tube is then gently drawn down by the assistant so as to produce a short intussusception. The invagination so produced should be sufficient to cover the line of junction completely. A few catgut stitches are inserted to prevent the bowel unrolling. After the abdomen has been closed the end of the tube is cut off outside the anus and a safety-pin put through it to prevent its retraction. It is nearly always possible to insert one or two stitches in order to produce the intussusception necessary. This operation is much safer if a tube is tied into the cæcum at the same time. It has been performed a considerable number of times, notably in the Mayo Clinic, where it has given satisfaction.

TOTAL COLECTOMY.

There is nothing very special about total colectomy to which reference has not already been made. I believe that in future surgeons will probably attempt to preserve the ileocæcal valve when it is not involved in the disease.

¹ *Lancet*, 1908, i, p. 1408.

The question as to whether the great omentum should be preserved, or removed in total colectomy is of some interest. Personally, I have always removed it with the colon. The fact that my cases of colectomy have so far been free from subsequent adhesions may possibly be due to this removal. Certainly, I have seen no bad results, and it is now from five to seven years since some of the cases were submitted to operation. I cannot argue the point from the number of cases upon which I have performed this operation, as they are not numerous enough. It would seem to be of some importance.

OPERATIVE RESULTS OF END-TO-END ANASTOMOSIS.

My private case-books show thirty-seven cases of resection of the colon for tumour with anastomosis. Twenty-three cases have been treated by end-to-end, or axial, union, with nineteen recoveries and two deaths. One of these patients died from acute dilatation of the stomach and gastritis due to the anæsthetic. This is a mortality of about 8·6 per cent. Fourteen cases were treated by lateral anastomosis, Paul's operation or Greig Smith's, with five deaths. I think these figures show clearly enough that axial union is just as safe as lateral anastomosis.

I may say that the average age of the last eleven patients upon whom I performed resection and axial union was 66·8 years, so that a mortality of under 10 per cent. cannot be considered excessive.

The Technique of Axial Anastomosis of the Alimentary Canal.

By CHARLES A. PANNETT, F.R.C.S.

THE proper assessment of the relative merits of an end-to-end or lateral junction of severed bowel is still a subject of controversy, but this does not concern us here. There are advocates of both methods. Convinced though we may be that, anyhow in the case of the large intestine, lateral anastomosis is fraught with less liability to leakage, there are occasions, when, owing to the fact that sufficient overlapping cannot be attained without tension, axial suture is forced upon us. It then becomes a very important matter to decide how this operation shall be performed. The mesenteric angle was always regarded as a dangerous area by the older surgeons, and, in spite of recent statements to the contrary, this belief has been substantiated by animal experimentation. Thus A. L. Soresi,¹ in literally hundreds of experiments, found that, without exception, an abscess always forms in the mesenteric angle when an axial suture is performed in the usual fashion. This small abscess however, usually burrows in the direction of least resistance which fortunately leads to the lumen of the bowel. The two non-peritonealized areas do not adhere together as firmly as the rest of the circumference of the bowel. This difficulty of union is due not only to the absence of the peritoneal membrane, but also to the almost inevitable interference with the blood supply at the mesenteric angle by the suture. Skiagrams of opaquely injected intestine after the performance of end-to-end suture, show that there is a very abundant vascular anastomosis in the wall of the gut, but just in the vicinity of the mesenteric angle the blood supply is less free. The anti-

¹ *Ann. Surg.*, 1919, lxi, p. 613.

mesenteric margin of the intestine has the best blood supply, so that there is no need to cut the ends obliquely from the fear that it will be insufficient in this area. It was Greig Smith who pointed out that the strongest adhesions in the abdomen takes place when a peritoneal covered surface comes in contact with an area devoid of peritoneum. In 1899 it occurred to J. E. Frazer,¹ therefore, that advantage might be taken of this fact in intestinal anastomosis. His suggestion was that by rotating slightly the two cut ends around their own axes in opposite directions, the raw mesenteric angle of one end would come in contact with a peritoneal covered surface of the other. He performed the operation upon the dead body. I have acted

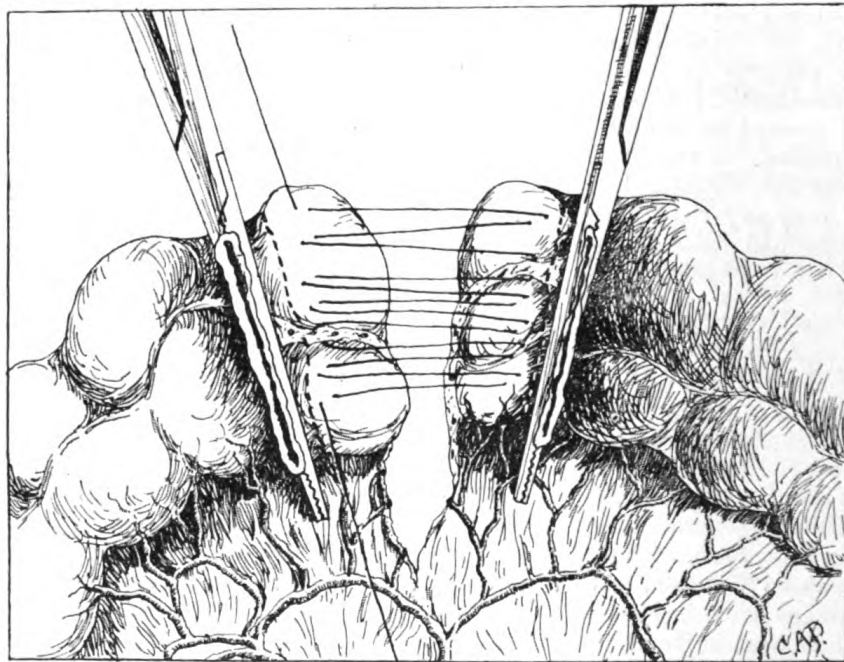


FIG. 1.

upon this suggestion and have found that both in the small and large intestine the operation is not only feasible in life, but also a very satisfactory one.

At the site of anastomosis the ends of the bowel are crushed in two large Kocher's forceps, so that the lumen is flattened in a plane at right angles to that of the mesentery, in such a way that when apposed the mesenteric angles will not be opposite to one another (*see fig. 1*). The posterior seromuscular suture is inserted first. This is important. It is much more difficult to get in this line of suture properly if the through-and-through stitch is done first and the seromuscular one afterwards. By cutting along the dotted lines the clamps are got rid of and the lumen of the bowel opened. Gross soiling of the area of operation is of course prevented by the usual rubber-covered clamps applied at a few inches distance away. The through-and-through

¹ *Lancet*, May 13, 1899, p. 1285.

suture is then started as in fig. 2 and carried right round the whole circumference, after which the seromuscular stitch is completed by continuing it round the anterior half of the intestine. A few stitches in the mesentery will complete the anastomosis.

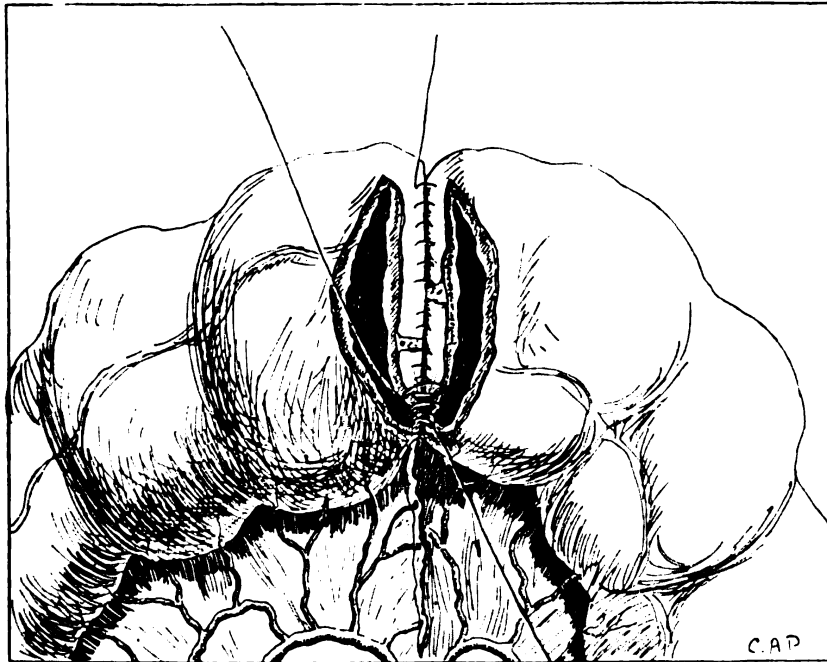


FIG. 2.

DISCUSSION.

Mr. A. H. BURGESS was pleased that Mr. Lockhart-Mummery had so strongly emphasized the importance of careful preparation of the bowels for several days at least before the operation of resection of the colon. If one had the slightest difficulty in thoroughly clearing out the bowel by laxatives and enemata, or if there were any degree whatever of obstruction present it was far better to perform a preliminary cæcostomy ten to fourteen days before the actual resection. He did not think the explanation offered by Mr. Mummery of the greater difficulty in securing firm union after end-to-end suture in the colon, as compared with the small intestine, was the real one, since the vessels coursed transversely in the wall of the small as well as in that of the large gut and would run an equal risk of being occluded by the suture near the mesenteric angle. There were other factors, for example the fluid, rapidly moving, and the comparatively slightly septic contents of the small gut. The close apposition of the peritoneal to the muscular coat (except at the mesenteric angle) in the small intestine was in striking contrast to that in the colon where fat was often present in considerable amount between the two, not only at the appendices epiploicæ, but elsewhere. This fatty tissue prevented very accurate apposition and did not possess the best of healing powers. After resection of the colon, therefore, he preferred to close each end and restore continuity by lateral anastomosis, and he was interested to find that Bevan, of Chicago,¹ found, both from his own practice and cases collected from the literature,

¹ *Journ. Amer. Med. Assoc.*, 1920, lxxv, p. 283.

that the mortality after a properly performed side-to-side was only half that after a properly performed end-to-end anastomosis. He thought some of Mr. Mummery's success with his end-to-end union might be ascribed to his present practice of always performing cæcostomy as the concluding step, and thus throwing less strain upon the line of suture in the colon; this was strongly advocated by Sir Harold Stiles, in his Presidential Address, before the Edinburgh meeting of the Association of Surgeons, in 1921, and was a most valuable procedure. The really difficult cases were those of lower sigmoid and recto-sigmoid growths, where, owing mainly to the depth and the shortness of the lower segment lateral anastomosis was impracticable; here he had received great help from Balfour's rubber-tube method. Considerable ingenuity had recently been expended in devising methods of anastomosis not necessitating exposure of the lumen of the bowel, some of which Mr. Mummery had mentioned; about twelve months ago he saw Professor Schoemaker (of The Hague), perform very neatly and rapidly three such resections of the colon, using his own special forceps. The chief objection to all these methods, apart from their necessitating the use of very special appliances, was that one had to trust to the crushing of the bowel wall for the arrest of hæmorrhage (assisted in some of them by cauterization of the crushed portion); there was no circle of sutures passing through the entire thickness of the wall and securely controlling the vessels as in the ordinary methods of suture. He knew of one case where severe, though fortunately not fatal, hæmorrhage into the bowel had occurred from this cause.

Mr. LOCKHART-MUMMERY (in reply) said that he believed axial union would soon entirely replace lateral anastomosis in resection of the colon. He thought his mortality figure of 8·6 was the lowest that had yet been published, and it had been obtained by axial union. He agreed with Mr. Burgess that the routine use of temporary cæcostomy and catgut sutures had a good deal to do with the successful result. He agreed that the continental method of trusting to crushing for hæmostasis of the cut edges of the bowel did not appeal to him.

Section of Surgery.

President—Mr. JAMES BERRY, F.R.C.S.

The Sites of Origin and Methods of Growth of Fibro-adenomata of the Breast.

By Sir G. LENTHAL CHEATLE, K.C.B., F.R.C.S.

THIS paper is published in full in the *British Journal of Surgery*, vol. x, p. 436 (April, 1923), with the title: "Hyperplasia of Epithelial and Connective Tissues in the Breast: its Relation to Fibro-adenomata and other Pathological Conditions."

Demonstration on the Immediate Microscopic Diagnosis of Tumours at the Time of Operation.

By ERNEST H. SHAW, M.R.C.P.

Dr. SHAW gave an account of the method he employs, which is described in full in the *Lancet*, 1923, i, p. 218. He then proceeded to give a demonstration of his method. Fresh tissues for this purpose were provided by Dr. Bowell, Mr. V. Z. Cope, and by Dr. Shaw himself.

Two Cases of Sarcoma of the Small Intestine.

By CECIL ROWNTREE, F.R.C.S.

CASE I.—SARCOMA OF ILEUM.

M. K. COMPLAINED of loss of weight and sharp attacks of abdominal pain and vomiting recurring about once a week, and lasting about twenty-four hours. No physical signs could be detected, but a barium meal showed delay in passage through the small bowel.

Operation, May, 1921: A bobbin-shaped growth, 3 in. in length, was found in the ileum, about 2 ft. from the ileocaecal valve. There were numerous pale, waxy glands in the mesentery and along the aorta. The growth was resected, and the patient is still in fairly good health, although only a few of the glands could be removed.

Pathological Report.—The portion of small intestine submitted shows an annular constricting growth, exuberant and ulcerating towards the lumen of the gut, but not quite completely blocking it. The microscopic section shows the tissue to be sarcomatous.

CASE II.—SARCOMATOUS CYST OF JEJUNUM.

A. H. had noticed for a few months only some enlargement of the abdomen with slight indigestion. The swelling had rapidly increased during the preceding forty-eight hours, and had become painful. Examination showed a cyst about the size of a five months' pregnancy, presumably a twisted ovarian.

Operation, May, 1922: Laparotomy revealed a large, smooth cyst with recent omental adhesions. No pedicle could be discovered below, and further investigation showed that the only attachment was to the jejunum, about 2 ft. from the duodeno-jejunal flexure. The implicated loop of bowel was resected, and the patient remains in good health.

Examination of the specimen shows a unilocular thick-walled cyst, filled with blood-stained fluid, and with many patches of recent blood clot on its wall. The cyst grows from the antimesenteric wall of the jejunum, and at the point of attachment, about 1 in. in diameter, is a pale, homogeneous growth projecting somewhat into the lumen of the bowel, and to a greater extent into the cavity of the cyst. The histological picture is that of sarcoma.

Case of Large Spindle-celled Sarcoma arising in the Mesentery of a Coil of Ileum successfully removed at Operation.

By FRANK KIDD, M.Ch., F.R.C.S.

HISTORY: Patient, a male, aged 42, examined on November 27, 1922, stated that four months ago he first began to feel an aching pain in the abdomen just below the umbilicus. For the last six weeks the pain had been continuous, and he had noticed that his abdomen had become larger. Dr. Robertson, of Boston, Lincolnshire, examined him, and thought he could feel a smooth swelling which he took for a distended bladder. The stream of urine was unaltered, and there had been no increased frequency of micturition and no alteration in the character of the urine. The stools had been normal, showing no evidence of blood, and he had experienced no symptoms of intestinal obstruction. He had never had venereal diseases nor any other serious illness.

Examination: The patient had a stout abdominal wall, which rendered palpation difficult. An indefinite elastic rounded swelling could be felt in the centre of the abdomen below the umbilicus, which felt like a distended bladder. The urine was clear and healthy, and passed in a good stream. Rectal examination did not reveal anything abnormal. The urethroscope showed that no stricture was present. No evidence could be detected of disease of the nervous system.

Cystoscopy: A full-sized rubber catheter entered the bladder without difficulty and drew off 6 oz. of urine. After this manoeuvre the swelling could still be felt as before. Inspection of the bladder (8 oz. distension) revealed a healthy bladder wall, no evidence of paresis and pouching, no intravesical enlargement of the prostate. I could not detect any evidence of tumour

pressing into the bladder wall. X-ray examination did not reveal anything abnormal. I suggested a provisional diagnosis of dermoid of the urachus, or fibroma of the posterior sheath of the rectus muscle, and advised an early exploration.

Operation : Exploration was carried out on December 7, 1922, under open-ether anæsthesia. An incision 4 in. in length was made 1 in. to the right of the middle line below the umbilicus, and the right rectus muscle was turned outwards. The tumour proved to be a large elastic mass the size of a large cocoa-nut lying in the mesentery of a coil of ileum. It rested on the pelvic floor behind the bladder, but had not obtained any adventitious attachment to the surrounding structures. It was covered with blood-vessels of great size, mostly veins the size of one's little finger, running up from the mesenteric vessels and spreading out over the surface of the tumour. A coil of ileum was buried in the mass, but was only partially obstructed thereby. It was clear that the tumour had its origin in the mesentery rather than in the intestine itself, yet it was obvious that the coil of intestine would need resection if a proper clearance of malignant tissue was to be effected. The coil of small intestine with a wedge of mesentery was clamped, and the tumour, with about a foot of ileum and its included mesentery, was then cut away in one piece. After ligation of the blood-vessels the lumen of the intestine was restored by end-to-end suture with an inner layer of catgut and an outer layer of silk, and the gap in the mesentery closed. A search of the abdomen revealed no other lesion, so the abdominal wall was closed and the patient returned to bed. The operation lasted one hour.

Course : The after-treatment consisted of rectal infusion and a single dose of morphia. There was no shock and the patient rallied quickly. No aperients were given until the fifth day. By that time the bowels had not acted and the accumulation of flatus was beginning to be troublesome. The exhibition of pituitary extract and a turpentine enema brought away a quantity of flatus, and on the sixth day 5 gr. of calomel were given, followed by an ounce of castor oil. This resulted in several copious evacuations and from that time the bowels acted naturally every day and without difficulty. The stitches were taken out on the twelfth day, the wound being then strapped, and the patient was allowed up on the fourteenth day. He left the home on the nineteenth day feeling fit and well, and when heard of recently he had resumed his occupation and has had no further trouble.

Pathology : The tumour (which was exhibited) proved to be a large spindle-celled sarcoma arising in the mesentery. Sections were cut and reported upon by Dr. Fletcher, of 6, Harley Street, as follows : "This large growth surrounding, but not arising from, the small intestine, has the histological structure of a large spindle-celled sarcoma. The cells are quite large and in some places show a tendency to be arranged in bundles. These facts, and the encapsulation of the growth, suggest that, though malignant, it is not highly so."

COMMENTS.

Rounded swellings in the middle of the hypogastrium met with in the male are usually swellings of the bladder. Any other origin is of rare occurrence. I have seen a small number of cysts and tumours arising in the urachus, also fibromata arising from the posterior rectus sheath. I have also seen sarcomata arising from the prostate or the fat of the cavum Retzii giving rise to such a swelling. I have also seen chronic abscesses arising from a gland in the urachal fat.

The question of cystoscopic diagnosis of pelvic tumours is interesting. It is usually possible to see with the cystoscope a pelvic tumour projecting into the bladder wall. For instance, it is not at all uncommon to see a carcinoma of the pelvic colon attached to the bladder on the left superior wall. In such cases there is also a characteristic appearance of oedema which is almost specific. I have also seen several cases of diverticulitis of the colon opening into or projecting into the bladder. In the case of women one can usually see the projection of the uterus and can often see the smooth projection of an ovarian cyst. Probably in this case if I had distended the bladder more fully at the time of inspection I should have seen a smooth projection into the posterior bladder wall.

As regards the operation itself, it is always helpful when one sees a tumour surrounded by veins of great size. It usually means one is dealing with a malignant tumour, and should lead one to carry out a wide resection based on that fact.

I believe that end-to-end anastomosis is always to be preferred to lateral anastomosis whenever we are dealing with two sections of gut that are of more or less similar size. There is a tendency for surgeons to carry out lateral anastomosis as it is certainly an easier operation to perform, but figures obtained from statistics at the London Hospital suggest that end-to-end anastomosis is both a safer operation and also that it leads to far better functional end-results.

The same figures suggest that it is not wise to give strong aperients after such an operation, but that it is better to leave the bowel at rest and to its natural resources and only use purgatives after the lapse at least of four or five days if no action has occurred naturally before that time.

As regards the origin of the tumour, sarcomata occur in rare instances in any portion of the small intestine. They occur fairly regularly in the post mortem room at the London Hospital. They are usually polypoid growths which project into the lumen of the gut and may lead to intussusception. From examination of the tumour it is clear that this sarcoma did not arise in any portion of the wall of the small intestine, nor does it appear to have originated from a lymphatic gland. The only conclusion at which I can arrive is that it must have arisen from the connective tissue in the mesentery itself.

Section of Surgery.

SUB-SECTION OF PROCTOLOGY.

President of Sub-section—Sir CHARTERS SYMONDS, K.B.F., C.B., M.S.

**Patient upon whom an Operation was performed in June, 1920,
for Cancer of the Rectum, by the Abdomino-anal Method.**

By HERBERT H. BROWN, O.B.E., M.D., F.R.C.S.

PATIENT, a male, aged 60, was suffering from an ulcerating carcinoma at the upper part of the rectum, involving three-quarters of the lumen. It was thought to be a suitable case for an attempt to be made to retain sphincter control.

The operation was performed as follows: The patient was placed in the Trendelenburg position (chloroform anæsthesia). The abdomen was opened in the middle line, and the bowel divided 6 in. above the peritoneal reflexion on the rectum. The proximal and distal ends of the colon were inverted and closed by suture (Moynihan's method). The lower portion of sigmoid and rectum was detached as far as could be done from within the abdomen, keeping close to the sacrum so as to include glands in the mesentery. The sigmoid was then detached by dividing its mesentery, care being taken to avoid injuring the vascular supply, until 12 in. were freed.

A tape was tied to the upper end of the distal and lower end of the proximal portions of the bowel. Both were pushed down as far as possible into the pelvis. The peritoneum was then sutured to the upper part of the freed portion of sigmoid colon to close the abdominal cavity, the detached recto-sigmoid portion being below it in the pelvis, and the abdominal wound sutured in the usual manner.

The patient was then put in the lithotomy position. The sphincter was dilated, and the skin round the margin of the anus divided with scissors, the incision being extended between the external sphincter and the bowel, as in Whitehead's operation for hæmorrhoids.

The bowel was completely divided with scissors just above the sphincter, and was then separated with the finger and scissors from the prostate gland and other tissues. The levator ani muscle was divided between forceps piece by piece and ligatured, so as completely to free the lower part of the rectum. This part of the bowel—the rectum and lower 6 in. of sigmoid—was drawn out through the sphincter, and the sigmoid drawn down by the attached tape through the sphincter, which was nowhere divided. The closed end was cut off and the wall of the bowel sewn to the skin round the anal margin.

[April 11, 1923.]

The patient suffered from a moderate amount of shock, but made a very good recovery. He is now, after the lapse of nearly three years, in perfect health; he has gained 36 lb. in weight, and sphincter control is quite satisfactory.

The specimen shows a typical ulcerating adenocarcinoma commencing at the level of the peritoneal reflexion.

Case of Early Tabes Dorsalis.

By J. P. LOCKHART-MUMMERY, F.R.C.S.

THE patient was a man aged 40. Eighteen months before I saw him he noticed that he had lost complete control over the anus and was liable to have accidents unexpectedly. This has been getting gradually worse. The bowels acted regularly, and apart from this he was perfectly normal. No loss of control over the bladder. No prolapse or bleeding. He was an active man and lived a normal life. He had not noticed any other symptoms of any kind, and there was no pain.

On examination: The sphincter appeared normal, and contracted normally. No piles, and nothing found in rectum. Nerve reactions: Knee-jerks completely absent, and pupils did not react to light at all. Examination of skin in anal region showed there was almost complete loss of sensation in this area for about 2 in. round the anal orifice. Patient could not feel a prick in this area, but the sensation to heat was normal. On examining his forearms I found that there were areas of impaired sensation on the inner side of both forearms when pricked with a needle, but that the sensation to heat was normal. On making the patient stand up with his eyes shut and feet together, he swayed about, but did not fall.

Blood-examination showed a well-marked positive Wassermann reaction, and on careful inquiry into his history I found that twenty-two years previously he had had syphilis, for which he was only treated for ten weeks.

The case was, therefore, clearly one of early tabes dorsalis, but the symptoms were certainly unusual, and might easily have led to an error in diagnosis. The case is chiefly of interest from the diagnostic point of view as illustrating what must be a very rare form of loss of control over the anus. The loss of control was clearly due to loss of sensation, and not to any muscular weakness. The patient could walk perfectly well, and had not a tabetic gait.

Sir CHARTERS SYMONDS, K.B.E., C.B., M.S., read a paper on "Nephrostomy for the Relief of Inoperable Rectovesical Fistula."

Mr. DONALD ARMOUR, C.M.G., F.R.C.S., showed a Case of "Syphilitic Disease of the Anus and Rectum in a Young Woman."

Section of Surgery.

SUB-SECTION OF PROCTOLOGY.

President of Sub-section—Sir CHARTERS SYMONDS, K.B.E., C.B., M.S.

DISCUSSION ON ULCERATIVE COLITIS.

Sir HUMPHRY ROLLESTON, K.C.B.

THE subject of chronic ulcerative colitis, though mentioned in Wilks and Moxon's "Lectures on Pathological Anatomy" in 1875, owes its recognition in great measure to the writings of Sir William Hale-White, the President of this Society, who gave the first of his several descriptions of the condition in 1888 [21]. There was a discussion at the Section of Medicine of the Royal Society of Medicine in 1909 [17] with statistics of 312 cases so diagnosed in the hospitals with teaching schools in London. In 1911 simultaneous discussions on colitis were held at the Harveian Society and at the Chelsea Clinical Society. It is a disease on which British physicians have made the great bulk of the contributions. The question naturally arises whether, in the years since these discussions, any important advance in our knowledge has been made. From the bacteriological and medical points of view it must be admitted that there is little to say. From the diagnostic standpoint the increasing appeal to the sigmoidoscope is important, and as regards treatment, the increasing stress laid on the value of appendicostomy is the most noticeable feature.

Scope of the Term.

In introducing a discussion it is important to define its scope, and in this instance it is somewhat difficult to limit the field. By a process of exclusion, some idea may be arrived at; it will be agreed that many forms of ulceration of the colon may be eliminated as not coming within the meaning of ulcerative colitis. Thus exogenous ulceration due to an abscess rupturing into the bowel, e.g., an appendix abscess, malignant ulceration, stercoral ulceration, diverticulitis, typhoid and paratyphoid, "uræmic" and tuberculous ulceration, bacillary and amœbic dysentery may be counted out. Ulceration due to acute colitis of known toxic origin, such as that due to corrosive sublimate poisoning, and the various forms of parasitic colitis and of infective proctitis of local origin and extent should not be included in ulcerative colitis. The ulceration exceptionally found in pneumonia and after accidents (those causing paraplegia having suggested a trophic disturbance) should also be excluded. A few words should be added about what was formerly known in this country as the ulcerative colitis of asylums, and in America as institutional dysentery. Early

[May 9, 1923.]

in this century Vedder and Duval (1902) [20] in America, and in this country Mott and Durham (1901) [14] and Eyre (1904) [3], showed that it was bacillary dysentery, thus confirming Gemmel's view (1898) [5]. Sir Frederick Mott has most kindly provided me with information as to the present position of knowledge of this disease in the London County Council Asylums: since the term "colitis" and the idea that it was a non-infectious disease due to trophic changes have been given up, the type of disease has altered so that severe and acute cases are less frequent. Outbreaks are almost certainly due to the presence of chronic carriers, and his assistant, Captain A. S. Mann, who has done much serological work on the subject, finds that the responsible organism is Type X of the Oxford series of Flexner dysentery organisms. It appears advisable to exclude from the group of cases that may for convenience be called ulcerative colitis, the cases often arising in epidemic forms in asylums, and to confine our attention to the sporadic cases seen in the ordinary civil practice which are not, from a laboratory point of investigation, bacillary dysentery.

Bacteriologically, ulcerative colitis is not a specific disease, for numerous organisms, *Bacillus coli*, coliform organisms, *Bacillus pyocyaneus*, pneumococci, and streptococci may appear to be the predominating and causal agents. A bacteriological diagnosis is comparatively easy in the acute stage, difficult when the condition has become chronic. The opportunities for secondary infection in the colon are most favourable, and it is probable that ulceration of the colon, originally due to one micro-organism, may be kept up by the combined attack of different bacteria, or to successive varieties or strains of pyogenic cocci. Lockhart-Mummery in 1911 [10] drew attention to the gravity of cases of ulcerative colitis following tropical dysentery in which the original infection has died out. It is indeed difficult to be certain that the original infection has entirely disappeared; for although the stools may not give any evidence of amœbic or bacillary dysentery, the organism may still be present in the walls of the colon. Dr. J. W. McNee has shown me a specimen of the colon with *Entamœba histolytica* in the blood-vessels from a man who had never been out of England and presented the symptoms of ulcerative colitis with absence of *Entamœba histolytica* from the stools. It is not surprising that in such cases specific treatment, for example, emetine for latent amœbiasis, may fail to be curative, just as iodides may prove disappointing in an actinomycotic lesion with secondary streptococcic invasion. Further, ulceration of the colon is often a stage in an acute or chronic inflammation of the bowel, and not, as far as is known, a process due to the advent of a special micro-organism. The acute phase, as in hæmorrhagic colitis, may pass off or prove fatal before ulceration is established (vide Grant [6]). It must indeed be admitted that ulcerative colitis is not a disease in the strict sense of the word, any more than rhinitis or bronchitis is, but that it is a syndrome with fairly constant clinical manifestations and anatomical changes, which may be excited by different factors.

Ætiology.

As the causes favouring infection of the colon and widespread ulceration are obscure, reference may be made to McCarrison's [13] and Findlay's [4] observations showing that a devitamized diet renders animals less resistant to infection; thus, McCarrison found that healthy monkeys, carriers of *Entamœba histolytica*, may, as the result of a deficient diet, develop amœbic dysentery, and in an infected area organisms of the hog cholera group attack pigeons on a devitamized diet and spare those properly fed. Diminution or deprivation of

the water-soluble vitamin B, such as occurs in food consisting of excess of starch and fats, favours infection. The whole alimentary tract of the experimental animals shows atrophy and necrosis; and acute colitis, but not ulcerative colitis, was described. It is, however, easy to understand that in man a diet deficient in protein and water-soluble vitamin B might so reduce the healthy resistance of the mucous membrane of the colon that micro-organisms previously harmless, such as *Bacillus coli*, would become pathogenic; in short, the carrier would become a patient. It is also conceivable that changes in the mucous membrane of the colon may so alter their environment that bacteria unable to multiply in ordinary conditions would grow without inhibition, in other words, that the intestinal flora would change. Bassler [1] speaks of a coliform organism of enhanced virulence as the *Bacillus pseudodysentericus coli*. Nearly thirty years ago the late Leopold Hudson [8] suggested that swine fever and ulcerative colitis might be identical, and quoted a few instances in which the disease had followed the consumption of pork. He also pointed out similarities between the morbid lesions and the clinical manifestations in the two diseases. Professor F. Hobday tells me that no definite proof has as yet been brought forward in support of any connexion between these human and swine diseases.

Complications.

Is the incidence of perforation lower now than it was formerly in this country? Among sixteen cases selected from those recorded in the *Transactions* of the Pathological Society of London (1885-1907) it occurred in five, and out of thirty-three fatal cases (probably including the above) collected in 1910 by Lockhart-Mummery [10], nine showed perforation and general peritonitis; possibly some of the cases were published on this account; or has the type of the disease changed either from alteration in treatment or from less obvious causes? Among twenty-two selected cases of ulcerative colitis examined after death at St. George's Hospital between 1890 and 1922, Miss N. Schuster found four cases of perforation, two being in the "nineties" and two during the war; so far as this small group goes there does not appear to be any special alteration in the incidence of perforation. In very rare instances perforation is found after death, with but little evidence of peritoneal response, and it is probable that the perforation was an agonal phenomenon when the reactive powers of the body were reduced to a minimum. Multiple perforations have been very rare. At the Mayo Clinic, Logan [11] found that among 117 cases of chronic ulcerative colitis, which appear to have mainly followed dysentery, perforation was not uncommon and was a cause of localized abscess and not necessarily of general peritonitis. Hurst [9], on the other hand, found that localized abscess is exceptional and general peritonitis very rare, and that the latter when present is due to direct spread of infection through the walls of the colon and not to perforation. He states that stricture from cicatrization of the healing ulcers never occurs, and other experience in this country supports this conclusion; among the sixteen selected cases from the Pathological Society there was one only in which any mention of narrowing is made; in that case the descending colon was described as much contracted and thickened; no stricture was found among the twenty-two cases analysed by Miss Schuster; Logan [11], however, definitely states that stricture is likely to occur. Possibly the nature of the Mayo Clinic cases are not exactly the same as of those in this country. Arthritis, well recognized in bacillary dysentery, also occurs in a small percentage of cases of ulcerative colitis. Among Yeomans' [22] sixty-five cases arthritis or joint pains were noted in four. Peripheral neuritis has also been recorded. Ulceration in the small intestine is most exceptional.

Clinical Features.

The sexes are equally affected, and the incidence falls chiefly on young adults and in early middle life; among Logan's 117 chronic cases sixty-five were under 30 years of age, and only eight over 50. The onset may be sudden or insidious, and the course of the illness also acute or chronic. Acute colitis due to one micro-organism, such as the pneumococcus or streptococcus, or dysentery bacillus, may be succeeded by a chronic infection by a different bacterium. The first symptoms are usually those of colonic irritation—frequent stools, sometimes preceded by constipation. In the acute cases the temperature is raised; in chronic cases it is normal or even subnormal, except for elevations corresponding to exacerbations. The leucocyte count appears to vary; in some instances there is a leucocytosis, in others it is absent. As would appear probable (Price-Jones [16]), on the assumption that there is a coliform infection, a lymphocytic increase has been reported (Horder, quoted by Hurst [9]); but the blood picture would naturally differ with exacerbations and intermissions in the chronic form. Anæmia with 50 or even 20 per cent. of hæmoglobin is partly due to loss of blood, partly to toxins absorbed from the colon. The number of stools varies from three to over twenty in the twenty-four hours, but sometimes there are periods of constipation. The number of evacuations are less than in tropical dysentery, and tenesmus, which depends on the presence of ulceration low down in the rectum, is inconstant; it occurred in one-third of Logan's cases [11]. The excreta contain blood, pus, mucus, and sometimes sloughs of the mucous membrane. Onset with hæmorrhage has been correlated with the commencement of ulceration low down in the colon (Hawkins [7]). Abdominal discomfort is commoner than actual pain, the latter chiefly occurring before the bowels act or when there is much flatulence; local tenderness on deep pressure over the colon is variable. The importance of a sigmoidoscopic examination does not at the present day need emphasis; though it only provides information about part of the large intestine, that portion generally shows ulceration if it is present anywhere. The ulcers are indistinguishable from those of bacillary dysentery. Logan [11] gives a number of skiagrams showing interference with the peristaltic waves, which become lengthened, more superficial, with rounded instead of sharp edges, so that eventually the fibrosed colon appears as a thick-walled tube without haustrations, often with stenosis. Hurst [9] describes mottling of the colon, which is often abnormally narrow from spasm. Appetite and digestion are fairly maintained, but loss of weight and weakness are induced by the diarrhœa and toxæmia. The mortality is high, about 50 per cent., and Lockhart-Mummery [10] estimates that it reaches 78 per cent. when treated medically. The cases are prone to relapse. The chronic cases must be diagnosed from carcinoma, tuberculous and follicular ulceration of the colon, the sigmoidoscope being the obvious means of so doing.

Treatment.

Many methods of treatment have been employed.

Dietetic.—As the stomach and small intestine are practically always free from ulceration, and as digestion and absorption are fairly well carried on, it is unnecessary to restrict the diet to slops and soft food. As deficiency in the antiscorbutic vitamin C has been shown experimentally to reduce the resistance to infection (G. M. Findlay [4]); and as McCarrison's [13] observations suggest that lack of water-soluble vitamin B favours the incidence of inflam-

mation of the alimentary canal, it would appear reasonable to arrange a diet containing a sufficiency of antiscorbutic food and a fair quantity of protein. Soured milk with living cultures of *Bacillus bulgaricus* has naturally been given a trial, but Hurst [9] did not find that it had any beneficial effect.

Drugs.—The large number of drugs employed is in itself a reproach to purely medical treatment. Intestinal antiseptics, such as salol, bismuth salicylate, β -naphthol, small doses of mercury or calomel, benzyl benzoate, gentian-violet, acriflavine, are on the whole disappointing. Pomegranate bark and simaruba have also been tried; and more recently Logan [11] remarks of kaolin that it "was of no benefit whatever, although it is advised strongly by German writers." Finely pounded charcoal has been useful in reducing flatulence and obviating colic. Liquid paraffin by the mouth has been recommended as forming in the colon a coating in which bacteria cannot live (C. J. Macalister [12]); and olive oil has been advocated both orally and by enemas (Logan). Lavage of the colon with antiseptics, such as boric acid, acetozone, argyrol, nitrate of silver, albargin (silver nucleinate), allantoin, emulsion of sulphur, has been much employed; but, as the whole of the colon cannot be thus satisfactorily cleaned, appendicostomy or cæcostomy has largely superseded rectal injections as an effective means of washing out the colon. Einhorn employs a "jointed intestinal tube" 15 to 20 ft. in length introduced by the mouth and carried, under the guidance of X-rays, into the cæcum, where it is left *in situ* for days as a means for getting a result comparable to that of cæcostomy; this procedure is on its trial. The respective merits of appendicostomy and cæcostomy have been much discussed; when the appendix is obliterated, or, for other reasons, cannot be found, a valvular cæcostomy can be made; but neither this procedure nor appendicostomy gives the colon that rest from the contact of fæces which is insured by an open cæcostomy or a colotomy. In 1911 Sir D'Arcy Power [15] took the cautious view that the improvement after appendicostomy was mainly due to the prevention of toxic absorption and keeping the bowel clean, and stated that the ulcerative process continues until the patient becomes naturally or artificially immune (by means of vaccine). Hurst considers that appendicostomy halves the time of illness that would be experienced under medical treatment, and surgical opinion is strongly in its favour. To give operation a fair chance it should be done before the patient is debilitated, and according to W. G. Spencer [18] medical treatment should not be persisted in for more than three months. Short-circuiting the ileum into the pelvic colon has the serious drawback that the rectum is often affected; the radical means of providing physiological rest to the colon is a complete transverse ileostomy so as to exclude the colon; Stone [19] recommends this in combination with a separate appendicostomy for irrigation of the colon.

Vaccines have been extensively employed; few reports are enthusiastic, and the difficulty of obtaining the causal organism is considerable, even with the help of serological reactions. An autogenous *Bacillus coli* or coliform vaccine has usually been employed; but should not be given during the acute exacerbations of this chronic infection, as the condition may thus be seriously aggravated. In 1909 H. P. Hawkins [7] advocated anti-dysenteric serum, especially in acute cases, but without giving any statistics as to its value. In 1921 Hurst [9] recorded very striking results from the intravenous injection of multivalent anti-dysenteric serum in two patients who did not show dysenteric bacilli in their fæces. The healing of the ulcers as watched by the sigmoidoscope occurred in one of the cases in five days after the first injection of serum, and

as the blood did not agglutinate any stock dysenteric organism, the question arises how far the curative action was due to the horse serum apart from its specific anti-dysenteric content.

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Sir THOMAS HORDER.

I may have made more dogmatic statements with regard to this condition ten years ago. In my experience ulcerative colitis is found more commonly in women. Of ten recent cases in private practice eight were in the female sex; of five hospital cases only one was a male. The disease favours young middle age, 25 to 35. Among ætiological factors I would mention enteroptosis, circulatory factors, and infections. Some of the patients are found to have a cardiac lesion dating from a previous infection. With regard to bacteriology I would say that streptococcal infection—not only *Streptococcus faecalis* or *viridans* but also long-chained streptococci—should come first, with the colon bacillus much later. Certain clinical points deserve mention. There is often an intense degree of anæmia. The spleen is sometimes enlarged, and this may be due to an infarct that does not suppurate. The liver may be enlarged. In one case in which I had the opportunity of seeing this organ during cæcostomy it was pale and fatty and reached nearly to the umbilicus. Within two months of the operation it went back to its normal level. Ulcerative colitis has a great tendency to recur. The doctor is often consulted after there have already been several attacks. Quite a number of people having normal-looking stools suffer from chronic ulceration. When the physician thinks he has cured the patient and has examined by means of the sigmoidoscope, quite frequently there are still many ulcers present. What determines the recurrences which are such a marked feature in the disease? I think that ulcerative colitis is not so much a disease of the colon as a disease of the patient. It is a disease largely due to low resistance, just as are phthisis and malignant endocarditis. With regard to treatment, if the patient be in such a condition that operative treatment must be tried, the colon may be drained by an appendicostomy or a cæcostomy. If this has already been done I recommend treatment as for pulmonary tuberculosis, in bed, and in the open air. The diet should be the fullest that the patient can comfortably manage without increasing the diarrhoea. A milk diet does not, in my experience, suit these patients. Tonics are useful. Vaccines may be used as supplementary treatment, but I do not rely upon them. To sum up, as stated, the disease is one of low resistance, the most common infection being streptococcal.

General methods of treatment should be adopted, supplemented perhaps by appendicostomy. There must be a long convalescence; for three months after the patient is up and about there should be no return to work.

MR. J. P. LOCKHART-MUMMERY.

The last general discussion on ulcerative colitis took place here in 1909, and the figures produced at that meeting showed that not only was the disease by no means as rare as was supposed, but that it had a mortality of over 50 per cent. Undoubtedly great improvements have taken place during the last fifteen years, and thanks to the improved methods of diagnosis and treatment the mortality has been greatly reduced. My own most recent figures show a mortality of only 15·7 per cent.

We have to bear in mind that ulceration of the colon, like ulceration of the skin, may occur in a great many different forms and arise from a great many different conditions. The actual amount of ulceration may be slight and confined to a small area, or, on the other hand, it may be so extensive as to involve the entire colon, a surface very nearly equal to half the area of the body surface. The ulceration may be quite superficial, or again it may be so severe that only a few islands of mucous membrane are left. Also, we may see it at different stages.

A careful study of the disease, now extending over a considerable number of years, leads me to believe that ulcerative colitis starts in most cases in the solitary follicles and spreads from these. In the very early stages of ulcerative colitis one finds small punched-out ulcers which are evidently due to the breaking down of these follicles. Later on this characteristic disappears and the ulcers tend to run together and to become irregular in outline. In very acute cases the whole of the mucous membrane appears to be acutely inflamed with enormous ulcers in the more prominent parts.

A great deal of interest naturally attaches to the mode of infection, and while in some cases this is comparatively clear, in others it is decidedly obscure. Cases of ulcerative colitis occurring in lunatic asylums under the name of "asylum dysentery" are probably an infective disease from some specific organism and due in the main to bad hygiene. The ulcerative colitis that follows Bright's disease, lead and mercurial poisoning, &c., hardly comes within the scope of our inquiry, though it is none the less important. Considerable interest attaches to those cases in which chronic ulcerative colitis is grafted on to acute epidemic forms of ulceration, as for instance where it follows amœbic or bacillary dysentery, or sand dysentery. Many of these cases were seen during and after the War, and as regards both their behaviour and appearance they differed little from the ordinary cases of ulcerative colitis. No doubt what had happened was that the ulcers produced by the amœbæ became secondarily infective, and after the amœbæ had entirely disappeared an ordinary streptococcic infected ulceration was left. It seems very probable in very many cases chronic ulcerative colitis is due to secondary infection of a more acute condition. A good instance of this came within my notice about a year ago.

The patient was a man, about 80 years of age, who had had slight attacks of colitis for some time, but never severe. During one of these attacks he was given milk which was badly contaminated with streptococci. Several other people were made ill by this milk which was eventually traced to a cow with an ulcerated udder. As a result he became exceedingly ill with a very severe type of ulcerative colitis and nearly died. There could be little doubt in this case that the severe condition was due to the secondary infection.

We are still very much in the dark as to the exact bacteriology of this disease. Great difficulty arises from the very large number of organisms which are found in any specimen taken from the stools. There is no exact means of ascertaining which is the controlling organism or organisms. Just lately a good deal of work has been done at St. Mark's Hospital, and by Dr. Carnegie Dickson, on some of my cases, and an organism belonging to the paratyphoid group has been found to be present in quite a number of instances, sometimes associated with streptococci and staphylococci, and sometimes without. We are hoping that this may prove to be one of the controlling organisms, but this side of the subject I shall leave to be discussed by the bacteriologists, except to say that so far we have found that the best results are obtained by an appendicostomy combined with attempts to substitute another organism. The one most frequently used being the Bulgarian bacillus. We had hoped that it would be quite easy by putting pure cultures of the substituted organism into the colon direct to replace the infecting one. This, however, has not proved easy. The replacing organism is very apt to die out unless administered in very large quantities, and unless fed with suitable substances. We have, however, had some success already with this method of replacement and I believe it has great possibilities, but so far our knowledge of this subject is only in the very early stages. Careful bacteriological tests have to be made and checked, and we have yet to learn how to replace the streptococci and staphylococci which unfortunately are the most serious infective agents. In my experience vaccine therapy has not proved of much value, but I believe that good results will be obtained by the replacement of the infecting by some harmless organisms, when the technique of this procedure has been perfected.

The most serious cases of ulcerative colitis appear to be those due to a streptococcic infection, but as already stated, probably the streptococcus is not the primary organism. Staphylococci are also frequently found to be present in considerable numbers. A very interesting type is that in which the *Diplococcus pneumoniae* is the primary organism. I have met with two of these cases, in both *Diplococcus pneumoniae* was present in almost pure culture; both cases were acute and accompanied by profuse bleeding and high temperature. In neither case was there any infection of the lung or any other part of the body.

The disease is one of early adult life. The average age of the first series of sixty cases that I collected was 37; while in my last series the average age was 32. The sexes appear to be about equally affected.

The leading symptom is diarrhoea, which is severe and persistent. Bleeding is almost invariably present and may be profuse so that the patients become distinctly anæmic. Wasting is usually rapid and severe. With the exception of the pneumonic cases the temperature is not generally high, the ordinary temperature ranging from 99° to 101° F. In cases that end fatally the patients usually die from exhaustion, perforation or hæmorrhage; exhaustion and wasting being the commonest cause of death.

Diagnosis of ulcerative colitis should in these days present no difficulty, all that is necessary being an examination with the sigmoidoscope. It should be insisted upon, however, that no diagnosis that is not based on an examination of the bowel with the sigmoidoscope can be relied upon.

The actual type of ulceration seen varies enormously. No diagnosis can be considered complete without a careful examination of the stools by a thoroughly expert bacteriologist, as it is very important to know what type of infection we have to deal with.

Natural Healing of Ulcers.

By watching cases of ulcerative colitis with the sigmoidoscope from time to time one is able to see the ulcers during the healing stage, and it is a very striking fact that although most extensive ulceration may have been present healing occurs often without a trace of scarring being left, except in a few cases. Where the ulceration has been severe slight scarring may sometimes be seen. Only recently I saw a case in which the mucous membrane still showed small shallow healed pockets, the result of healing ulcers. I have, however, seen quite large ulcers, which appear to go down to the muscular coat, disappear without leaving behind any obvious scarring.

It is certainly a fact that stricture of the colon secondary to ulcerative colitis, although not unknown, is a very rare condition. Stricture after ulceration is more likely to occur where cæcostomy or colostomy has been performed, and one of the great objections to these operations in cases of severe ulcerative colitis is that severe stricture may result below the colostomy and prevent the possibility of re-establishing the bowel if the ulceration is healed up. This objection, however, does not apply to appendicostomy.

Prognosis.

The prognosis in cases of ulcerative colitis is distinctly bad, apart from operation. The mortality in operated cases was shown, in the last discussion which took place on this subject, to be over 50 per cent.: now it is only a little over 15 per cent., a reduction of 35 per cent.

In non-operative treatment recovery, even when it occurs, is very slow, and I have seen several cases treated without operation in which the patients have ultimately had to have an appendicostomy performed upon them, and it would have been much better if they had undergone it at the beginning.

In these days the prognosis is quite good provided an appendicostomy is performed early, but a great deal depends upon not leaving the operation until too late; once a really extensive ulceration has occurred and the patient has got badly run down in health the prognosis is not nearly so good. Nearly all the fatal cases in my series were those in which the patient did not come under the surgeon until he was almost *in extremis*.

There are two forms of operation for this condition:—

(1) *Giving rest to the Colon by the Establishment of an Artificial Anus.*—This method, while yielding good results, is open to serious objections. The disease practically always involves the entire colon and therefore a cæcostomy will be necessary. A cæcostomy is a very unpleasant form of artificial opening, and in addition it considerably interferes with the patient's digestion. The other serious objection is that it may prove impossible to close the opening owing to the contraction of the bowel below. The only cases of stricture of the colon following ulcerative colitis which have come under my observation have been those in which a cæcostomy had been done, which would make it appear that in a non-functioning colon secondary contraction is liable to occur.

(2) *Making an Opening through which the Colon can be irrigated*—namely, *Appendicostomy*.—One of the great advantages of appendicostomy is that the operation can quite easily be performed under local anæsthesia and consequently is not contra-indicated by the illness of the patient. It is, however, very advisable that the appendicostomy, if possible, should not be opened until the small wound in the abdominal wall is completely healed, or infection with organisms from the inside of the colon is certain to occur and may give rise to

considerable trouble. It might be thought that if good results arise from appendicostomy, irrigation from below will be equally effectual, but this is not the case. The attempt to introduce large enemata into an irritated colon produces violent tenesmus and has to be abandoned. There is no such objection to fluids run in at the other end of the colon.

There are one or two points of considerable importance in treating cases by appendicostomy. A catheter should not be tied into the appendix if it can possibly be helped, as it is very liable to give rise to sloughing of the appendix. A catheter should only be passed when the irrigation is being performed. A properly performed appendicostomy will remain patent indefinitely and no leakage occurs from the opening. It causes quite the minimum of inconvenience and the bowel can be washed through very easily by the patient himself.

I think it is very important that the solutions used for irrigating the colon should be as nearly as possible at blood temperature, and an accurate thermometer should be used for making certain of this. The best fluids to use are hypertonic solutions, so that the tendency is for the fluid to run into the blood, rather than for fluid to be drawn from the blood into the colon. No poisonous solutions should be used, as poisoning is very easily produced if antiseptics are used in the colon.

Of all the different solutions I have found the best results follow the use of a hypertonic solution of sea salt. Why sea salt should act better than ordinary salt I do not know, but it certainly appears to do so. Weak silver solutions such as protargol or argyrol are useful in some cases. Silver nitrate, however, should not be used as it is liable to give rise to argyria. When hæmorrhage is a marked feature a solution of kaolin is often very effectual in checking the hæmorrhage. As already mentioned we have been using sour milk containing Bulgarian bacillus for the purpose of washing out the bowel, and I believe there is a distinct future for this method of treatment when we have learnt rather more about it. Solutions containing oil or petroleum are also distinctly useful in protecting the ulcers during the healing stage.

Recurrences.—The bowel should, as a rule, be washed out twice a day, or even more often in bad cases, the important point being to keep the ulcers as clean as possible and prevent the discharges from accumulating in the colon.

To sum up, I believe that the mortality of ulcerative colitis has been very greatly reduced by appendicostomy, but this operation should be performed as early as possible, as soon as a definite diagnosis has been made by means of the sigmoidoscope. Operation can be perfectly well performed under local anæsthesia without endangering the patient's life. Everything depends upon early treatment and the fatal cases are nearly always those in which operation has been postponed.

Dr. W. E. CARNEGIE DICKSON.

From the point of view of the pathologist and bacteriologist, it is no easy matter to focus in one's mind a distinct entity or definite disease that can be specifically termed "ulcerative colitis"; just as one cannot speak of "ulcerative dermatitis" or "ulcerative stomatitis" otherwise than as manifestations of a whole group of different causes. I take it for granted that it is neither the intention nor the wish of this meeting to enter into a discussion of all possible causes of ulceration of the large bowel.

We may put on one side therefore such conditions as malignant disease, tuberculosis, syphilis, actinomycosis, and other mycotic infections: intestinal

schistosomiasis, and, perhaps, amœbic dysentery—although one is tempted to include this in the discussion. Such specific fevers as cholera and typhoid need not detain us; though we should remember that, in the latter, ulceration need not be limited to the ileum, but may attack the solitary lymphoid follicles of the large bowel, especially the cæcum.

An acute catarrhal enteritis may, of course, occur as part of, or as a complication of, many acute infective conditions of known, or unknown, causation, for example, some of the acute infectious fevers, such as scarlet fever, diphtheria, or measles; and may go on to follicular ulceration, particularly in the cæcum, as well as in the small intestine, especially the lower part of the ileum; and such ulceration may spread and lead to the formation of more extensive ulcers. Similarly, follicular and more extensive forms of ulceration may supervene in various chronic catarrhal conditions of the intestine, and may be found especially in cases of chronic nephritis, waxy disease, &c.

We may, however, usefully concentrate our attention upon those cases of ulcerative colitis in which that lesion is the outstanding clinical feature of the case, and in which the ulcers tend to be of considerable size. Bacillary dysentery, the paratyphoid and allied infections, and cases in which we may suspect the causal importance of such organisms as *Bacillus pyocyaneus*, pneumococci, streptococci, and enterococci, &c., either singly, or in combination, may, I think, usefully occupy our attention.

In any one of these infections, one may find all degrees of implication of the intestinal mucous membrane, from a mere slight surface catarrh, up to severe and extensive ulceration; and it appears to be a point of extreme importance that we should remember that, even if such ulceration be due originally and primarily to one specific type of organism, it speedily becomes a "mixed infection," in which a whole series of bacteria all take part in attacking the bowel-wall. So much is this the case that, except in a few instances, one cannot definitely be certain of the presence of a specific organism, and we have to treat the condition as one of mixed infection. Even when one obtains one of the paratyphoid, Gärtner, or dysentery bacilli, from the fæces, such secondary mixed infection has occurred if ulceration has taken place; and one has to bear this fact in mind if we determine to treat the condition with vaccines or sera. Thus, in some forms of bacillary dysentery, in which a vaccine made from the dysentery bacilli may not be suitable because of its great toxicity, the specific serum may with advantage be used along with a mixed vaccine made from the organisms of secondary infection.

Personally I have not had much experience with the treatment of typhoid fever itself with vaccines; but, in a considerable number of cases of paratyphoid infection I have obtained better results, if the bowel has been ulcerated, by using a vaccine containing both the autogenous paratyphoid bacilli and a suitable selection of the other organisms in the fæces likely to take an important part in secondary infection, including the patient's own *Bacillus coli*, streptococci, &c.

When vaccines first came into use the practice was laboriously to isolate the various organisms, e.g., from the nose, or throat, or sputum, &c., and to prepare the vaccine from some single organism, say a pneumococcus or streptococcus, or the like, which one took to be the primary, or the chief, infective agent. But now, I have practically discarded this method, and, by making use of a widely varying series of culture-media, we can, by selecting from them, obtain a bacterial emulsion approximating, as closely as possible, to the

organisms found in direct films of the original exudate ; and I find that such a mixed vaccine, is incomparably more efficacious than the single-organism vaccine.

In cases of colitis it is sometimes a matter of very great difficulty to determine which of the organisms present are of greatest importance ; and I would plead for the use of an extended series of culture-media in such cases. A bacteriological report founded upon a single agar plate from the fæces is of little use, and may, indeed, be quite absurd, for example, where practically nothing but *Bacillus coli* grows, or *Bacillus coli* with a smaller or greater number of colonies of "enterococci," staphylococci, and the like.

My own practice is to put on, as a routine, a series of agar, blood-agar, broth, blood-broth, anaërobic meat-broth, litmus-milk, MacConkey lactose agar, and, sometimes, other media ; and the extraordinary differences in the variety and number of the various organisms grown from the fæces in these different media are very striking, and from them one may "average up" the result of the whole investigation.

Perhaps the greatest handicap from which the bacteriologist suffers is that he has to report some more or less definite finding within a more or less reasonable time ; whereas, for the full scientific investigation of the case, he would require three weeks, or a month, or more, to isolate his organisms in pure culture, and give them long enough in the differential culture-media to be certain of their biological reactions. This, of course, is impossible in the great majority of cases, and he has to be content with such approximate results as he can rapidly obtain in one, two, or three days, or so. Fortunately, some of the more important pathogenic organisms can be comparatively rapidly isolated, and identified by specific serum-tests ; but there are many organisms in the fæces probably of great importance, which are either slow-growing, or their growth is limited or inhibited by the presence of other organisms. Every bacteriologist is familiar with the fact that his cultures from the fæces, if left for a week, or several weeks, or, for that matter, several months, show an extraordinarily different picture from that obtained in the first day or two. A dozen or more varieties of organisms may have come to light in what seemed at first to be, perhaps, a pure culture of *Bacillus coli*, or the like.

One organism of, I believe, considerable importance in connexion with colitis, ulcerative and otherwise, is *Bacillus pyocyaneus* ; and the recognition of its presence may be difficult in early cultures because it requires a copious supply of oxygen for the production of its characteristic pigment ; and the production of this pigment may be prevented by the presence of other organisms which use up oxygen, or in some other way. It may, in fact, sometimes be detected only after repeated plating-out. In a series of colitis cases, including those with severe ulceration and hæmorrhage, I have, in seven cases, found *Bacillus pyocyaneus* present, and have sometimes also obtained it from the blood and urine in these cases, which may clinically resemble typhoid or paratyphoid fever.

It would take too long to enumerate the many non-lactose-fermenting Gram-negative bacilli, classified and unclassified, which one finds in these cases of ulcerative colitis ; and, if one were to include the lactose-fermenters as well, the task would be endless. Castellani, for example, gives a list of about 100 of these ; and Mr. Lockhart-Mummery has referred to the clinical aspects of a case of very severe hæmorrhagic ulcerative colitis, in which I found a paratyphoid-like organism corresponding with *Bacillus carolinus* of Castellani in that list, this case being treated with an autogenous vaccine and

the administration of *Bacillus bulgaricus*, in addition to surgical and other measures.

It would take up too much time to deal with the question of vaccine treatment of individual cases and individual infections; and I must content myself with the above remarks as to the greater efficacy of mixed vaccines over single-organism vaccines, this being specially so in the case of the colon; although it may not apply so much to ulceration higher in the bowel, for example, in the duodenum; for, in duodenal ulcers, one sometimes obtains excellent results with a vaccine made from streptococci alone. In all such cases, local and general medical and surgical treatment, appropriate to the case and to the nature of the lesion, should always be employed along with the vaccine administration, which should be regarded as assisting, but not replacing, such ordinary measures. The neglect of this common-sense rule often brings vaccine administration into undeserved bad repute.

I have found agglutination and other immunity-tests with the patient's serum of comparatively little immediate practical value, and often, indeed, probably fallacious, in determining the specificity, or otherwise, of organisms isolated from the fæces. Every bacteriologist knows that even typhoid and paratyphoid bacilli, when newly isolated from the body, may not be specifically agglutinated by the patient's own serum, and may only develop the capacity of being so agglutinated after repeated culture and subculture. High-potency experimental sera obtained by the inoculation of animals, are, however, a valuable, though laborious, means of identification of certain organisms, e.g., the strepto- and pneumo-cocci; but they are, of course, part of the routine method for members of the typhoid-paratyphoid-dysentery group.

I have confined my remarks, in helping to open this discussion, more or less to general principles, and later speakers will doubtless deal with many of these points in greater detail; but I may, perhaps, be permitted, for one moment, to refer to the question of the origins of such infections, and to some of the pathological and bacteriological examinations which may help in diagnosis.

We have already noted that a colitis may form merely part of a general acute, or other form of infective disease. In many cases, the infective agent may be ingested with food or drink; but, in the absence of any evidence pointing to such sources, the physician or surgeon should always consider the possibility of other sources of infection in the body itself: more particularly the presence of septic teeth, tonsils, nasal sinuses, &c., and whether the appendix is involved. Especially in obstinate and long continued, or in cases of constantly recurring, colitis, the appendix often acts as a residual focus of infection; and, in such cases, a cure is often not effected until it is removed.

Lastly, one may obtain important collateral evidence as to the nature of the infective organisms from cultural examination of the blood, and of very carefully taken catheter-specimens of urine, as the kidneys are specially concerned with the elimination of organisms which have entered the circulating blood. One is, of course, familiar with the presence of *Bacillus coli*, typhoid and paratyphoid bacilli, &c., in the urine; but one may also find various other organisms, such as streptococci, *Bacillus pyocyaneus*, and various unclassified bacteria, probably of fæcal origin, which may have entered the circulation from an inflamed and ulcerated bowel. One need scarcely add that a general blood-examination may often be helpful. In my own series of blood counts in such cases, I have usually found a leucopenia, the diminution affecting especially the polymorphs, and thus producing a *relative* lymphocytic increase:

and a secondary anæmia, especially a hæmoglobinæmia. I agree with Sir Thomas Horder as to the occurrence of leucopenia in many chronic streptococcal, just as much as in coli-typhoid-paratyphoid, infections, and I have also observed that injections of normal horse-serum in such cases of leucopenia often bring about a rise to normal of the leucocytes, or even a distinct leucocytosis. One interesting point which I have often noted in cases of enteritis, with or without ulceration, is the diminution or absence of eosinophils from the peripheral circulation, these cells often accumulating in enormous numbers in and around the intestinal lesions. It is scarcely necessary, of course, to remind you that these cells are often, though by no means constantly, increased in the peripheral circulation in intestinal infections with worms.

Professor LEONARD S. DUDGEON.

My remarks concern entirely the sporadic form of a disease known chiefly in this country as ulcerative colitis.

Sir William Hale-White has argued by every means at his disposal that this disease is distinct from true dysentery, whilst others have urged that ulcerative colitis is simply bacillary or amœbic dysentery as met with among a population who have not visited countries where dysentery occurs in endemic or epidemic form. Personally I do not regard such evidence as of the slightest importance at the present day. The late Dr. F. M. Sandwith, who was a recognized authority on dysentery, severely criticized in his Lettsomian lectures in 1914, the view that ulcerative colitis is distinct from dysentery. It may be advantageous to quote two paragraphs from these lectures: "No one acquainted with both these diseases can have failed to appreciate the resemblances as regards symptoms and post-mortem appearances which one bears to the other"; and again, "We are confronted by an illogical outcome, for a case diagnosed as ulcerative colitis in a London hospital ward might be relabelled if it were discovered later that the patient had lived in the tropics and that his fæces contained amœbæ or bacilli." No advantage will be gained by further discussion of the arguments brought forward by various writers on this subject. My own interest in this disease was first made possible about 1908 owing to my association with Dr. H. P. Hawkins at St. Thomas's Hospital. Hawkins had come to the conclusion from his study of the disease that more elaborate bacteriological methods would probably lead to the solution of the problem. He made use of the rectal speculum so that scrapings could be made from any visible ulcers, which would afford greater opportunities to the investigator than mere examination of the fæces. By this method I was able to isolate a strain of the Flexner bacillus from a case of ulcerative colitis when repeated examination of the fæces had been unsuccessful. Dr. Hawkins believed that by this method of direct examination of the intestinal ulcers the causative organism of ulcerative colitis would be identified. This important line of investigation, which was commenced by him about 1907, has been greatly extended by Manson-Bahr and Gregg, who have employed the use of the sigmoidoscope for the examination of intestinal ulcers. It is fully recognized by those of us who have had personal experience of bacillary dysentery in all its forms that the isolation of true dysentery bacilli is infinitely more difficult in the presence of fæcal material, and still more so in pure diarrhœic stools without mucus, while the chances of successful findings is still further diminished if specimens containing fæcal

material are not examined at the earliest possible moment. In my experience typical dysenteric stools are exceptional in cases of ulcerative colitis, while abundance of mucus such as occurs in true dysentery is uncommon. The motions in ulcerative colitis are usually diarrhoeic in character, while blood may occur in large or small amount. These facts have helped to influence previous observers to regard ulcerative colitis as distinct from true dysentery, in spite of the fact that any variety of stool is met with in Flexner and to a much less extent in Shiga infections. Since the war I have seen six sporadic cases of so-called ulcerative colitis in this country and have isolated a Flexner bacillus from one of these, while from four of the others almost pure cultures of hæmolytic colon bacilli were obtained, and from the sixth case abundance of hæmolytic streptococci and the *Staphylococcus aureus*, even on repeated examinations. The patient in Flexner case had not been abroad, but, as already stated, I do not regard this fact as of any practical importance. Serological examinations in each case were made, but with negative results with my Shiga and Flexner antigens. I have not met with any case diagnosed as ulcerative colitis from which the free or encysted form of *Entamoeba histolytica* was obtained, or in which the intestinal ulceration resembled the amœbic form of dysentery, or in which a solitary abscess of the liver existed. In every case of ulcerative colitis, in my experience, the intestinal lesions have resembled those of bacillary dysentery. I must admit, however, that I saw cases in the East during the war in which there had been dysenteric symptoms during life; the intestines showed intestinal ulceration such as occurs in bacillary dysentery, but the *Bacillus paratyphosus* B or C were isolated from the fæces during life. The whole argument appears to me to rest on the bacteriological or protozoological findings. If, in a case diagnosed as ulcerative colitis, evidence of a true dysenteric infection, in spite of full investigation, cannot be obtained, the clinical diagnosis must remain. The term "ulcerative colitis" has the distinct advantage that it merely signifies the morbid process met with in the large intestine, associated with certain clinical phenomena.

In conclusion, I am opposed to the view that ulcerative colitis and true dysentery are of necessity caused by the same specific organisms, because I believe that such a condition as ulcerative colitis may be due to other bacteria. I have, however, referred to two cases of typical ulcerative colitis which ultimately proved to be Flexner infections. For this reason I consider it essential in every case diagnosed as ulcerative colitis to employ the following lines of investigation, as it is only by such means that the effective nature of the disease can be ultimately settled :—

- (1) Examination of the bowel by means of the sigmoidoscope.
- (2) If ulceration is present material should be obtained from the floor of the ulcer or ulcers and should be submitted to a detailed bacteriological and protozoological examination.
- (3) Repeated examination of the fæces.
- (4) Examination of the blood, more especially in relation to the presence of immune substances in the serum.

It is to be hoped that as a result of this discussion a combined effort will be made for the study of such cases as occur in the future.

Dr. A. F. HURST.

My remarks will deal with the treatment of ulcerative colitis and bacillary dysentery by anti-dysenteric serum.

The first description of the sporadic ulcerative colitis occurring in England was published in 1875 by Sir Samuel Wilks, who pointed out that it was anatomically indistinguishable from dysentery. Saundby, in 1906, and Hawkins, in 1909, came to the same conclusion as a result of clinical and pathological investigations, but without using the sigmoidoscope. In a report on an epidemic of "idiopathic ulcerative colitis," which resulted in 118 deaths in the Lancaster County Asylum in 1898, Gemmel expressed his belief that this condition, which had always been well known in asylums, was really dysentery. Vedder and Duval, working under Flexner in 1902, proved that epidemics of dysentery occurring in institutions in America were caused by the *Bacillus dysenterix*, and two years later Eyre showed that asylum dysentery in England was also caused by this organism. All attempts, however, to isolate the bacillus from sporadic cases have failed, though in a small number the blood has strongly agglutinated Flexner's bacillus. A diagnosis of ulcerative colitis can only be made with the sigmoidoscope. It is first necessary to exclude a growth beyond the reach of the finger. It is then necessary to decide what form of colitis is present, to ascertain the severity of the condition, and to exclude the possibility of amœbic dysentery, even if the examination of the stools has proved negative, for I have seen a few cases in which this infection developed in patients who had not been out of England. An anæsthetic is very rarely required, as the passage of the instrument does not cause pain unless the anal canal is inflamed; in such cases a cocaine sempule should be introduced a quarter of an hour before. If the sigmoidoscope is carefully introduced under visual guidance without inflation and only as far as it goes without difficulty, there is no danger, the few cases in which perforation has occurred having all apparently been due to its blind passage. In ulcerative colitis the mucous membrane is bright red and thick, the swelling being particularly obvious in the normally thin Houston's folds. It bleeds very readily when touched, and its surface is covered with blood-stained, purulent mucus. Superficial ulcers are invariably present, but in early cases they may be so small that they are difficult to recognize. More frequently they are of larger size and are sometimes so extensive that only small islets of mucous membrane are left, which may feel like small flat polypi on rectal examination, the floor of the ulcers being mistaken for the surface of the mucous membrane. The ulcers are always superficial, with irregular edges, and the thick mucous membrane is not undermined. The floor of the ulcers appears greyish-yellow when the blood and mucus are wiped from their surface. In some cases the sigmoidoscope shows that the rectum or the rectum and lower part of the pelvic colon are alone inflamed, as normal mucous membrane is found higher up, but it is doubtful whether the proximal part of the colon is ever involved without the pelvic colon and rectum.

I have often watched with the sigmoidoscope the process of healing. When healing occurs new mucous membrane forms; owing to the superficial nature of the ulcers very little scarring occurs and strictures never develop. When the sigmoidoscope is passed after complete recovery from ulcerative colitis or bacillary dysentery, nothing more is seen than some very slightly puckered areas in the mucous membrane, which may be a little paler and

smoother than normal. During the war I had several opportunities of examining the colon with the sigmoidoscope in cases of acute dysentery, and since then I have examined a considerable number of chronic cases in the same way. The differences between the appearance of the mucous membrane in amœbic and bacillary dysentery are so marked that a diagnosis can be made with the sigmoidoscope of one from the other with a considerable degree of certainty. I was much struck from the first, however, with the fact that the appearance of the mucous membrane in bacillary dysentery is indistinguishable in life, as Wilks had observed post-mortem, from that of the sporadic ulcerative colitis which occurs in England. On the other hand, in amœbic dysentery small, round, red elevations are seen on the otherwise normal-looking mucous membrane, corresponding to the collection of broken-down material in the submucous tissue caused by the invasion of the *Entamœba histolytica*. In the centre of each elevation there is a depressed yellowish ulcer, caused by the rupture of the submucous abscess through the mucous membrane.

In view of the very favourable effects I had obtained in chronic as well as acute bacillary dysentery by the intravenous injection of large doses of the polyvalent anti-dysenteric serum of the Lister Institute, I decided to try the effect of the same treatment in ordinary ulcerative colitis. The results have been remarkably favourable. They are not, I believe, due simply to the use of horse serum, but are definitely specific. In a severe case under my care I tried the effect of large doses of ordinary horse serum; no improvement resulted, but the administration of anti-dysenteric serum after a week's interval had the usual rapidly favourable result. I generally begin with an injection of 40 c.c. intravenously; this is followed on consecutive days by injections of 60, 80 and 100 c.c. The four injections are often sufficient, but it may be necessary to repeat the maximum dose two or three times. A considerable reaction often occurs; the patient's temperature rises and a profuse erythematous rash appears. The joints may become swollen and painful. But these symptoms generally only last a few hours, and never longer than a few days. I think they are less likely to occur if 15 gr. of calcium lactate are given three times a day the day before and during the days of treatment. No anaphylactic symptoms have been observed, even in patients who had previously had serum, but care was always taken to desensitize the latter by very small preliminary injections, and the same precautions would be necessary in any patient who was subject to asthma.

I have employed this treatment in about ten cases. Very rapid improvement has always taken place, though in one case in which appendicostomy had been performed some months earlier, it was incomplete. A secondary streptococcal infection had apparently become grafted upon the original dysenteric infection, as complete recovery only took place after the removal of the teeth, which was followed by a temporary severe local reaction. An attempt to treat this patient with a vaccine prepared from streptococci isolated from the stools had to be given up owing to the violent reaction which followed the injection of only a quarter of a million bacteria. My colleagues, Dr. John Fawcett and Dr. J. A. Ryle,¹ obtained an equally satisfactory result in one case under their care. The treatment failed in two others, but in both of these they recognized the ulcerative colitis as being of a hypertrophic character quite different from the ordinary type, so that in all probability it was caused by a different

¹ *Guy's Hosp. Gaz.*, 1923, xxxvii, p. 136.

infection. One of these cases died shortly afterwards from perforation, which rarely, if ever, occurs in the usual form of ulcerative colitis, and in the other an appendicostomy, which was subsequently performed, led to no further improvement. Dr. B. E. Jerwood¹ had also reported a case in which the patient was "incontinent, very weak, wasted, and quite determined to die," and in which rapid recovery followed treatment with antidysenteric serum. In most cases the improvement was as rapid as that of amebic dysentery with emetin and epidemic bacillary dysentery with intravenous anti-dysenteric serum. In my first case the patient was so ill when I first saw him that I advised an immediate appendicostomy. In spite of this the patient went steadily down hill, and a fortnight later, when the first injection was given, his condition appeared to be almost hopeless. But in three days the blood had disappeared from the stools, in five the sigmoidoscope showed that the mucous membrane was entirely free from ulcers, though still a little red, and in ten days the stools were normal and the mucous membrane was perfectly healthy, though the ulceration before the first injection was most extensive. I discussed the details of treatment by rest, diet, albargin injections, and charcoal, in the *Guy's Hospital Reports* for January, 1921, when I first described the results obtained with anti-dysenteric serum. They were of great importance before the introduction of the serum treatment, but recovery rarely took place in less than six months, and a year was often required. Until three years ago I was therefore inclined to advise appendicostomy as an additional measure in all cases of any severity. But since I have been using large intravenous doses of serum I have not found it necessary to advise appendicostomy in a single case, and I have only occasionally had recourse to albargin injections. I realize, however, that there must be cases, such as the two under the care of Dr. Fawcett and Dr. Ryle, already mentioned, in which the pathology is different and in which serum will be useless. In these the other methods of medical treatment, and appendicostomy, will be required, but the possibility of amebic dysentery, even in patients who have never been abroad, and even if the pathologist has failed to discover amebæ or their cysts, should always be remembered, as such cases recover very rapidly with emetin injections.

No case can be regarded as cured until the sigmoidoscope shows that the mucous membrane is perfectly healthy. It is then necessary to keep the stools soft by the use of paraffin or saline aperients for several weeks until the newly formed mucous membrane has overcome its original vulnerability to mechanical irritation.

I conclude with a plea for the more extensive use of the sigmoidoscope. It should no longer be regarded as an instrument for the proctologist or surgeon alone; it should be used by every physician, and to treat a case as colitis without first visually examining the rectum and pelvic colon should be regarded as equally as absurd as the treatment of a case of tonsillitis without first looking at the tonsils.

¹ *Guy's Hosp. Rep.*, 1921, xxxv, p. 292.

Sir CHARLES GORDON-WATSON.

I regret that I was not present to hear the opening of the discussion by Sir Humphry Rolleston and Sir Thomas Horder. I was much interested in hearing Dr. Hurst's account of his success with anti-dysenteric serum. In view of these results we certainly ought to give anti-dysenteric serum a more extended trial in the acute cases of ulcerative colitis which clinically resemble acute dysentery. In the chronic cases there is no doubt that results have enormously improved in recent years with treatment by appendicostomy and daily irrigation. I believe that the most generally useful fluid for irrigation is a weak solution of flavine, which is non-irritating and non-toxic and is very effective in neutralizing offensive stools. If appendicostomy is carried out early recovery is usually rapid ; advanced cases may end fatally in spite of appendicostomy. The type of case most usually met with occurs in middle-aged women, and starts as a chronic infection, but tends to become acute, if neglected, differing in this respect from epidemic dysentery. It is important that these cases should be kept under observation and under treatment after the primary attack has subsided. The appendicostomy opening should not be allowed to close, as recurrences are the rule rather than the exception, and attacks may recur after several years of immunity. The cases which give the most anxiety are those which are subject to severe hæmorrhages. I have had a case under my care with repeated attacks extending over the past twelve years ; on two occasions this patient has been almost *in extremis* from severe hæmorrhage from the colon. Hot hazeline irrigation is valuable in hæmorrhagic cases and blood transfusion, when possible, is certainly indicated. There can be no doubt that, in some instances, the affection of the colon is secondary and cannot be cured until the primary focus has been dealt with. Two illustrative cases have come under my notice : in one early case an inflamed appendix was removed instead of being utilized for appendicostomy, and the patient made a good recovery from the colitis without the cæcum being opened for irrigation purposes. In the other, treatment for an infected antrum resulted in, or at any rate played an important part in the cure. I have not been much impressed with vaccine treatment, but recently in a case under my care all efforts to check the disease and to control the hæmorrhage were unsuccessful until a vaccine was employed. Within a week after the use of a vaccine the temperature, which had been irregular for many weeks, became stationary, and the patient made a rapid recovery. There was a recurrence, but this was nipped in the bud by the use of vaccine.

Mr. DOUGLAS DREW.

I am very disappointed with the operation of appendicostomy as a means of treating of severe cases of ulcerative colitis ; I have practised it in a number of cases and have used irrigation and a large variety of drugs with almost uniformly bad results. One patient, after many weeks of treatment, left the hospital in what appeared to be a hopeless state but eventually she recovered at home and returned some four years later in good health, having in the interval had two children. The recovery could not be attributed to the appendicostomy as it had closed long before improvement began to take place. In another case the patient improved for some months after cæcostomy and then suffered a serious relapse and became so ill that it was decided, if possible,

to perform colectomy; on the abdomen being opened the colon and sigmoid were found to be so much thickened and inflamed that an anastomosis was impossible; the cæcostomy was therefore detached and converted into a right-sided colostomy in the middle line. More than a year has elapsed since this was done and the patient has greatly improved in health but still from time to time has a slight temperature and discharge of pus from the bowel.

In these severe cases it is most important to put the bowel at rest by diverting the fæcal stream by means of a right-sided colostomy and I attribute the failure of appendicostomy to the passage of the fæces over the ulcerated surface.

Section of Surgery.

President—Mr. JAMES BERRY, F.R.C.S.

The Treatment of Fractures of the Patella.

By R. H. ANGLIN WHITELOCKE, M.D., F.R.C.S.

FRACTURES of the patella are not uncommon and unless carefully treated are liable to leave a permanent insecurity in the limb, a liability to refracture, and to fracture of the other patella (fig. 1).

Various methods of subcutaneous treatment have from time to time been introduced, practised, and then abandoned as insufficient. Without an open operation true osseous union is almost unattainable and probably there are few



FIG. 1.—Case of refracture some years (ten) after fibrous union without open operation.

surgeons to-day who are not agreed on this point. *Fibrous* union, which is all that can be attained without an open operation, is relegated to those cases in which for any reason such as age, organic disease or unsuitable surroundings, the open operation is contra-indicated.

Fractures of the bone result from two very different forms of violence, viz., (1) violent strain upon the bone by the extensor muscles of the thigh as in a sudden attempt to avoid falling backwards, and (2) direct violence such as a blow, kick, or fall upon the knee, and sometimes from a combination of these factors; traction and impact. They may be simple or compound, of two or many fragments. The tip, base, or edges of the patella may be avulsed in

strain-fractures, and in direct fractures split into as many as eight or nine pieces. The line of fracture may extend in any direction, and the fragments be of very unequal size.

In every fracture of the patella there is bleeding into the synovial cavity of the joint and into the soft tissues around it—a point of much surgical importance.

Sir William Macewen,¹ as long ago as 1887, drew attention to the fact that, owing to the elasticity of the soft structures exceeding the cohesion of the bone in strain-fractures, the soft tissues do not yield at the same time as the bone. As a result of this in almost every indirect or strain-fracture a fibro-periosteal curtain curls inwards so as to envelop in part or completely one or both fragments. Lucas-Championnière² and Baerlocher,³ writing in 1903, both laid stress on the surgical value of this observation. In only four of my cases have I found it absent, and two of these were compound fractures.

The bony fragments are as a rule not widely separated except in long standing cases, where the separation has been known to exceed $4\frac{1}{2}$ in.; in one such case of refracture the separation amounted to nearly $4\frac{1}{2}$ in. Refracture usually occurs within a few months after the primary injury, and results from atrophy of the distal fragment, adhesion of a fragment to the femur, or from forcible flexion of the knee from a fall or too vigorous manipulations during treatment.

If we call to mind the various factors concerned in keeping the fragments apart and preventing bony union, such as muscular spasm, tearing of the peri- and pre-patellar tissues and capsular ligament, the effect of hæmorrhage and effusion into and around the joint, the curling in and envelopment of the fragments by the fibro-periosteal curtains already referred to, as well as the frequent tendency to rotation or tilting of the distal fragment from the pull of the patellar ligament, it is surprising that any other form of treatment should be considered. And especially does this argument apply in cases of long standing or refracture. In these, resection of inter-fragmentary soft tissues and refreshing of bony fragments even with saw or chisel, are usually an absolute necessity.

If we agree that only by open operation can bony union be secured, it necessitates our making use of some method which, whilst overcoming most of the difficulties likely to be met with, will also be of the greatest service, and be universally adaptable.

Three methods in recent times have been practised and often with excellent results, namely, osseous union, *cerclage* or circumferential looping, and a meticulous suturing of all the soft tissues that have been torn. Not one of these, however, seems sufficient by itself. Even the advocates of osseous suture are obliged to repair capsular tears in addition.

For over twenty years I have abandoned osseous suture entirely and substituted for it a combination of antero-posterior looping, with or without *cerclage*, with an accurate sewing of all tissues that are torn; every suture being of an absorbable material.

With a perfectly aseptic surgical technique there should be little or no fear of suppuration, septicæmia, or ankylosis.

Stimson,⁴ in 1907, had operated for fractured patella over 200 times with

¹ *Ann. Surg.*, 1887, Philad., v, p. 77.

² Lucas-Championnière, *Archiv. Internat. de Chirurg.*, 1903, i, p. 27.

³ Baerlocher, H., *Cor.-Bl. f. Schweiz. Aerzte*, 1903, xxxiii, p. 105.

⁴ Stimson, L. A., "Practical Treatise on Fractures and Dislocations," Philad., 1907

only a single slight mishap. In nearly eighty consecutive operations for cases of indirect fracture I have had but one case of suppuration. This happened in the last of my series, a case still in hospital. The patient, a man aged 28, suffered during the war and since from shell-shock, and periodically from psoriasis. This case I now realize was one quite unfitted for open operation and it is recorded here to accentuate the importance of careful and full investigation before operation. There was much bleeding into the joint before and after operation and on the seventh day a catgut stitch came away with some suppuration. The knee is likely to be stiffened though the fragments are in fair position. Benjamin Tenny¹ notes that in 1897 only 16 per cent. of cases were sutured in some fashion, whilst in 1906 the percentage was 60. Apparently wire was used in all cases operated on previous to 1899, and in 1906 there were more than three cases sutured to one wired.

Professor Heineck, of Chicago, wrote many years ago: "In recent fractures of the patella I have abandoned osseous suturing. I have not seen a case of old fracture of the bone in which I felt that a good result could not be obtained without employment of osseous suturing." This has also been my experience.



FIG. 2.—Very small distal fragment; union after antero-posterior looping with catgut: almost ideal; wiring impracticable.

For over twenty years I have practised and taught that osseous suturing was quite unnecessary for long-standing as well as recent fractures, whether indirect or direct.² I believed then, as I do to-day, that the disadvantages of osseous suturing are serious and considerable.

The following are the objections to osseous suturing:—

- (1) It is unnecessary. It adds injury to injury and equally good if not better results may be obtained by less difficult and less laborious methods.
- (2) It requires special instruments and considerable skill in handling them, whilst they are liable to break even in skilful hands and remain embedded in the bone or become lost in the joint.
- (3) It is a method neither suitable nor applicable in a considerable number of cases. It cannot be usefully employed when there is great inequality in the size of the fragments. One or other fragment may be so small that there cannot be sufficient hold for the wire suture or metal plate (fig. 2).

¹ Tenny, Benj., *Ann. Surg.*, 1908, xlviii, p. 719.

² Whitelocke, *Brit. Med. Journ.*, 1910, ii, p. 292.

Unusually brittle bones are liable to splinter; drilling or boring them causes additional injury.

In cases of secondary operation the fragments, especially the distal fragments, may be and often are so atrophic as to be incapable of holding the sutures, which cut out as soon as strain is put upon them. When wire sutures break and screws or plates become loose they may fall within the joint and require removal.

Open circumferential looping or *cerclage* was introduced by Berger,¹ of Paris, as a supplementary measure to osseous suture in cases in which the fragments were too small for successful suture, in cases of much comminution, and when the bones were unduly friable. It is the only method of suture used by some, but it cannot be relied upon as entirely sufficient, though safe and serviceable in many cases.

Simple suture of the capsule and lateral patellar ligaments and all the soft tissues that are torn has met with marked success, and in some cases of *recent* fracture may be all that is required. In this particular class of case the fragments fall readily together, and when the limb is kept in extension there is but little strain upon them. On the other hand in long-standing cases, and where the strain is great it is not always sufficient, and must be supplemented by antero-posterior looping. The ultimate strength of the tendon or bone depends upon its capacity to reproduce its own tissues, and any suture material remaining at the end of four or six weeks is but a useless foreign body, troublesome if not dangerous. Accurate stitching of the soft tissues would seem to be of even greater service than *complete* restoration of the patella itself, and it is always safer to remove any small fragment of bone or articular cartilage which cannot be kept accurately in place by stitching. Such a loose portion may do harm later by furnishing a roughened posterior surface to the patella or even become detached and fall into the joint, grow, and increase considerably in size as a loose wandering body.²

I have found the following operation suitable for all cases of patellar fracture, whether simple or compound, due to strain or impact, recent or of long-standing. It is entirely simple, and is adaptable, as far as I have experienced, to every possible condition likely to be met with. The requirements have all been met, bony union has followed and good functional results have been attained. I know of no case of refracture nor of stiff joint resulting from an operation for recent fracture except the one mentioned. In two cases of compound comminuted fracture there was for a time some widening of the patella, enough to produce a misfit, and some limitation of complete flexion which was overcome eventually. In one case the movements were free and good in six months, in the other in ten months. In compound fractures the operation should be performed as soon as possible. Delay is injudicious owing to the risk of sepsis and all that this means. Every effort should be made to counteract the possible infection by cleansing not only the skin but the articulation and lacerated tissues. In recent subcutaneous fractures there is no immediate call, and there are many advantages to be gained by waiting a few days. Opportunity is afforded of studying the precise condition, and of procuring a proper and suitable operative field. The patient is kept in bed with extended knee and elastic compression over it to limit intra-articular bleeding.

The limb is shaved and the knee washed carefully each day with alcohol

¹ Berger, *Bull. et Mém. de la Soc. de Chir.*, Paris, 1892, xviii, p. 523.

² Whitelocke, *Brit. Journ. Surg.*, 1914, i, 650.

and enveloped in an aseptic dry dressing until the time of operation. I do not now use a tourniquet or elastic bandage for operations on the knee. I find these inconvenient, unnecessary, and likely to promote undue oozing. A general anæsthetic is the routine. The skin incision should be adapted to circumstances, but must always be of sufficient size to allow of full exposure of the whole operative field and proper cleansing of the joint, as well as adequate suturing of all tears. Formerly I used the classical longitudinal incision, now I use a curved one with the base of the flap on the inner side of the joint. The

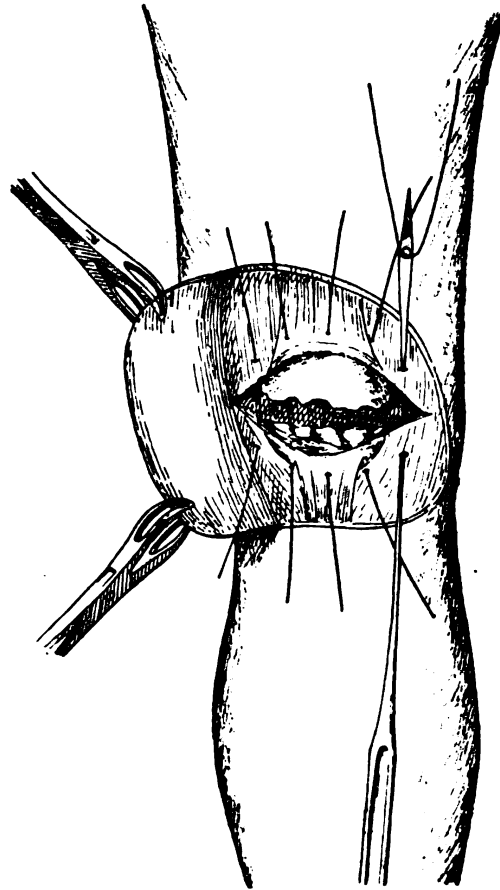


FIG. 3.—Showing how the catgut sutures are passed with needle, after beginning in middle line.

joint is not irrigated but swabbed gently with normal saline. At one time I used kangaroo tendon, now almost invariably non-chromicized No. 3 or 4 catgut, both for encircling the fragments and suturing the soft tissues. I regard it as positively unwise to leave any *non-absorbable* suture material permanently either in the articulation or in the peri-articular tissues.

Metallic sutures, whatever may be said to the contrary, frequently break across or become detached, and remain in the joint or tissues around. I have myself seen and removed pieces of wire, portions of drills, and even screws and

plates from joints after attempts at osseous suture of the patella had been made by skilled and competent operators. Metallic substances inserted into bones that are subject to movement and strain will very frequently become loosened in time, or cut out and set up subsequent irritation.

As sure as constant pressure will induce atrophy or thinning in any living tissue, so will a metallic or other foreign substance, inserted and left in the body, tend to become loose and set up irritation. The joint is always searched for detached portions of bone or articular cartilage. These are removed if too small or incapable of safe replacement.

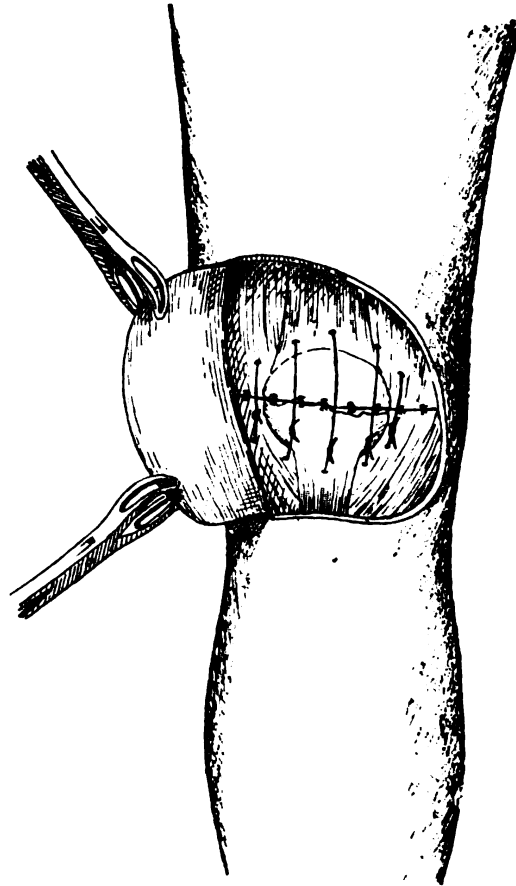


FIG. 4.—Sutures tied and periosteum stitched.

Every portion of fibro-periosteal tissue found between the fragments must be carefully removed, if necessary with a curette. At this stage, in recent cases, the fragments usually fall together readily, whilst in long-standing injuries it will be necessary to remove the inter-fragmentary tissues and refresh the bony fragments with saw or chisel. This having been done, the separated portions are brought together and united by passing a long-handled needle (fig. 3) threaded with No. 4 catgut through the centre of the patellar ligament behind the distal fragment through the joint and from behind for-

wards immediately above the proximal piece. Two or three similar threads are passed on each side of this in the same way. These strands of gut are tied tightly across the patella after the fragments have been placed in accurate apposition, and the periosteum stitched (fig. 4). This is usually a simple matter in recent cases, whilst in those of long-standing it will sometimes require much manipulation and even partial division and stretching of the extensor muscle, or detachment of the patellar ligament at or near its insertion. In comminuted fractures and where there is obliquity rather

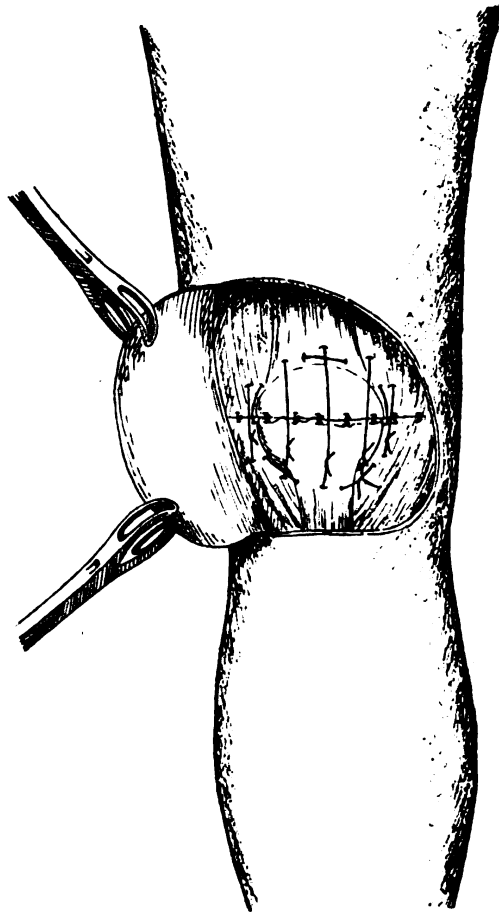


FIG. 5.—Antero-posterior sutures tied, supplemented by circumferential looping (*cerclage*).

than the usual transverse splitting of the bone, in addition to the antero-posterior peri-articular sutures, I pass one or two catgut sutures by circumferential looping around the bone (fig. 5). After this, all the soft tissues that are torn are stitched accurately, but not too closely, so as to leave sufficient space here and there between them to allow of drainage of fluids from the synovial cavity. The skin wound is closed with silkworm sutures and drainage is dispensed with. If drainage for any special reason be indicated, a strand of twisted silkworm gut is used for forty-eight hours, not longer. Over the knee

a thick absorbent dressing is applied and bandaged firmly to exert constant elastic pressure. In recent cases, and in those in which there is but little tension or strain, the firmly applied elastic dressing, if carried well above and below the joint, is all that is needed to keep it from being flexed unduly. In some cases, especially in long-standing ones, a light moulded back splint should be applied and retained until at least the wound is healed. In cases of refracture, and of long-standing, when the fragments are brought together with difficulty and there is strain, it is advisable to flex the thigh and

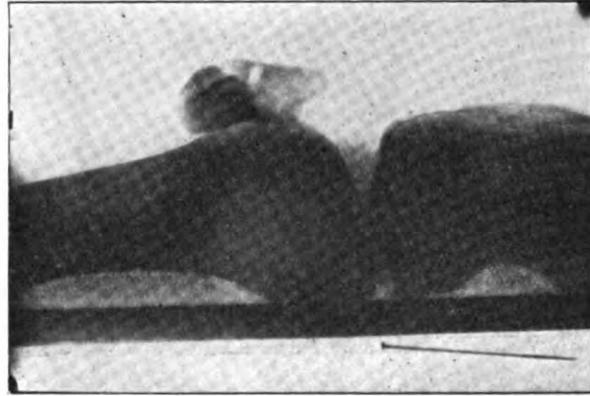


FIG. 6.—Comminution, antero-posterior looping and cerclage six weeks after. Note size of patella.



FIG. 7.—After antero-posterior looping (six and a half months). Note (usual) greater density of proximal fragment.

elevate the limb for a few days. I usually allow walking after twenty-eight days and encourage voluntary movements in the foot and ankle from the first day. When this voluntary routine is maintained regularly, passive movements and early massage are seldom, if ever, required. *Active voluntary movements of joints are never overdone by the patient, whilst early passive movements, unless carefully regulated, are seldom, if ever, without risk in every injured joint.* The after-care is of much importance, and well regulated movements should be continued under supervision until the limb is restored to strength and the movements of the knee complete. Eight weeks is usually ample time for ordinary

purposes. Unless the patient in the end is able to run upstairs or climb a ladder as well as before the accident, the result cannot be considered good. I would even go further and say that unless an athlete can play at his games or a workman balance himself on a roof the result is not ideal. I believe that the number of cases of long-standing disability are becoming rarer, since most surgeons now perform an *open* operation nearer the time of the accident.

I regret being unable to give full or comprehensive statistics of my cases, for I do not possess either the records or the pictures of any of the cases upon which I operated during the war. It is my belief, however, based upon a considerable experience, that the very simple methods just described will prove satisfactory and helpful to any who may wish to try them.

In conclusion, I may add that similar methods of suturing are used both for ruptures of the patellar ligament with upward displacement of the bone and for fractures of the olecranon.

Figs. 2, 6, 7 show results of union after antero-posterior looping and *cerclage*.

The Closure of the Suprapubic Urinary Fistula following Suprapubic Prostatectomy ; Observations on Sixty-eight Cases.

By H. P. WINSBURY WHITE, F.R.C.S.Eng.

THE sixty-eight cases are from a series of seventy-seven consecutive cases operated upon at St. Peter's Hospital, between April and November, 1921. Of the nine cases not included, none survived convalescence. I have to thank the staff of St. Peter's Hospital for permission to publish these notes.

The purpose of this paper is to consider :—

(1) All factors which have a possible influence on the time of closure of the suprapubic urinary fistula ;

(2) The utility of the indwelling catheter.

The subject is dealt with under the following headings :—

(1) Brief outline of post-operative treatment.

(2) Division of cases into groups in relation to the employment of an indwelling catheter.

(3) The use of the indwelling catheter.

(4) The operative procedures.

(5) The onset of micturition.

(6) The time of removal of the suprapubic drain.

(7) Secondary hæmorrhage.

(8) Malignant cases.

(9) Summary.

(1) BRIEF OUTLINE OF POST-OPERATIVE TREATMENT.

The parietal and bladder wounds were closed round a Freyer's tube [1], at the end of the operation ; and in some cases according to the tastes of the individual surgeon, a small rubber drain was inserted into the prevesical space.

[June 6, 1923.]

At the end of three or four days Freyer's tube¹ was replaced by a smaller one, the urine, meanwhile, escaping into suprapubic cellulose dressings [2], retained by a many-tailed bandage, and changed four-hourly. In a few cases in which the prostatic cavity had been packed at the operation as a precaution against hæmorrhage, the gauze was removed on the third day. The bladder and prostatic cavity were irrigated daily, both by the suprapubic route and by Janet's method [3]. On the tenth day a large steel sound was passed per urethram, the skin sutures were removed, and in the majority of cases the suprapubic drain was finally dispensed with. The patient was allowed to sit in a chair towards the end of the third week, unless prevented by some complication supervening, or in anticipation of an indwelling catheter, and was not discharged from hospital until the suprapubic fistula was closed.

(2) DIVISION OF THE CASES INTO GROUPS IN RELATION TO THE EMPLOYMENT OF AN INDWELLING CATHETER.

The following grouping as shown in Table I (p. 125) has been found convenient:—

Group I.—All cases in which fistulæ were finally closed by the twenty-eighth day without an indwelling catheter.

Group II.—All cases showing signs of delay in closure of fistulæ, subdivided as follows: (a) Treated with an indwelling catheter; (b) indwelling catheter contra-indicated for time being.

The cases under (b) terminated in one of two ways: (1) Spontaneous closure in due course; (2) closure following delayed use of indwelling catheter. The actual figures are given in Table I.

Of those cases in which the fistulæ did not close in response to the indwelling catheter, it is interesting to note that two-thirds had not commenced micturition up till the time at which the catheter was tied in. The importance of the catheter in such cases will be referred to later. It will be noted that the fistulæ of the cases in which the indwelling catheter was used closed sooner than in the others, thus demonstrating its value.

The following were the conditions which supervened and which prevented or delayed the use of an indwelling catheter: (1) Acute epididymitis; (2) pyelonephritis; (3) slough or phosphatic deposit on the wound surfaces.

By far the commonest of these was acute epididymitis accounting for about 54 per cent. The part played by acute epididymitis in determining delay depended on whether the complication supervened early or late in the convalescence. If early, there was not necessarily a contra-indication for the catheter about the fourth week.

In a previous paper, entitled "Epididymitis and Suprapubic Prostatectomy—A Study of Fifty Cases" [4], the writer showed that 64 per cent. acute cases occurred during the first week of convalescence. In the few cases in which an indwelling catheter was tried, in the presence of some degree of pyelonephritis the result was always to increase rather than to diminish the signs of infection. It was, therefore, found more satisfactory in such cases to dispense with the catheter entirely. It is questionable whether in such cases urethral instrumentation of any kind should not be avoided, and this practice of avoidance was observed.

With regard to slough or phosphatic deposit on wound surface, in a few

¹ Measurements of Freyer's tube: $\frac{3}{8}$ in. tubing, $\frac{3}{8}$ in. lumen, 4 in. in length.

cases a coating of phosphatic deposit on the wound surface occurred. It appeared early, and commenced to peel off as a fine slough about the end of the third week. An indwelling catheter could serve no useful purpose until the granulations were free from slough. Moreover, these cases were rather prone to acute epididymitis and pyelonephritis.

In other cases it was unusual for the portion of the parietal wound closed by suture not to heal by first intention. The open portion generally showed healthy granulations by about the seventh day. Two-stage prostatectomy cases, however, were an exception to healing by first intention, as the wound invariably broke down in the subcutaneous area.

(3) THE USE OF THE INDWELLING CATHETER.

The ideal sought and encouraged was undelayed closure without the aid of an indwelling catheter: but in many cases in which fistulæ persisted without any indication being given of their closing, closure was established at once by a judicious use of the catheter; and again, in those cases of delay in recommencing micturition, the indwelling catheter re-established the habit, which was an essential before closure would occur.

The reason for avoiding the use of the indwelling catheter, if possible, is that it is a foreign body in the urethra and granulating prostatic cavity, and in the presence of already existing sepsis tends to produce more.

The urethritis set up appears to be in proportion to the length of time the catheter is left in. No amount of care can prevent a purulent discharge from the urethra which has borne a catheter for six days. The discharge is serous till about the third day.

In prolonging unduly the use of an indwelling catheter in the hope of providing an efficient dependent drainage, it should be borne in mind that the value of the procedure is being minimized to some extent by the inevitable sepsis which will result from the presence of the catheter in the urethra. In this series of cases the practice was followed of not leaving the catheter in for longer than three consecutive days. In none of the cases did such usage give rise to any complication. On the other hand, it is not difficult to produce acute epididymitis by leaving a catheter in position for a week. In addition, its vesical end becomes coated with urinary salts [5]. In order to get the maximum benefit in the minimum of time, it is essential that the catheter be not resorted to, too soon [6]. When any doubt arose as to whether a case was ready for an indwelling catheter it was generally advisable to postpone its use for a day or two. Employing the catheter too soon means leaving it in position longer than intended. Thus, a urethritis is set up which could have been avoided. Any question of the fistula becoming epithelialized in the meantime was safeguarded by curetting it with a sharp spoon, while the drawing together of the edges of the wound with adhesive plaster was helpful in bringing about closure in many cases.

Before final closure of the fistula can be precipitated by the use of an indwelling catheter, it is necessary that the wound surface be free from slough or phosphatic deposit, and that micturition be re-established.

These factors being present, then the most favourable time is when the fistula remains dry for an hour or more at a time. In such cases closure from an indwelling catheter can be relied upon. On the other hand, it may be said that once a case has reached the "wet and dry" stage spontaneous and final closure is imminent, and the catheter is not required. This was certainly so in some cases, but in several in which an indwelling catheter was contra-indicated for other reasons the fistula, in spite of these indications, remained

open for a considerable time subsequently. The plan therefore followed was to resort to the indwelling catheter, if spontaneous closure had not occurred after several days of this stage, provided there was no contra-indication for so doing.

Where the spontaneous onset of micturition was delayed it was essential to resort to this procedure. In some, this was done when micturition had not commenced by the twenty-first day; but in no such case did final closure of the fistula result after removal of the catheter until it had been used a second time following a week's interval of rest for the urethra.

Fistulæ most obstinate in closing even after the use of an indwelling catheter were noted in the following cases: (1) After the second stage of prostatectomy, where preliminary cystostomy had been performed several months previously, and where a cicatrized fistula remained; (2) where re-establishment of micturition did not occur until after the use of the indwelling catheter.

The catheters employed were gum elastic coudé, ranging from Nos. 18 to 22 (French scale). The largest size was always used where possible, as it provided the best drainage. It was seldom that one so small as a No. 18 had to be used, and, when so, always required a good deal of attention, as the narrow lumen easily became blocked. When this occurred, the only satisfactory way to deal with it was to remove it and replace it by another. Obviously, all the good intended from the catheter may be lost if it is allowed to remain blocked for several hours, as the increasing intravesical pressure may cause the fistula to re-open. It was an essential for the success of this part of the treatment that frequent inspections and irrigations were made.

(4) THE OPERATIVE PROCEDURES.

These embraced three types of operation: (1) Freyer's operation; (2) Thomson-Walker's operation; (3) two-stage prostatectomy.

Eighty-one per cent. were dealt with by one or other of the first two methods, and on an average the fistulæ in these cases closed on the twenty-sixth day. In the remaining 19 per cent. of cases, prostatectomy was performed in two stages, the fistulæ closing on an average on the thirtieth day, but the slower cases were by no means all in the last group.

With the more tardy cases the question naturally arose whether the delay was due to any obstruction to the outflow from the urethra. In a number of these, on a metal sound *per urethram* being passed, an obstruction could be made out between the prostatic cavity and the bladder [7]. In all of these cases Freyer's operation had been performed. As it did not fall to my lot to pass the sound in all cases I cannot give the actual figures.

In two-thirds of the total number of cases operated upon Thomson-Walker's operation [8] was performed, which entirely eliminated the possibility of obstruction.

In the two-stage cases the intervals between the cystostomy and the removal of the prostate covered periods varying from two weeks to eight months. In two cases the patients underwent prostatectomy as long as eight months after cystostomy, and as would be expected in each case the fistulous track was considerably fibrosed at the time of operation. One took eight weeks and the other nine weeks to close.

(5) THE ONSET OF MICTURITION.

Except in several exceptional cases, micturition did not recommence until several days after the suprapubic drain had been removed. The practice followed was to remove the drain on the tenth day unless there was an indication for continuing the drainage a little longer. As stated by Sir John Thomson-Walker in his book [9]: "Occasionally there is a rise of temperature when the patient first passes urine through the urethra, but this subsides on the following day." This was noted in a number of cases. The actual figures with regard to the times of onset of micturition are shown in Table II (p. 125). In one case micturition was as late as the thirty-fourth day in commencing, and only after the use of an indwelling catheter. In two cases micturition was established on the day following removal of the suprapubic drain.

In backward cases, the use of the indwelling catheter undoubtedly helped to establish the habit. In one, however, it was not successful until after the use of the catheter for the second time. It was demonstrated that the slow return of micturition was one of the most important factors in connexion with delay in closure of the fistula. It must be obvious that the final closure of the fistula is impossible until micturition is re-established. Moreover, acute retention occurred in two cases in which the fistulæ suddenly closed before micturition had commenced. They were treated with the indwelling catheter. With regard to the causes of the late onset of micturition, it was found that in two cases some obstruction to the passage of a sound was manifested between the prostatic cavity and the bladder. In about 90 per cent. of those cases, however, with micturition delayed towards the end of the fourth week, there was one or other of the two following factors present in the history: (1) Symptoms of enlarged prostate for several years; (2) marked chronic retention of recent origin. The inference is that loss of tone of bladder muscle from chronic retention was the chief cause of delay.

(6) THE TIME OF REMOVAL OF THE SUPRAPUBIC DRAIN.

In seven of the earliest cases of the series the drainage-tube was removed on the seventh day. The motive in early removal was the hope of thereby shortening the convalescence. The average number of days before final closure of the fistulæ in these cases was twenty-eight; whereas the average for a similar number of cases in which tubes were removed on the tenth day, being treated at the same time, was twenty-one; thus the object of early removal appears to have been defeated. It is undoubtedly an advantage to continue the drainage until the granulations in the prostatic cavity are well formed. There was a very striking tendency to unsteadiness in the temperature in those cases in which early removal was practised. Freyer [10] points out that patients who pass urine early in the convalescence often do badly.

(7) SECONDARY HÆMORRHAGE.

This occurred in four cases, about 6 per cent. In each case, owing to the state of the urinary tract, the patient was a poor subject for operation. The bleeding occurred at varying periods between the eleventh and the twentieth days. In no case was the hæmorrhage so severe as to cause any real anxiety. Each was treated by displacing the clots from the bladder with irrigation by Janet's method, after dilating the fistula so that the larger

size tube could be replaced, and this was left in position until all signs of bleeding had ceased. It is important to recognize that to deal effectively with this complication the bladder must be emptied of clot; otherwise the contractions set up by its presence will cause the hæmorrhage to continue. Hæmostatic serum and morphia, hypodermically, were useful adjuncts.

(8) MALIGNANT CASES.

There were seven of these, 10 per cent. of the total. All were early and recognized clinically before operation, except one, and all offered some prospect of relief by prostatectomy. In two cases the enucleation was accomplished with the forefinger. In the other five cases, the removal was only completed by sharp dissection, after the patient had been placed in the Trendelenburg position. The average number of days in closing for six of these was twenty-one. The other case was discharged with a permanent suprapubic drain.

Although the fistula in six of these cases closed so readily, five of the patients reported back to hospital within a few months with the fistula re-opened.

(9) SUMMARY.

(1) Too early removal of the suprapubic drain, by diminishing the drainage too soon, tends to delay the convalescence. Rapid closure of the fistula is not always desirable.

(2) Closure of the suprapubic fistula without an indwelling catheter should be the aim in all cases. This was accomplished in about 52 per cent. of the cases.

(3) The indwelling catheter is necessary in a large proportion of cases, to avoid an unduly protracted convalescence. It was employed in about 48 per cent.

(4) The indwelling catheter does not help the fistula to close if used too soon; it increases the amount of sepsis present if left in too long.

(5) No complication arose from its use for three successive days in any case of this series.

(6) In a considerable majority (about 66 per cent.) the fistulæ were finally closed either with or without the aid of an indwelling catheter by the end of the fourth week.

(7) In the remaining cases the chief causes of delay in closure were: (a) Complications preventing the use of an indwelling catheter, such as acute epididymitis and pyelonephritis; (b) delayed onset of spontaneous micturition, most commonly in cases of previous chronic retention; (c) long-standing suprapubic fistulæ in two-stage prostatectomy cases; (d) a shelf of mucous membrane between the bladder and the prostatic cavity, in some cases after Freyer's operation for prostatectomy.

(8) The fistulæ in malignant cases may close very readily following suprapubic prostatectomy, but tend to re-open within a few months.

I would emphasize, in conclusion, that I do not bring this matter forward as a plea that any particular method of after-treatment that will hasten the closure of the fistula is the most desirable; nor do I advocate that the procedures followed in these cases should always be practised, knowing that there are alternatives which are equally efficient. But in realizing the importance of ascertaining the results of treatment in a considerable number of cases by any given method, I have stated the foregoing facts in the hope that their permanent record may prove of some value.

TABLE I.—DIVISION OF CASES INTO GROUPS IN RELATION TO THE EMPLOYMENT OF AN INDWELLING CATHETER.

Group I.

All cases whose fistulæ were finally closed by the twenty-eighth day of convalescence, without an indwelling catheter:—

26 cases	38 per cent. approx.
Average time for fistulæ to close	20 days

Group II.

All cases showing signs of delay in closure of fistulæ :

(a) Treated with an indwelling catheter :

28 cases	41 per cent. approx.
In 19 cases fistulæ closed by the twenty-eighth day.						
In 9 cases fistulæ closed after the twenty-eighth day.						

(b) Indwelling catheter contra-indicated for time being :

18 cases	20 per cent. approx.
In 8 cases fistulæ closed spontaneously in due course: Average time in closing ...						37 days
In 5 cases fistulæ closed after postponed use of indwelling catheter: Average time in closing ...						34 days

TABLE II.—THE TIME OF ONSET OF MICTURITION.

For whole series, the average day of convalescence was the nineteenth.

In 60 per cent. spontaneous onset by the twenty-first day.

In 25 per cent. spontaneous onset between the twenty-first and twenty-eighth day.

In 14 per cent. onset only after indwelling catheter.

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111

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PROCEEDINGS
OF THE
ROYAL SOCIETY OF MEDICINE

EDITED BY
SIR JOHN Y. W. MACALISTER
UNDER THE DIRECTION OF
THE EDITORIAL COMMITTEE

VOLUME THE SIXTEENTH

SESSION 1922-23

SECTION OF THERAPEUTICS & PHARMACOLOGY



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1923

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SECTION OF THERAPEUTICS AND PHARMACOLOGY.

CONTENTS.

October 10, 1922.

W. LANGDON BROWN, M.D. (President).	PAGE
The Problems of Asthma	1
T. IZOD BENNETT, M.D.	
The Modification of Gastric Function by means of Drugs (Abstract) ...	6

November 14, 1922.

E. BOOCK, B.Sc., and J. W. TREVAN, M.B.	
The Effect of Light on the Response of Frogs to Drugs	8

January 9, 1923.

DISCUSSION ON "THE PRESENT POSITION OF ORGANOTHERAPY."	
Professor SWALE VINCENT, D.Sc., M.D. (p. 9), Dr. GEORGE R. MURRAY (p. 14), Dr. W. R. GROVE (p. 18), Dr. H. VINES (p. 23), Professor W. E. DIXON, F.R.S. (p. 23), Dr. W. LANGDON BROWN (President) (p. 23)	

March 13, 1923.

DISCUSSION ON THE ACTION OF QUINIDINE IN CASES OF CARDIAC DISEASE.	
Professor FRANCIS R. FRASER, (p. 25), Dr. A. N. DRURY (p. 30), Dr. A. E. CLARK- KENNEDY (p. 32), Dr. T. F. COTTON (p. 38), Dr. F. PARKES WEBER (p. 40), Dr. B. T. PARSONS-SMITH (p. 41).	

April 10, 1923.

W. HAMILTON KIRK, M.R.C.V.S., Captain R.A.V.C.	
The Idiosyncrasies to Drug Tolerance of Animals as compared with Man	43

**SECTIONS OF OBSTETRICS AND GYNÆCOLOGY,
THERAPEUTICS AND PHARMACOLOGY.***(JOINT MEETING.)*

December 7, 1922.

H. H. DALE, C.B.E., M.D., F.R.S.	PAGE
The Value of Ergot in Obstetrical and Gynæcological Practice ; with Special Reference to its Present Position in the British Pharmacopœia	1
DISCUSSION: Sir NESTOR TIRARD, Dr. HERBERT SPENCER, Professor W. E. DIXON, F.R.S., Dr. T. W. EDEN, Mr. ALCK BOURNE, Professor H. BRIGGS, Dr. DALE (reply), pp. 5-7.	

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Section of Therapeutics and Pharmacology.

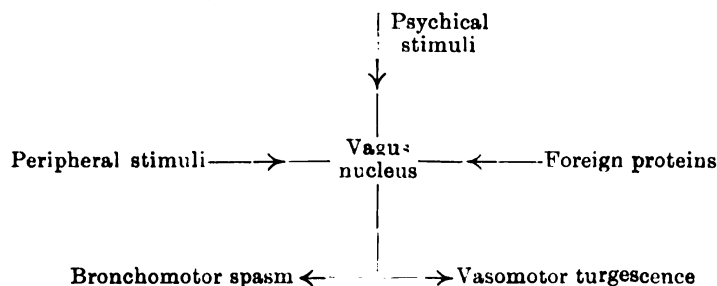
President—Dr. W. LANGDON BROWN.

The Problems of Asthma : PRESIDENT'S ADDRESS.

By W. LANGDON BROWN, M.D.

IN pursuance of my conception of the functions of this Section, I have chosen as the subject of my address one which has both clinical and experimental aspects. The problems of asthma were for years most baffling, but recent work has cleared the ground and defined the issues. These problems have now revealed themselves as involving subjects of most topical interest—psychotherapy, sensitiveness to foreign proteins, vagotonia and endocrine balance. The treatment of asthma summarizes in itself, as it were, the chief trends of modern therapy. But there has been a tendency on the part of enthusiastic workers in some one of these fields to neglect due consideration of the other aspects. Successful treatment involves them all.

It is always a dangerous thing to try to express a disease in a formula. Nevertheless, I attempted to do so for asthma not long ago.¹ Hurst² has recently improved upon that formula, and I accept his emendation gladly. He has defined asthma as due to an unstable or irritable condition of the bronchomotor portion of the vagus nucleus, which causes it to react unduly to psychical or peripheral stimuli, or to foreign proteins in the circulating blood. I think this could still be modified with advantage by including the obvious vasomotor turgescence which accompanies the bronchomotor spasm, as this is always present, and is sometimes the most striking feature of an attack, just as it is in the closely allied condition of hay fever. Moreover, it is carried out by the same vagal mechanism. Thus modified the formula can be expressed diagrammatically as follows:—



¹ *Clin. Journ.*, July 7, 1920, xlix, p. 97.

² *New York Med. Journ.*, March 15, 1922, p. 347. In the *Lancet*, 1921, i, p. 1113, Hurst gives a definition practically identical with the one here suggested.

I will first consider the stimuli acting on the centre, and then some features of its response.

(1) *Psychical Stimuli*.—That asthma often occurs in neuropathic families, and that asthmatics are unduly suggestible, are well-known facts. The paroxysm excited by an artificial flower figures in every text-book. But it is extraordinary to witness the almost instantaneous relief that may follow a hypodermic injection of distilled water into a patient who is anticipating one of adrenalin, when this is known to be efficacious. It makes me wonder how much of the success of single minim doses of adrenalin are due to suggestion and how much to the drug. At any rate, it explains why the asthma curer who has profound belief in the efficacy of his treatment finds increasing justification for that belief. For his confidence arouses responsive confidence in his patient. But what is not so clearly recognized is that the effective stimulus often arises from a psychic conflict. Such a conflict may express itself at the psychic level of the nervous system as an obsession or a phobia, and at the sensori-motor level as a paralysis, a tic, a contracture or an anæsthesia; while if it sinks deeper to the visceral level it may express itself as glycosuria, exophthalmic goitre or asthma. Naturally other influences will help to decide in which of these ways the psychic trauma reveals itself. Thus, one sufferer from a psychic conflict who has to face a hostile environment may escape through an hysterical paralysis, but another who has to face the internal disorder produced by sensitiveness to foreign proteins is more likely to develop asthma.

(2) *Peripheral Stimuli*.—I have little to add to the common stock of knowledge of this subject. The influence of eyestrain, hay fever and other nasal troubles, sinus infection, gastric and intestinal disturbances, and of uterine disorders is well recognized. But I should like to call special attention to enlarged bronchial glands, especially in children, and to suggest that this may explain the liability of healed tuberculosis to excite asthma in susceptible subjects. Interesting observations by Baccarini show that peripheral irritation of the pleura, such as paracentesis, may excite an attack of asthma. In the epileptic a fit can be similarly produced, which leads him to compare bronchial spasm to a localized epilepsy. Other points of resemblance between epilepsy and asthma will readily occur to you.

Foreign Proteins.—Of recent years great attention has been paid to this factor. Morley Roberts has made the profound remark that "immunity is assimilation." There is one flesh of birds and another of beasts. From the welter of amino-acids which result from the disintegration of food proteins each animal has to build up its own characteristic and specific tissues. Specificity is chemical as well as morphological. To some foreign proteins we are naturally immune, i.e., we can assimilate them automatically; to others we acquire immunity, i.e., we learn to assimilate them. But to some foreign proteins immunity is neither congenital nor acquired. The tissues continue to resent the intrusion of such. They will not assimilate them. Such proteins excite anaphylaxis in varying degrees. Richet has defined anaphylaxis as the last stand of the race against adulteration of its protoplasm. In extreme degrees anaphylactic shock is fatal because assimilation would mean too profound an alteration of bodily structure. In less degrees anaphylaxis declares itself in violent attempts to get rid of the foreign invader. As Drury¹ has expressed it, the toxic idiopathies are conservative, self-repairing mechanisms under parasympathetic control.

¹ *Med. Journ. South Africa*, 1921, xvii, pp. 66-74.

The Influence of the Parasympathetic.—The great function of the parasympathetic may be defined as promoting the assimilation of suitable and the rejection of unsuitable material. Thus it starts the secretory and muscular processes of digestion, while it empties the rectum and bladder, and can reject food by vomiting. The excitation of bronchial catarrh and cough is similarly a method of ridding the body of unsuitable material, and it is interesting to recall in this connexion that drugs which are expectorant in small doses are emetic in larger ones. The similarity of the parasympathetic action on the alimentary and respiratory tracts is not surprising when we remember that the latter tract develops as an outgrowth from the former. The vagal factor in normal respiration may indeed be defined as a mechanism for insuring the alternate taking in of assimilable oxygen and the rejection of unnecessary CO₂. Confronted with an irrespirable gas the vagus checks its entrance to the lungs by laryngeal spasm. But I do not think we can in this way explain bronchomotor spasm, which, while tending to interfere with elimination, does not prevent the entrance of unsuitable material. This, I think, is an example of that exaggerated response which is so typical of pathological states. The undue irritability of the vagus centre sets up such a strong efferent impulse that is not confined to the vasomotor channels, but overflows along the bronchomotor fibres.

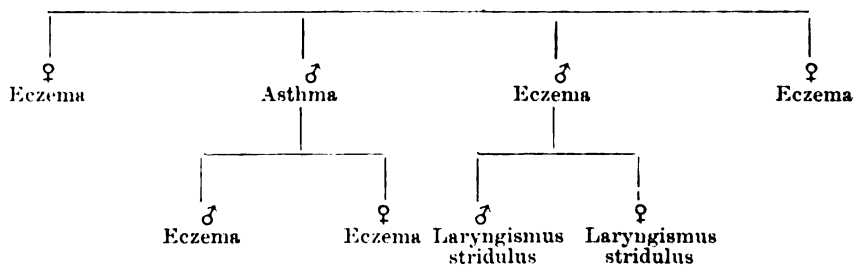
Without accepting the whole of Eppinger's and Hess's theories we may agree that the asthmatic is a vagotonic, and is liable to other manifestations of vagotonia. I recently had a curious instance of this in a man who suffered from both asthma and gastric ulcer, and who had found out for himself that he could relieve the pain of his ulcer by smoking a stramonium cigarette. And I should agree with the description which has been given of a form of mucous colitis as "asthma of the colon."

The Sympathetic and Endocrine Balance.—But vagotonia may be relative as well as absolute. In other words, it may well be that an overacting vagus is due to diminished action of the antagonizing sympathetic. One of the ways in which the sympathetic carries out its defensive function is by activating those endocrine glands which flood the blood with sugar, thus increasing metabolism and exciting a febrile response. Lest this excess of sugar should be wasted by overflowing into the urine, the threshold of the kidney is raised in fever. But the febrile response is not the only way in which immunity is acquired. One of the greatest gaps in our knowledge at the present time is our almost total ignorance of the part played by the endocrine glands in immunity. Beyond the fact that thyroid extract may increase the yield of amboceptors, following the injection of an antigen, we know hardly anything on this point. Yet I feel that further research will show that just as anaphylaxis is associated with vagotonia, the opposite condition of sympathicotonia is related to immunity through the endocrine system. Not infrequently hypo-adrenalinism follows or accompanies anaphylaxis, and certainly adrenalin has a valuable action in anaphylactic shock. It has a similar effect in some cases of urticaria (Harrower). Hurst¹ considers that the toxic idiopathies, including asthma, produce their effect, at any rate in part, by depressing adrenal activity. He believes that the influence of fatigue in inducing the asthmatic paroxysm is capable of a similar explanation. Morley Roberts suggests that so might be the apprehensiveness so common in asthmatics. The overactivity of the broncho-constrictor fibres of the vagus

¹ Loc. cit.

is kept in check by the broncho-dilator action of the sympathetic, which the secretion of adrenalin would stimulate. Hence the effect of an injection of adrenalin, or of sudden fright in checking an attack of asthma. Asphyxia has a similar action and produces the same effect; no doubt it is thus a factor in bringing the attack to an end without a fatal issue. I am aware that Stewart and Rogoff deny this emergency action of adrenalin, but Cannon's¹ recent experiments have disposed of much of their purely destructive criticism. The liability of asthma to come on during sleep, when the parasympathetic gains the upper hand, further points to the influence of disturbed balance between the two great branches of the visceral nervous system. The reinforcement of the adrenalin effect by pituitrin may be held to point in the same direction. The glands which co-operate with the sympathetic appear generally to help in checking the asthmatic paroxysm. The stimulating effect of the gonads in the sympathetic-endocrine group may account for the influence of uterine disease in exciting asthma. It must be admitted, however, that it is difficult on this hypothesis to explain why asthma is liable to appear at puberty and to be aggravated by pregnancy, when the thyroid and pituitary are stimulated, unless it is that some asthmatics have a gonadal deficiency which declares itself in this way. Certainly gonadal deficiency may be associated with thymic asthma, which is comprehensible since the thymus is an infantile organ, apparently antagonistic to the gonads. Recently I saw a case of what appeared to be thymic asthma in a stout girl aged 17, with amenorrhœa. Treatment with an ovarian extract was sufficient to stop the attacks. No element of suggestion came in here, for she did not know what she was taking. In general terms, it would appear that when the endocrine balance sways in the direction of the vagus, asthma is likely to occur in susceptible subjects; when the balance is redressed in favour of the sympathetic attacks may be cut short or prevented.

Asthma, Anaphylaxis and the Skin.—The skin undoubtedly has excretory functions. I recently had a severe and ultimately fatal case of uræmia, which first manifested itself by a marked toxic erythema. Urea was greatly increased both in the blood and cerebro-spinal fluid while in the cerebro-spinal fluid other amines were demonstrated to the extent of 0.24 per cent. The rashes of the exanthemata have often been compared with that produced by serum in anaphylactic subjects. This conception of an excretory mechanism is expressed in the popular idea of the benefit derived by "getting the rash out." The toxin which cannot be assimilated, i.e., to which the body is not immune, must be got rid of somehow, and at any rate from the vital structures. In this connexion it is interesting to note the occasional alternation of attacks of eczema and asthma. The following family tree which came under my notice is a striking example of this alternation in different members of the family:—



¹ *Journ. Ment. Nerv. Dis.*, 1921, liv, p. 421.

The condition of laryngismus stridulus is recognized as the infantile equivalent of asthma. It is further of interest to recall that both in skin diseases and in asthma eosinophilia is a usual feature of the blood picture. As an attack of asthma goes on and expectoration becomes more abundant, the eosinophils pass into the sputa. I should regard the skin reaction of an asthmatic to foreign proteins as an attempt to wash out the offending non-assimilable material from the skin. I was proceeding to look for the occurrence of eosinophilia in the urticarial wheals produced by this test, when Dr. Mackenzie Wallis informed me he had already observed it. I think this all accords with the observations of Kanthack and Hardy, thirty years ago, on the rôle of eosinophilia in bacterial infections and intestinal digestion, which have passed into undeserved oblivion.

TREATMENT.

These considerations help to provide a rational explanation for some methods of treatment which have been empirically used in the past. I hope also that they may help to co-ordinate the views of those working at different aspects of the problem, and to emphasize the necessity for all-round assault on every case of asthma from these various aspects. For some, asthma is merely a psychological problem; but that is to close one's eyes to the toxic factor. For others the toxic element is the primary thing, the nervous factor being merely the failure of the nervous system to co-operate successfully in getting rid of the toxin. This view has been well-expressed by Morley Roberts thus: "Why is there a spasm? What *is* the spasm but violent overacting surface tension pulling every cell in the small bronchi and alveolar spaces into its least form, as it tries to squeeze out irritants, tries to defæcate, so to speak. The sympathetic and parasympathetic rush in to aid. There is a tendency to over-rate nervous action. Cells live their lives, have their disasters, even without nervous interference except in stress, as they did in the beginning." But this view is not capable of explaining the influence of suggestion in actually producing a paroxysm in the absence of the exciting toxin. The suggestibility of the patient should be utilized to help him confidently to expect a cure. The effect of climate may often be due to suggestion. An asthmatic goes to a place and has an attack: therefore each time he goes there he expects to have one, and has it. The converse is also true. To point this out in the former instance may help him to avoid repetition. But search must also be made for deeper-seated psychological troubles. Naturally sources of peripheral irritation must be eliminated; this includes avoidance of late suppers and cold bedrooms. The skin reactions are in my experience very helpful in determining the foreign proteins to which the patient is sensitive. Not only in asthma should this be tried; it is worth trying in any vago-tonic with toxæmic symptoms. I recently had a striking example of this in a patient who was emaciated and profoundly depressed, but whose chief objective trouble was a painful swelling of the tongue and lips. Dr. Mackenzie Wallis kindly tested his skin reactions and found he was highly sensitive to cereals. In addition to other simple measures cereals were removed from his diet, and he was allowed many things which had formerly been prohibited in the belief that he had gout. The effect was dramatic; in one month he put on 16 lb. in weight. Only yesterday I received a letter from his medical man in the course of which he says: "I think his almost daily progress has been one of the most marked and interesting cases I have had under my observation for a long time. Socially he is entirely altered . . . bright and entertaining

6 Langdon Brown: *Asthma*; Bennett: *Gastric Function*

and his old-fashioned courtesy has returned. In fact he is so far a complete revolution and revelation." An experience like that gives one "furiously to think" of the toxic factors we must often overlook in cases diagnosed as neurasthenia, or "functional." Where the offending protein cannot thus be eliminated we should consider what methods of desensitization are feasible. I should like to add that in my experience it is most important to exclude the syphilitic toxin as the offending protein. I saw a case in a woman, aged 40, who had asthma during her last pregnancy; she was delivered of a macerated foetus and her Wassermann reaction was strongly positive.

The universal belief in the efficacy of the belladonna group in asthma finds its justification in the paralysing action of such drugs on the parasympathetic endings, just as the fillip given to the sympathetic by adrenalin or cocaine helps to redress the balance in another way. According to some, the influence of iodide is mainly to activate the thyroid: if this is so the benefit of this drug in asthma is comprehensible, since the thyroid secretion lowers the threshold to sympathetic stimulation. But so far we have no explanation of the way in which arsenic acts in this disease, though certainly it often seems to help. The importance of doing everything we can to restore an impaired endocrine balance is, I think, undoubted.

This all-round attack on the problems of asthma seems to me to afford the best chance of relief, especially if carried out before structural changes such as emphysema have occurred. After that a vicious circle is set up which is hard to break.

The Modification of Gastric Function by Means of Drugs.

By T. IZOD BENNETT, M.D.

(ABSTRACT.)

[This paper will be published in full in the *British Medical Journal*.]

IN no branch of medicine is the difference between the list of remedies used in practice, and that approved by pharmacologists as having definite actions, more noticeable than in the branch which deals with gastric disorders. Recent researches do enable us to point to certain substances as being proved experimentally to have definite effects, and they may best be classified into: (I) Those affecting gastric secretion; and (II) those affecting gastric motility.

(I) SUBSTANCES AFFECTING GASTRIC SECRETION.

(a) *Diminishing Gastric Secretion: Atropine.*—Medical literature shows an entire lack of agreement as to the potency of atropine in arresting gastric secretion, but the author has found its effect to be very marked. Especially is this so if the drug be used as a wash to the mucous membrane; curves of fractional gastric analyses were exhibited illustrating this; and the author gave it as his opinion that the effect of weak atropine solution on the gastric mucosa was as powerful as its effect when applied to the eye. In practice he had found atropine in great dilution a valuable remedy when given on an empty stomach.

(b) *Increasing Gastric Secretion: Pilocarpine.*—Pilocarpine applied locally is correspondingly less effective, because though, as shown by fractional

gastric analysis, it excites secretion by the gastric glands, it is rapidly absorbed and will then cause profuse salivation which frequently dilutes the total gastric contents and so lowers their acidity.

(c) *Substances affecting the Gastric Secretion after its Evolution*: (i) *Substances Neutralizing Acid Secretion*.—Having referred to the controversies as to the actual effects of the alkaline salts in cases of hyperacidity, the author showed curves of fractional gastric analysis which demonstrated the increase in secretion produced by bicarbonate, and the complete but temporary neutralization produced by magnesium hydroxide if administered after a meal. Summarizing the results of recent experimental work he said:—

(1) That sodium bicarbonate tends to excite the gastric mucosa to increased secretion and that this effect more than counterbalances its neutralizing effects.

(2) That magnesium hydroxide and bismuth carbonate are less stimulating and stronger in their neutralizing effects.

(3) That all such salts should be given after meals.

(4) That for arresting secretion atropine is much the most effective substance.

(5) That the true indication for sodium bicarbonate is in those cases with low acidity which show an excessive secretion of mucus.

(ii) *Substances replacing Deficient Acid Secretions*.—The author referred to the functions of HCl in the stomach, and pointed out the necessity of giving large doses if an effect were to be produced. Recent work has shown that in normal circumstances any added HCl is neutralized by duodenal regurgitation. Reference was made to the very potent action of HCl on bacterial growth.

(II) SUBSTANCES AFFECTING GASTRIC MOTILITY.

Having briefly referred to the modern work on the bitters, which has failed to show any gastric effects arising from the ingestion of these substances, the author discussed drugs which may influence gastric motility. He said that atropine does not usually produce any marked effect, but that in cases of gastric spasm it may be of great use. He compared its action in such cases to that of the digitalis series when they are employed for producing heart-block and checking tachycardia in cases of auricular fibrillation. For increasing gastric motility pilocarpine has been employed, but its effect was not marked unless dangerous doses were employed.

The recent work of Berti on strychnine was then described, a series of radiographic tracings being shown which demonstrated the effects of this drug on the stomach. Only in very small doses—less than 1 mgr., by mouth—can this drug be shown to cause more rapid emptying of the stomach; in larger doses an initial increase in peristalsis is followed by rapid tiring, with production of spastic contraction in the pyloric region. With large doses hour-glass constriction of the stomach is produced. The author emphasized the importance of employing only small doses of this drug in gastric cases.

In conclusion, reference was made to a series of experiments with adrenalin; although emotional states such as fear can readily be demonstrated to delay the emptying of the stomach the author had been unable to produce such effects with adrenalin. A large number of observations had been made, the drug being given both hypodermically and as a wash, but the effects both on gastric secretion and gastric motility has been negative.

The Effect of Light on the Response of Frogs to Drugs.

By E. BOOCK, B.Sc., and J. W. TREVAN, M.B.

(From the Wellcome Physiological Research Laboratories.)

FROGS (*Rana temporaria*) exposed to light on a white background until yellow, succumb to much smaller doses of members of the digitalis series than frogs which have become dark coloured by being shielded from light. Frogs vary in the rate at which they change colour. A yellow frog will occasionally remain yellow for some hours in the dark, and such a frog remains more susceptible to digitalis in the dark than a black frog. Black frogs more often die out of order in digitalis testing than do yellow frogs. The lethal dose for the black frog is about 1·75 times that for the pale frog.

Frogs rendered yellow by the injection of adrenalin 0·5 c.c., 1/10,000 per 20 gm. frog, are also more susceptible to digitalis than black or medium tinted frogs, while frogs rendered black by injection of extracts of the posterior lobe of the pituitary are less susceptible than controls.

The effect in this case is not due merely to alteration of absorption, for the lethal dose of digitalis when injected intravenously is affected in the same way by the administration of adrenalin or pituitary in pithed or intact frogs.

Macht¹ showed that quinine, when injected into frogs exposed to light, killed in smaller doses than when the frogs were kept in the dark. He attributed this result to an alteration of the quinine by the light—quinine being a fluorescent substance and absorbing the ultra-violet rays. We have tried the effect of pituitary extract and adrenalin on the minimal lethal dose of quinine, and find that, just as with digitalis, yellow frogs are more susceptible than black, this suggesting that a part at least of Macht's effect is an alteration of the frog and not of the quinine.

Hogben and Winton² suggest that the change of colour in frogs in response to light is mediated by pituitary or adrenalin secretion and our results are at least not a contradiction of this view.

These experiments suggest that when considering the mode of action of light on mammals, which is arousing so much interest at the present time, it would be well for us to bear in mind the possibility that the metabolic changes in mammals are not due to a photochemical synthesis in the skin, but to an action resulting from the stimulation of light-sensitive nerve endings in the skin or the retina, with the consequent reflex alteration of metabolism similar to that found in the experiments described above.

Biedermann³ claims to have shown that the receptors for the light reflex in the frog are situated in the skin of the animal.

¹ *Proc. Soc. Exp. Biol. and Med.*, 1922, xix, p. 397.

² *Proc. Roy. Soc.*, 1922, B, xciii, p. 318.

³ *Pflüger's Archiv f. d. ges. Physiol.*, 1892, li, p. 457.

Section of Therapeutics and Pharmacology.

President—Dr. W. LANGDON BROWN.

DISCUSSION ON "THE PRESENT POSITION OF ORGANOTHERAPY."

Professor SWALE VINCENT, D.Sc., M.D.

It has sometimes been maintained that in therapeutics as in other matters, only those materials and those methods which are found to be useful and beneficial will ultimately survive. In the ordinary practical affairs of life this anticipation may be well founded, but in such a matter as the employment of various substances to relieve pain, or to restore to the normal the deranged machinery of life, it would seem that we cannot even yet trust either the scientific discrimination of medical men or the common sense of the general public to eliminate the useless and to hold fast to that which is good. The reasons for this suspicion will be stated in due course.

At the present time many physicians are in the habit of prescribing thyroid and parathyroid extracts, pituitary preparations of various kinds, numerous adrenal substances, and material derived from the digestive glands, the reproductive organs, the thymus, lymph glands, prostate, mammary gland, and the pineal body. Less frequently extracts of nervous, muscular, and other tissues, are employed. It is not unusual to recommend various mixtures of glandular extracts.¹ In brief, the doctrine and practice of organotherapy involve the therapeutic use of practically every organ and tissue in the body. The belief in the efficacy of all these substances is based upon a widely current view in regard to the internal secretions, namely, that all organs and tissues in the body manufacture and pour into the circulation substances the function of which is to regulate the activities of different parts of the nervous system, and, either through this system or in some other way, to influence the general metabolic activities of the animal economy. If any of these be lacking or deficient, what is easier than to replace them by administering preparations containing them? This general theory has many ramifications and complications. The untenable hypothesis that the secretion of the chromophil tissues maintains or helps to maintain the tone of the blood-vessels and the normal blood-pressure, has given rise to the whole fabric of vagotonia and sympathicotonia so dear to credulous physicians, and has even stimulated some imaginative authors to give names to non-existent "hormones."

When it became clear that physiological experiments lend no support to

¹ One of the most recent and perhaps the most foolish of such preparations is a combination of desiccated pancreas, tonsil, and duodenal mucosa, to be taken by the mouth as a remedy for diabetes!

the view that all parts of the body furnish internal secretions; when it could no longer be maintained, with any show of reason, that the organism is liable to suffer from a deficiency of any one or of several of these secretions; then a large number of physicians insisted on the value of organic extracts urging that, although they may contain no physiologically active principles, they are found empirically to do much good in various diseases and disorders. This attitude is difficult to criticize on *a priori* grounds because some few cases may be adduced where the value of a drug has been discovered by purely empirical means. But in the majority of the cases here referred to it has been possible at a later period to correlate the observed therapeutic value with a definite pharmacodynamical action subsequently discovered. It would be difficult to mention a drug which is clearly and beyond doubt of value in the treatment of disease and yet which possesses no known pharmacodynamical effects. It is not necessary, I imagine, to expand this thesis, or to give illustrations.

When we turn to organotherapeutics we find a large number of substances recommended for administration by the mouth, which, so far as can be ascertained by careful experiment, produce no physiological effects of any kind when given in this way. When this is the case, and when physicians persist in administering such preparations, say, of muscle or of ovary, they must be called upon to defend their action by careful records of cases and well authenticated instances of beneficial results.

Although extracts made from the majority of organs and tissues produce no physiological effects when given by the mouth, yet when they are administered beneath the skin or into a vein some influence on the organism may be produced. Thus, tissue extracts in general when injected subcutaneously into dogs and cats, have a mild but noticeable stimulant or irritant action, lasting perhaps for a few hours. When such extracts are applied intravenously the blood-pressure may be lowered and other less obvious changes produced. These effects, however, are not specific, and, at best, are only temporary. The substance present in tissue extracts which lowers the blood-pressure when introduced into the circulation has by some been supposed to be choline, by others histamine. Its real nature is yet unknown, but we may assert confidently that it is neither of these. There may, of course, be several active substances; some or all of them are soluble in water, alcohol, and ether. No one has been bold enough to attempt to correlate these physiological actions with any kind of possible therapeutic action of the extract. Or, at any rate, the theories which bear upon this subject are so groundless, so fantastic and so complicated, as not to deserve serious consideration. The extracts given medicinally are recommended for reasons quite independent of the physiological action just referred to, which indeed is usually unknown to the physician.

It is true that in addition to the thyroid and the posterior lobe of the pituitary there may be a few instances in which feeding with an organ for a long period may produce some effects upon the rate of growth in young animals, or even upon the growth of special organs or tissues. Thus adrenal substance (cortex) when administered to young growing animals seems to induce a growth of the testis greater than in the control animals. I do not believe, however, that there are many such instances, and such as there are can only be revealed by the administration of very large doses and by very careful observations and measurements. It is difficult to believe that in the present state of our knowledge any result in practical therapeutics is to be derived from such findings.

In the whole province of therapeutics there are many fallacies and pitfalls.

The ancient stumbling block, the relation between *post hoc* and *propter hoc* looms large in our path. The apparently simple question, "Have we done our patient any good by the giving of this drug?" is often very difficult to answer. We are all aware that many aches and pains disappear without any treatment. And we all frequently suspect that even when medical treatment has been adopted the subsequent recovery of the patient has had nothing to do with the treatment—in some cases it may have occurred *in spite of* the treatment. It has been aptly said "It is a poor cure that will not find its lucky patients." What makes the difficulty greater is the circumstance that for various reasons (most of which are obvious), and in the class of illness I am referring to, treatment is often begun just at the time when things are at their worst and are about to return to normal. A doctor is called in the middle of the night to see a highly nervous woman suffering from paroxysmal tachycardia. His arrival acts as a soothing influence and he is constrained to administer some one or more drugs from among those which are reputed to be sedatives to the heart; but a bright idea strikes him and he gives a dose of extract of parathyroid glandules. No sooner has the substance passed the pillars of the fauces than there is a sudden change for the better, and in an hour's time he leaves his patient apparently as well as she was before the attack. Now he probably ignores the fact that his patient was going to get better in any case soon after the time of his arrival. He forgets the wonderful effect of the mere presence of the doctor; he chooses to disregard the possible effects of the other drugs and he attributes the recovery to the parathyroid extract, which, by the way, was very possibly derived from some other tissue and not the parathyroid. Then he publishes the case, and it will not be astonishing if he claims not only that parathyroid extract is a useful drug in the treatment of paroxysmal tachycardia, but that this condition is in fact due to a hypofunction of the glandules.

It seems clear that if the value of a drug is to be ascertained and demonstrated something in the way of a series of *experiments* must be carried out. It would be presumptuous on my part to point out precisely how a clinical worker should conduct therapeutical experiments. It is only necessary to remind such workers that one of the essentials in conduct of these, as indeed of any experiments, is the institution of adequate and rigid controls. It is obvious to any scientific reader that the endless records of single cases alleged to be cured by this, that, or the other drug, the records of which litter our medical journals, are not worth the paper on which they are written.

If all this is true in the realm of general therapeutics it is much truer in the province of organotherapy.

Thyroid Medication.

Murray, in the year 1891, discovered that the administration of thyroid substance cures myxœdema, and up to the present time thyroid material has proved to be one of the most satisfactory drugs at our disposal. Thyroid extract and its active principle thyroxin bring about an increase of the total metabolism of the body. The effect is most marked where there is a deficiency of the thyroid secretion.

There is no need to lay stress on the value of thyroid preparations in myxœdema and cretinism. In lesser degrees of hypothyroidism (*myxœdème fruste*) there is little reason to doubt that thyroid treatment is very valuable. The difficulty in these cases lies in the diagnosis. If the symptoms of obesity, dryness of the skin, slow pulse, &c., are found concomitantly with a low basal

metabolic rate, then one may reasonably hope for a good result from thyroid treatment. In some cases of goitre thyroid extract is useful, but iodides are more efficient in the hands of many physicians. In obesity thyroid medication has been found to be very valuable, but if carelessly applied it may result in much harm. Many observers state that thyroid substance does good in rheumatism, rickets, mental disease, epilepsy and other conditions. It is of course quite conceivable that a drug which stimulates the chemical activities of the body may be valuable in a variety of conditions. If there is any good at all to be obtained from the so-called polyglandular preparations, this is most probably attributable to the thyroid substance contained in them.

Parathyroid Medication.

This has been used in many different conditions, but with very uncertain effects. In tetany some good results have been reported, but oral administration is admitted to be less beneficial than implantation. In other conditions even the more optimistic "endocrinologists" confess that negative results are usually obtained. It must be urged that there is no satisfactory evidence that any form of tetany (except that which often occurs after operations upon the thyroids and parathyroids) is due to a lesion of the parathyroid bodies. It seems certain that tetany is a name applied to a condition which may arise from a great variety of causes.

Pituitary Medication.

Recent work on the pituitary body has compelled us to readjust our views on the whole subject. It now seems tolerably certain that many, if not all, the symptoms which were supposed to be due to damage to or lesions of the organ are in reality the effects of injury to the hypothalamus. It has never been shown that treatment by pituitary extracts has the slightest effect in remedying the symptoms thought to be due to pituitary insufficiency. Posterior lobe preparations have been found useful as drugs in the treatment of polyuria (diabetes insipidus) and by gynecologists to aid the contraction of the uterus. Their influence upon smooth muscle has also led to their successful employment in meteorism. These uses of posterior lobe preparations have, however, no bearing on the functions of the organ. They are not instances of a "substitution-therapy."

Adrenal Medication.

The value of preparations made from the chromaphil tissue of the adrenal body is well known. Adrenin is in everyday use by physicians and surgeons. But this value bears a very problematic relation to the function or functions of the adrenal body as a whole, or to the services possibly rendered to the animal economy by the chromaphil tissues. Here again, as in the case of posterior pituitary extracts, it is not a question of "substitution-therapy." The study of the physiological effects of extracts of the chromaphil tissues has led to the employment of certain substances (amines) of known chemical constitution and well-ascertained pharmacodynamical activity. The discovery of a certain active substance in the tissues has served as a suggestion as to the value of materials having a definite chemical constitution. We do not yet know the chemical nature of the active substance contained in the posterior lobe of the pituitary. When we do, it will not be necessary to use pituitary material at all.

In Addison's disease adrenal preparations do not seem to be of the slightest value. Transplantation of a human adrenal gland has been alleged to lead to improvement in the condition of the patient. It seems clear, at any rate, that adrenin produces no effects of any kind, in health or disease, when given by the mouth, unless it be on the lining membrane of the stomach.

Testicular and Ovarian Medication.

It is more than doubtful whether extracts and preparations made from the ovary and testis produce the slightest effect of any kind, in health or disease, when administered by the mouth. Yet such preparations are not only ordered frequently by medical men, but are bought and swallowed by the general public on its own initiative. When given subcutaneously there may be observed a mild stimulant or irritant effect, which, however, is not specific, but produced by material from all kinds of organs and tissues. This effect has been already referred to.

In the cases of the testis the methods of Steinach and of Voronoff ought to be mentioned. The former ties one vas and so puts an end to the spermatogenic functions of one testis. Then, it is alleged, the "puberty gland," the interstitial tissue, hypertrophies, and its internal secretion is increased in amount, and "rejuvenation" is the result. Voronoff does not think that this method is calculated to give good results because he attributes the internal secretion to the whole testis and not specially to the interstitial cells. And so he recommends and carries out transplantation of testis, including both kinds of tissue. It is too early either to appraise the value of these rival claims or to express a positive opinion as to the results which may be expected to follow either of the methods. It must be remembered that unless blood-vessel suture be employed grafting is at best a temporary expedient, and that the best results can only ensue when material from human beings or the anthropoid apes is employed.

Pancreatic Medication.

Although it is generally recognized that the pancreas furnishes an internal secretion, the elimination of which may give rise to a condition identical with or closely resembling the disease known as diabetes, yet the administration of pancreatic extracts has not proved to be of any value in the treatment of this disease. This, at any rate, was the condition of affairs until within the last few months. But quite recently a group of workers in Toronto, using an alcoholic extract, report that they can reduce the blood-sugar level in both normal and diabetic (depancreated) animals. Further, it is stated that the observations on dogs have been confirmed by similar observations on several human diabetic patients.

It will be wise not to be too sanguine in regard to these new pancreatic preparations. But it seems probable that a step in advance has been made, and it is not out of the question that sooner or later we may have at our service a preparation made from the pancreas which will benefit diabetic patients in a degree comparable with that of thyroid extract in myxœdema. The preparation of Banting and Macleod has to be given subcutaneously. Mackenzie Wallis claims that he has prepared a pancreatic extract by a method practically identical with that of the Canadian investigators, and that this preparation is effective when given by the mouth.

Summary.

If we seek for cases where it is possible to apply a true substitution-therapy—artificially to replace the internal secretion of a gland—we have no clear instance except the solitary one of the thyroid. It may be that we shall before long be enabled to include the pancreas in this category.

There are, however, some few instances in which extracts of organs, or purified chemical substances obtained from them, are valuable as drugs, apart altogether from the question of internal secretion. Such are adrenin and extracts from the posterior lobe of the pituitary body.

Of the rest of the gland and tissue extracts and preparations, it might perhaps be well to say nothing, if it were not their exploitation on the present-day scale constitutes a very formidable kind of quackery. There is no subject upon which so much utter nonsense has been talked as upon internal secretion, and organotherapy, or at any rate a large part of it, may be defined as the application of this nonsense to practical medicine. In the meantime certain firms of manufacturing druggists are making the most of a unique opportunity and are growing rich by reason of the inadequate education of medical practitioners and the notorious ignorance of the general public on all matters relating to their own bodies.

DR. GEORGE R. MURRAY.

A discussion on organotherapy at the present time will serve a useful purpose. Certain lines of treatment by this means are based on sound physiological principles and have proved to be of great value. On this sure foundation we must be content to build up our knowledge by reliable methods so that the structure may be sound, and it is fortunate that this discussion has been opened by a physiologist who has such a wide knowledge of this department of physiology. Unfortunately, owing to the enthusiasm of some speculative writers, a great deal of ephemeral literature has appeared, much of which is of no value, and some has been positively harmful to the object we have in view. The term "organotherapy" should be confined to the employment of the products of the excretory, secretory and incretory glands in the treatment of disease. Other tissues should be regarded as foods rather than drugs. The excretory glands, from the nature of their function, do not promise much help in this direction. Urea may be mentioned as a substance which, though not produced by the kidney, is excreted by it, and has well known uses in medicine. The secretory glands are those which deliver their secretion through a duct to a surface of the skin or of a mucous membrane. The incretory or endocrine glands are ductless and discharge their products into the blood, lymph or cerebro-spinal fluid. Some glands, such as the pancreas and gonads, supply both a secretion and an incretion. The products of some secretory glands have been in common use in medicine for many years. Pepsin and pancreatin may be mentioned as members of this class, the value of which is well known and does not call for further consideration. On this occasion we are chiefly concerned with the uses of the incretions and the conditions in which their value has been definitely proved. As the physiological principles upon which organotherapeutic treatment is based have already been so clearly put before you by Professor Swale Vincent, I shall confine my remarks to the conditions in which such treatment is of proved value.

The thyroid gland supplies an incretion containing at least one active

hormone, thyroxin, of which the composition is known. Fortunately the thyroidal hormones can be absorbed unaltered in activity from the alimentary canal, and can therefore be given by the mouth either in the raw gland or in special preparations, such as a liquid extract or a dry powder, which can be so conveniently dispensed as a tablet of any desired strength. The first and most important use of these preparations is to supplement a failing thyroid gland which, owing to fibrotic atrophy or other destructive lesion, can no longer provide even the minimum amount of hormone necessary for the maintenance of metabolism at the normal level. Under these circumstances the symptoms of hypothyroidism in its lesser degrees are present, as indicated by mild cretinism in the child and slight myxœdema in the adult, and call for early recognition. In these cases small doses of 5 minims of liquor thyroidei or of 2 or 3 gr. of dry thyroid each day will be followed by steady improvement and the disappearance of all symptoms due to thyroidal insufficiency. In the later stages of thyroidal atrophy, which are accompanied by well marked cretinism in the child or myxœdema in the adult, correspondingly larger doses produce equally successful results, provided no incurable degenerative change in any other organ has developed. In such cases, after removal of the symptoms, the patient can be kept free from any signs of hypothyroidism as long as ever the treatment is adequately maintained. When thyroid treatment is begun there is a latent period of a week or more before any definite effect is observed. Kendall has shown by repeated observations on the basal metabolic rate that after a single injection of thyroxin in a case of myxœdema there is a latent period with a slow increase of reaction which reaches a maximum at about the tenth day and persists for five to seven weeks. Boothby has calculated that in order to maintain a normal basal metabolic rate, 12 to 14 mgm. of active thyroxin must be present in the body, and that the thyroxin is used up at the rate of about 0.5 mg. a day. In order to maintain a normal basal metabolic rate in a fully developed case of myxœdema due to complete thyroidal atrophy it would require a daily intravenous injection of this amount of thyroxin. A larger amount is necessary when given by the mouth. Plummer and Boothby estimate this at about 1.5 mg. daily. From clinical observation of cases which have been successfully treated for many years I estimate the equivalent amount of liquor thyroidei (B.P. 1898) to be 10 minims or 5 gr. of fresh gland, as this is a sufficient daily dose to maintain health after complete atrophy of the thyroid gland. In the treatment of simple parenchymatous goitre a daily dose of 2 gr. of dry thyroid is often very useful. If any symptoms of hyperthyroidism are present its use is, of course, contra-indicated. As the thyroidal hormone is a powerful stimulator of metabolism, there are other conditions in which its use is beneficial. In diseases of the skin, and specially in the treatment of psoriasis, remarkable effects may be obtained, as has been shown by Dr. Bramwell. Dry thyroid is also of considerable value in the treatment of chilblains, especially if calcium is given at the same time. In simple obesity thyroid is seldom beneficial unless it is combined with a suitable diet, and it must be used with caution, or harm may be done if large doses are given. In some menstrual disorders, and in nocturnal enuresis, Dr. Leonard Williams has shown the beneficial action of quite small doses.

In tertiary syphilis good results have been obtained, due probably in part to the iodine contained in thyroxin. The stimulation of metabolism at the same time probably aids the action of the iodine on some resistant lesions, especially when the skin is involved. We know very little about the relationship of the thyroid gland to the alimentary canal. Constipation is usual in

myxœdema, while frequency of defæcation is common in Graves' disease, so that the thyroïdal hormones are evidently stimulators of intestinal peristalsis and relieve some forms of constipation. I may briefly mention a recent case which suggests that possibly thyroid treatment has other uses in the treatment of diseases of the alimentary canal.

On August 11 a woman, aged 48, was admitted to my ward at the Manchester Royal Infirmary and gave a history of recurring attacks, at intervals of several years, of gastric pain and hæmatemesis from the age of 18. As a result of two separate X-ray examinations it was reported on September 1 that "there is an irregularity of outline associated with an hour-glass condition of the stomach and the five-hour meal showed considerable delay in emptying. The X-ray examination suggests an ulcer fairly high up on the posterior wall." Operation was advised by a surgical colleague but as the patient had signs of early myxœdema it was decided to treat this condition first. As the symptoms of myxœdema disappeared the gastric symptoms also subsided and the X-ray report of October 17 was as follows: "Stomach outline quite regular. Duodenal cap well formed. Nothing abnormal seen in duodenum. No delay in emptying." So no operation has been performed.

Parathyroid Glands.

For many years the function of the parathyroid glands has been under discussion; some have held them to be of great, others of little importance. There seems, however, to be good reason for the view that they neutralize or prevent the formation of guanidine, excess of which is probably a cause of tetany. These glands are also concerned with the regulation of calcium metabolism. Dr. H. Vines holds that in addition to the action of parathyroid gland substance in the relief of tetany it also stimulates a leucocytosis and is of value in the treatment of chronic septic conditions. The best results are obtained by a single daily dose of $\frac{1}{10}$ gr. of the dried gland by mouth.

Suprarenal Glands.

We know that Addison's disease is due to a destructive lesion of the suprarenal glands, and we also know that adrenalin can be extracted from the medulla of these glands, but there is a large gap in our knowledge between these two observations. Administration of the whole adrenal gland or of any preparation of it is quite unable to take the place of the functionless gland. I have never seen permanent improvement take place in a case of Addison's disease as the result of this treatment. A few recoveries are recorded but it is probable that in these cases, as was found in one case, the symptoms are due to loss of one suprarenal gland, and that recovery is due to the development of a compensatory hypertrophy of the other. We may reasonably look forward to a satisfactory treatment of Addison's disease, but the right way to supplement adrenal deficiency has not yet been found. We have, nevertheless, in adrenalin an extremely valuable remedy the uses of which, owing to its action on the sympathetic nervous system, are well known. I need only remind you of its value in the treatment of shock and of its local action in arresting hæmorrhage in epistaxis and gastrostaxis. Another valuable use of adrenalin consists in its relaxation of spasm of involuntary muscular fibres and it is now a well-known remedy for asthma. The hypodermic injection of from 1 to 5 minims of the official solution gives prompt relief in many cases and the continued daily use of even large doses for several years in order to prevent the asthmatic attack has not, to my knowledge, had any harmful effect. Dr. Brian Melland informs me that in one of his patients almost daily doses of as much as 15

minims of adrenalin solution have been used for more than fifteen years for the control of asthma and that the blood-pressure in this case is lower than it was formerly. The use of adrenalin in the treatment of œsophageal spasm, as first suggested to me by Professor J. N. Langley, has had a remarkable effect when swallowed by patients in 15-minim doses ten minutes before meals in the few cases in which I have employed it [3]. Dr. Ashby informs me that he has had excellent results in the treatment of this condition in young children by adrenalin.

Pituitary Gland.

What has already been said with regard to the suprarenal glands applies largely to the pituitary gland. Assuming that adiposo-genital dystrophy is due to loss of pituitary hormones we are unable to replace them by glandular preparations given by mouth, or to remove the signs of this disorder by this means. Pituitrin is a valuable remedy when given subcutaneously as a stimulant of involuntary muscular contraction. It has a well recognized place as a uterine stimulant in obstetric practice but should only be used under well-defined circumstances, or disastrous results may follow. Dr. Dale [4] has shown that there is an enormous difference in the strength of different preparations of pituitrin and this may account for the rupture of the uterus which has been known to occur after its use. It has proved useful in the treatment of paralytic distension of the bowel, provided there is no mechanical obstruction. As a temporary cardiac stimulant in acute disease, such as pneumonia, pituitrin in my experience has been of use when strychnine and camphor have failed.

Gonads.

There is very little satisfactory evidence to show that either testicular or ovarian preparations have any therapeutic action when injected or taken by the mouth. The development of eunuchism after removal of the testicles and the corresponding condition after removal of the ovaries are generally considered to be due to loss of internal secretion of the gonads. There is, however, no clear evidence that either eunuchism can be removed by the use of testicular extract or the signs of a premature post-operative menopause averted by taking ovarian tablets. The relief of subjective sensations has been reported, but this is not sufficient evidence. We still have to learn how to administer the internal secretions of the gonads in an effective form, if we are to succeed in making a eunuch, for example, grow a normal amount of hair on the face. An active preparation of the pancreatic incretion has long been sought and the recent achievement of Banting and Best in obtaining in insulin an efficient extract of the islands of Langerhans encourages us to hope that further researches may yield new and useful therapeutic agents from the endocrine glands and teach us how best to employ those we already possess.

In summarizing the present position of organotherapy we may certainly claim that in preparations of the thyroid, parathyroid, pancreas, suprarenal and pituitary glands we possess agents of proved value. In the case of the other endocrine glands while recognizing the importance of their functions we cannot fairly claim that we know as yet how to obtain or employ active preparations made from them. To those of us who have devoted attention to endocrinology for many years the recent exploitation of organotherapy for all kinds of diseases is deplorable, as it is apt to discredit what is a valuable means of treatment when properly employed.

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Dr. W. R. GROVE.

My remarks deal mainly with the clinical aspect of parathyroid treatment. It was through investigations of calcium metabolism in the blood that my introduction to the use of parathyroid took place, and therefore I must extend my record some years back. In the *British Medical Journal* of 1906,¹ Stephens first drew attention to the good effect of calcium salts, chloride and iodide, in ulcers and other diseases; this at least was my first introduction to the use of calcium salts. I was able to confirm his good results in the treatment of ulcers and, in addition, of many catarrhal conditions of the mucous membranes, bronchial and intestinal, and some kinds of eczema, all of which seemed to benefit equally from the administration of these salts. Moreover, for many years, acting on the suggestion of Dr. Dixon, of the Pharmacological Laboratory at Cambridge, I had been injecting intramuscularly calcium chloride in 1 gr. doses, diluted 1 in 100, in the treatment of hæmorrhages which could not be reached surgically. Here again the results were more than satisfactory, but they were purely clinical. I could not find what happened in the blood as the result of such injections, and therefore it was impossible to say when an injection could be safely repeated. On the other hand Dr. Dixon told me that however good the results might be from calcium salts taken by the mouth, they were certainly not due to the absorption of these salts into the blood, for the total amount could be recovered from the fæces; but he would not say they were not absorbed into the blood from the stomach and secreted again into the large intestine and thus in passing through the blood some result might be obtained.

So that two and a half years ago my position was this, that I was a thorough believer on clinical grounds in the effect of the administration of calcium salts both by the mouth and by intramuscular injection, on certain morbid conditions of the body, especially in aiding healing and the natural repair of tissue; but I was completely in the dark as to what was happening. It was then that Dr. Vines, who knew of my interest in calcium and had perfected his method of estimating blood calcium, asked me to collaborate with him from the clinical side of the work.

The first case taken was that of a prematurely old woman, aged 55, with a large and long-standing varicose ulcer, completely encircling the leg. She was taken into hospital and kept in bed. She was found to be very deficient in calcium; there was very little more than 40 per cent. of normal. Intramuscular injections of calcium chloride were given at weekly intervals and the calcium content of the blood was increased, but never more than to 80 per cent. of normal; this was always varying and, generally, it could be said that the higher the calcium content of the blood, the greater the improvement in the ulcer. Injections were stopped for three weeks and calcium chloride was given by the mouth; the blood calcium then ran at a lower level and the ulcer increased in size. By this time some half-dozen other cases were also under the same treatment; they all showed a calcium deficiency, improved by intramuscular injections, though never completely reaching normal; all showed improvement in the condition of the ulcers with the higher content of the blood calcium, while calcium administered by the mouth showed no improvement in the blood and little in the ulcer. We were therefore in the position of being sure that calcium was a factor in healthy healing; as Dr. Dixon had

¹ *Brit. Med. Journ.*, 1896, ii, p. 138.

predicted, little good was done by oral administration, but more by injection into the muscles.

After two hundred days of these experiments, during which time half-a-dozen cases had shown the same reactions, we had not been able to establish the state of normal calcium in the blood. It was then that we began giving parathyroid gland in $\frac{1}{10}$ gr. doses daily. In every case the blood rapidly became normal, never taking more than a fortnight to become so; and the ulcers began to heal rapidly. At the end of five weeks from the time of beginning parathyroid treatment, the first and largest ulcer which had completely encircled the leg, and which had gone on with varying results for some two hundred days before commencing this treatment, was completely healed. In the case of the smaller ulcers this result came much more rapidly.

We had therefore reached the point of being able to say, in the case of lesions which could be watched, that calcium was an important factor in their healing, that deficient calcium was combined with non-healing, that parathyroid was capable of influencing calcium metabolism, reducing a deficiency in it to normal, and with normal calcium healing speedily occurred.

After this ocular demonstration we then tested the blood in many other diseases in which the lesion is not apparent; and naturally among the first were gastric and duodenal ulcers. In all of these the blood calcium was deficient and the administration of parathyroid produced beneficial results in clinical symptoms, though the blood did not always very quickly become normal, and even when it had reached that point or near it, relapses in the content were not uncommon. At the time we believed this to be due to an increased absorption from the ulcer itself, neutralizing the stimulating effect of the parathyroid. Moreover, in some cases after stopping parathyroid administration definite clinical relapses occurred. Then we had some trouble with new cases of varicose ulcer: the previous series had been old chronic cases, usually without pain, and these had speedily healed up, but in the new cases the ulcers were small, just beginning, very painful, and refusing to heal. After the experience of the first series these results were disappointing, and the advisability of a search for a previous focus of sepsis occurred to me. In every case this focus was easily found, most often in pyorrhœa; and after the offending teeth had been removed, the parathyroid acted at once. Further, the old series of varicose ulcers was again examined, and in every case some other focus of sepsis was found. It therefore seemed that ulceration only occurred in varicose veins when the body resistance to a septic focus had broken down.

There is no doubt that gastric and duodenal ulcers arise from a similar cause, and if the original septic focus can be found and cured parathyroid will aid in the rapid healing of the ulcer. Several undoubted cases of long standing treated on these lines have remained free from dyspeptic symptoms under ordinary diet longer than ever before, and the patients are satisfied they are cured.

Connected with these cases an acute phlebitis with oral sepsis was also found to be calcium deficient. Under parathyroid treatment the pain and swelling rapidly disappeared, the teeth were extracted, and now a year afterwards the man says his veins have never given so little trouble. Bedsores and severe burns have likewise been assisted to heal by parathyroid, and in the case of burns the calcium deficiency may help to explain the complication of duodenal ulceration.

The question as to whether a calcium deficiency may be considered as an

index of the absorption of a toxin received confirmation in the experimental injection of the prophylactic vaccine against influenza into myself on two occasions. In both instances my calcium content was normal before injection; in twelve hours the deficiency was at its maximum, and in the first instance had not returned to normal in thirty-six hours, but in the second (three weeks afterwards) it was normal this time.

With these results it is not surprising that all diseases of the rheumatic group showed invariable calcium deficiency—rheumatoid arthritis, osteoarthritis, chronic rheumatism, sciatica. And in every case there were undoubted symptoms of improvement after the administration of parathyroid. In a case of true osteo-arthritis with ankylosed joints there was only lessened pain: but in rheumatoid arthritis there has generally been a cure, especially where the septic focus could be found and satisfactorily treated. But even if the original sepsis cannot be cured a favourable result is not impossible.

A case of non-articular rheumatism in an old lady, following an acute cholecystitis, is practically the only failure. Here the blood was very resistant to parathyroid, taking some six weeks to reach the normal, and, practically, she never experienced any good effects, except that she thought she slept better. The sepsis under tension was apparently too much for the remedy.

In cases of sciatica there was found an invariable calcium deficiency, and the administration of parathyroid gave speedy relief to the pain, so that the patients were in a few days able to see the dentist they had neglected.

In all these usually disappointing diseases of this group, in spite of the absolute failure mentioned, I am convinced no other ordinary treatment is as satisfactory.

In acute rheumatism calcium deficiency was found, and was watched under salicylates; as improvement sets in the calcium content improves and a relapse was not foretold by further deficiency, but seemed to be contemporaneous. If a bruit developed parathyroid was given, and in all cases disappeared eventually. Whether parathyroid was a factor in that recovery I cannot say, but on general principles it is well worth a trial in such cases. Pericarditis has also shown an early improvement under parathyroid exhibition.

It is one of our regrets that during these investigations we have not been able to find a case of malignant endocarditis; in this disease it is certain that calcium deficiency will be found, and the blood should be examined once a week to see if reaction is possible.

Other acute diseases—diphtheria, scarlet fever and influenza all showed calcium deficiency, and it is probable that complications following these are due to the natural reaction being unable to overcome the prolonged action of the toxin, and possibly parathyroid may aid this reaction.

Of nasal infections, unrelieved by operation or when it has been refused, there are no cures to be recorded, yet the relief has been greater than that given by any other treatment, and as long as parathyroid is continued the patients are comfortable and both look and are in better health than they have been for years.

A gumma of the face gave a remarkable reaction to parathyroid. For months it had been very resistant to antisyphilitic remedies, and when parathyroid administration was begun the patient was in her third series of arsenical injections. These were stopped and the blood was deficient in calcium. In a week under parathyroid, the calcium content was normal and in twelve days the ulcer, from being the size of a florin, was completely healed, and after fourteen months it remains so.

Another striking success was a case of herpes zoster of the conjunctiva with iritis. The patient, a man aged 60, had been under treatment for four months, six weeks in the London Hospital. It was still unhealed, and his vision was only perception of light. He was calcium deficient. Within a few days of beginning parathyroid treatment the chemosis improved, the power of vision increasing almost daily, so that in a month he could read print fairly well. A similar case was that of a patient with hypopyon following trauma, which completely absorbed under parathyroid administration after removal of septic teeth. In a case of delayed healing of fracture, also with calcium deficiency due to a very bad state of the teeth, the patient refused dental treatment, but under parathyroid the deficiency improved and the fracture healed. A child suffering from a very chronic otitis media was also calcium deficient, and under parathyroid the discharge first increased and then slowly diminished.

In the treatment of pernicious anæmia there is a hopeful field; two cases are now under observation; there is calcium deficiency yet improvement is obvious, it is too early in such a disease to make any definite prognosis.

In cases of ulcerating and inoperable carcinomata, calcium deficiency has been found, but under parathyroid treatment the blood has improved or become normal with simultaneously a great decrease in pain, so that morphia could be almost stopped. In the case of cancer of the stomach there was less vomiting and in one case there was an increase of weight for some weeks, with a complete abatement of symptoms and a feeling of perfect health.

All of these cases are clearly septic diseases, but enlargement of the prostate has never, I believe, been attributed to sepsis; yet about a dozen cases have shown calcium deficiency and have been treated with parathyroid and many more have received the treatment without blood examination with striking results.

J. C., aged 81, was first catheterized on August 8, 1921: he had much hæmorrhage; he was sent into hospital and refused operation. He did not regain power, and was sent out with a catheter and was seen on October 6 quite unable to pass anything without his catheter. He was calcium deficient, and was put on parathyroid treatment, and by October 18 his serum was normal and two or three days previously he had passed a little urine spontaneously. By November 5 he was passing more and more spontaneously, he thought about a pint a day, but he still used the catheter twice daily. On December 15, two months from the commencement of treatment, he left off using the catheter for good. He is now a healthy old man with a good complexion, disturbed only once or twice at night; he takes parathyroid at intervals, and has kept well up to date, though he steadily refuses to part with three septic teeth.

In our earlier papers [2] Dr. Vines and I described cases of tachycardia, chronic tonsillitis, neurasthenia, arteriosclerosis with and without anginal attacks, high blood-pressure, and certain cases of menorrhagia, in all of which there was calcium deficiency, and all showed definite improvement with parathyroid.

Dosage.—To standardize results the same preparation of parathyroid has been used throughout. Of the preparations on the market as estimated by Dr. Vines, all are not equally potent, one in fact was practically inert. The dose given has been one tablet of $\frac{1}{10}$ gr. daily, and in cases in which the blood has been slow in returning to the normal state, one twice a day has not hastened matters. In view of the threatened shortage of parathyroid, I am not sure that the dose could not be lessened. No harmful nor untoward effects have been found. One correspondent has told me of a patient who gave them up because

they caused her headaches, but this is the only instance I have come across in many hundreds of my patients. In the early stages of treatment certain effects may arise in the septic focus responsible for the general condition, a purulent discharge may increase in amount, or may appear where absent before; a chronic appendix may become acute, septic teeth may begin to ache and give evidence of concealed trouble. In three cases under treatment an attack of erythema—almost erysipelas—appeared. In all of these there was marked dental sepsis. Apparently, at times, the defensive mechanism at the septic focus has been physiologically stirred up.

The septic foci believed to be the cause of the calcium deficiency have in various diseases been found in a chronic ear discharge, the tonsils, the nasal accessory sinuses, a chronic appendicitis, the gall-bladder, probably the colon as evidenced by colitis, possibly chronic constipation, possibly the uterus, but most of all in the teeth. Here ulceration of the gums—pyorrhœa—has often been the cause, since removal of the teeth has materially changed the condition for the better. A red œdematous line leading from the gum margin to the apex of the tooth is an infallible indication for its removal. From personal observation in all these cases the apex of the tooth has shown erosion. In lesser instances of pyorrhœa I have found no better local treatment than rubbing the gums with dry salt. The pain and bleeding gradually lessen, but it has the very real disadvantage of being too cheap and common. Generally speaking, in an open discharge, e.g., from an ulcer or superficial pyorrhœa, or from the ear or tonsils, the blood quickly becomes normal under parathyroid treatment, while in a closed situation, such as the gall-bladder, an appendix, and often nasal sinusitis, and an abscess at the root of a tooth, the blood condition improves under parathyroid, but may take weeks to become normal, with even occasional relapses.

There is strong evidence, therefore, that calcium deficiency is an index of the absorption of a toxin, and that many chronic diseases, some of which have been mentioned, are due to the breakdown of the defences of the body in controlling this mechanism, which is apparently situated in the parathyroid glands. In some diseases at least, the site of the resulting lesion depends on the special idiosyncrasy of the individual, but it is just possible that in others a second unknown factor may guide the toxin into its own particular channel.

Parathyroid has shown a definite remedial action in all the chronic diseases accompanied by a calcium deficiency, which is simultaneously abolished or brought to normal.

Yet parathyroid is not a universal "cure-all," in fact, with our present knowledge, it is not a cure for any disease. On the other hand, it is a physiological aid to the healing of chronic lesions due to toxin absorption: if the focus of this toxin be removed, parathyroid will then, and then only, bring about a cure. If the focus cannot be removed parathyroid is but a remedial agent, though a valuable one.

Whether complications of acute disease will also show improvement from the administration of parathyroid is a subject for longer and more extensive trial.

Of the older uses of parathyroid in tetany, I have no experience. I have found it no help in paralysis agitans, but the cases were well advanced. I can only confirm the observation that it has a controlling influence in epilepsy.

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Dr. H. VINES

said that in all the various diseases improved by parathyroid therapy the underlying condition was sepsis. In some cases in which the white blood count had been low, the full polymorphonuclear leucocyte count had risen to 18,000 in two or three weeks. In many cases increased purulent discharge had followed parathyroid therapy. He believed the action to be a stimulation of leucocytes, probably indirectly through the action of calcium on blood plasma. Broadly speaking, he believed that the use of parathyroids increased physical resistance to bacterial disease.

Professor W. E. DIXON, F.R.S.

(who was asked by the PRESIDENT to describe recent experimental evidence of the action of ovarian extract) said that Harvey Cushing had observed that pituitary extract was secreted into the cerebro-spinal fluid. In order to determine whether pituitary secretion was present in the cerebro-spinal fluid, he (Professor Dixon) had put a cannula into the spinal canal in dogs and cats and had tested the fluid by its action on uterine muscle. He had succeeded in finding pituitary extract present in nearly every specimen, though different samples contained different amounts. In attempting to ascertain what substance or substances could increase the pituitary secretion, he had found that the one substance which invariably produced an immediate increase in pituitary secretion was ovarian extract boiled, filtered, and injected into a vein. The active substance was not present in the corpus luteum, nor was orchitic extract efficacious. This stimulating action of ovarian extract appeared to offer an explanation of the normal action of the pituitary body on uterine muscle. It did not explain the intestinal action of pituitary extract. However, it was found that extracts of duodenum also produced increased pituitary secretion, though not immediately. The increase was smaller and more prolonged. Pituitary extract increased the tone of the whole of the small intestine, and wide relaxation of the large intestine was associated with this, the reason being unknown.

Dr. W. LANGDON BROWN (President)

said that the discussion had been most interesting, none the less so because Professor Swale Vincent had opened it with a refreshing dose of scepticism. It was quite time that they should be reminded of the need for studying the underlying physiological basis for all this work, and should take a little more careful stock of the evidence for and against any particular diseases being associated with endocrine defects and of their possibilities of remedy by organotherapeutic means. Organotherapy was lagging some way behind the observations made in endocrinology. It must be admitted that the basis upon which the clinical observations had been made was in many instances not as satisfactory as could be wished. At the same time the observations which Dr. Grove and Dr. Vines had brought forward showed that evidence was available by clinical means, apart from ordinary physiological laboratory experiments, which would meet all the tests that could be asked of it. The observations mentioned by Professor Dixon were very important as showing that there was something in the ovary which definitely stimulated the pituitary gland. Therefore he did not think one should take up so negative a position

24 Langdon Brown: *Present Position of Organotherapy*

as Professor Swale Vincent had done. In the glycosuria of pregnancy the blood-sugar curve had been demonstrated by Mackenzie Wallis to be exactly the same type of curve as the one given in hyper-pituitarism, suggesting again some direct influence of the gonads on the pituitary. He mentioned two cases in which he had used ovarian extract, and in which the evidence pointed to a definite effect.

A woman, aged 20, had an artificial climacteric induced by the removal of both ovaries and tubes for tuberculous disease. She began to put on weight, which was not unexpected, and she had also curious seizures, attacks of choking, with quickening of pulse and respiration, each attack lasting about six minutes, and resembling some kind of minor epilepsy. Her thyroid swelled up during the attack, and she passed a little sugar after the attack. She was given ovarian extract, and whereas she had previously had three or four attacks a week she had had only one attack since last Easter. This patient was observed under hospital conditions; she did not know what she was taking, and no attempt was made to suggest that it was going to have a remarkable action.

The other case was one of thymic asthma. A number of remedies had been tried, and the attacks continued to be quite frequent. After ovarian extract was given they became less frequent, and were now rare.

It would appear that here ovarian extract had some therapeutic effect, but he (the President) himself had approached this question as a sceptic, and he still recognized that it was necessary to look to the basis of the clinical observations very thoroughly.

Section of Therapeutics and Pharmacology.

President—Dr. W. LANGDON BROWN.

DISCUSSION ON THE ACTION OF QUINIDINE IN CASES OF CARDIAC DISEASE.

Professor FRANCIS R. FRASER.

SEVERAL hundreds of cases have been recorded of auricular fibrillation in which normal rhythm has been restored, and we can accept it as demonstrated that quinidine can bring about this change of mechanism in a large proportion of all cases. Our knowledge of the pharmacology of quinidine has been greatly extended by the recent work of Lewis and his colleagues [1], but our knowledge of the therapeutics is small, probably because of lack of ability to ascertain the causes of auricular fibrillation in patients.

Like all powerful drugs quinidine can bring about effects that are unpleasant or even dangerous, and so, necessarily, we should learn in what cases the conversion can be effected without producing such results. The advantages to be gained might conceivably be so great that the risk of unpleasant symptoms is not a serious deterrent to giving the drug, but in digitalis we have so successful a method of treating the effects of auricular fibrillation that we must seriously consider in what cases a successful quinidine result surpasses the effect of digitalis treatment in therapeutic value. To maintain the normal rhythm it may prove in some cases to be necessary to continue the administration for a long time or even indefinitely. This may produce greater comfort or efficiency than the continuous administration of digitalis.

If quinidine treatment is to find a place in general therapeutics, answers to the two following questions must be obtained:—

(a) In what cases can the conversion be brought about without risk of producing dangerous symptoms?

(b) In what cases does the therapeutic effect of this conversion surpass that of digitalis treatment?

A discussion here may well help us to answer two further questions:—

(c) What is the best method of administration, especially in regard to dosage?

(d) For how long after the conversion should the quinidine treatment be continued?

In this country Hay [2] advocates 2 grm. a day in ten doses of 0.2 grm. each, while Parkinson and Nicholl [3] and Clark-Kennedy [4] give it three times a day or at six-hourly intervals. Similar differences in practice are recorded in the foreign literature.

Many other questions must arise, but preliminary answers to these four are

desirable before the necessary amount of material will be collected on which sound therapeutics can be based.

The effect of quinidine on disturbances of the mechanism of the heart-beat other than auricular fibrillation and flutter will, I have no doubt, be referred to by other speakers, but my experience with them has been too small to justify any reference from me. Many interesting electro-cardiographic phenomena have been observed as a result of quinidine treatment, but this is probably not a suitable occasion for their discussion in detail.

I shall first describe four cases in which the administration of quinidine resulted in dangerous symptoms, or in symptoms so distressing that the administration had to be stopped before any beneficial effects were produced.

(1) Among the earlier cases in which I tried quinidine was that of a woman, aged 54, with mitral stenosis and regurgitation, who was readmitted in June, 1921, after being under observation and treatment for a year with auricular fibrillation. When first seen in July, 1920, she had auricular flutter, and this had changed to fibrillation as the result of digitalis treatment. No history of rheumatic disease was obtained. The heart was greatly enlarged both to right and left, and the apex was in the sixth space, 5 in. from the mid-line. Under digitalis treatment she was capable of only very light work in the house. She was put on quinidine sulphate 5 gr. at 9 a.m. and 7.30 p.m., and reverted in twenty-four hours to flutter with an auricular rate of 196. After eight days her pulse-rate, which had continued between 80 and 90, suddenly rose to 200, and the patient felt faint. Probably a 1 : 1 response occurred. The treatment was stopped, digitalis was recommenced and fibrillation returned.

(2) A woman, aged 31, with a history of rheumatic fever at 8 years of age and a "weak heart" since, and with mitral stenosis, had auricular fibrillation with a ventricular rate of only 50-65, and severe heart failure while at complete rest in bed. Presumably some lesion of the auriculo-ventricular conducting structures was present. On quinidine sulphate, 5 gr. three times a day, flutter resulted after five days, with an auricular rate of 186 and a ventricular rate of 98. The dose was raised to 7½ gr. three times a day, and six days later, when the auricular rate was 225, with a response of 2 : 1 and 4 : 1, a Stokes-Adams attack occurred. The patient was being electro-cardiographed at the time, and a tracing was obtained showing a ventricular standstill while the auricular activity continued. Auricular fibrillation returned on stopping the quinidine, and the patient died a year later with malignant endocarditis and tricuspid and mitral stenosis.

(3) A woman, aged 58, with mitral regurgitation and auricular fibrillation, probably of eighteen months' duration, had no history of rheumatism, and had evidence of arterial and renal disease. She had a ventricular rate on admission of 180, and the heart was enlarged to the right and left, with the apex in the fifth space 5½ in. from the mid-line. The response to digitalis treatment was good, but at a ventricular rate of 90 numerous premature beats occurred. After four doses of 15 gr. of quinidine sulphate at six-hourly intervals, normal mechanism was restored, but the patient experienced much precordial discomfort and nausea. The quinidine was stopped, and fibrillation returned a day later with relief from the subjective distress.

After the experience of these cases, I selected with care, avoiding all cases with evidence of much damage to cardiac structures, such as great enlargement, heart block, or severe valvular disease. Similar conclusions have already been recorded in the literature.

(4) Recently I met with another limitation to the administration. A woman, aged 48, with mitral stenosis, had auricular fibrillation, possibly of only a few weeks' duration. It was this short duration that induced me to try it in a case that seemed otherwise unsuitable. No causal infection could be determined. On admission she had severe

abdominal pain with dyspnoea, and orthopnoea, and numerous râles at the bases of the lungs. The apex beat was in the fifth space $5\frac{1}{2}$ in. from the mid-line. The response to digitalis was good. Quinidine sulphate 5 gr. three times a day had no effect, and after four days it was raised to $7\frac{1}{2}$ gr. three times a day. On this dose the ventricular rate rose steadily, and on the fifth day, after 175 gr. in all, she complained of headache, vomited, and the temperature rose to 100° F. Next day there was a rash on the abdomen, lower chest, above the back of the elbows, and above the knees on the anterior aspect of the thighs. It consisted of small rose-red papules, raised in the centre and fading into the surrounding skin at the margins. They varied in size from one-twentieth to one-fifth of an inch in diameter, and were in places confluent. Next day the eruption had a morbilliform appearance, and had spread to the back in the lumbar region, on to the upper chest, neck and lower part of face. The arms were affected more on the extensor than on the flexor surfaces. It did not spread below the knees or on to the hands. A few of the lesions were purpuric. The quinidine was stopped on the first appearance of the rash, which commenced to fade on the third day, but did not entirely disappear till the seventh day.

In the following cases there was little evidence of extensive permanent changes in the cardiac structures and the conversion to normal rhythm resulted without any discomfort to the patient.

(5) A man, aged 39, was admitted in July, 1921, with auricular fibrillation of probably five months' duration. He had syphilitic aortitis. No valvular lesion was discoverable, but the apex beat was in the sixth space $5\frac{1}{2}$ in. from the mid-line. Three months previously when first seen he had severe heart failure at rest in bed, but he had responded well to digitalis and had been put on antisyphilitic treatment. The Wassermann reaction became negative, but he had much precordial distress and palpitations, and there were numerous ventricular extrasystoles. Quinidine sulphate 5 gr. three times a day was increased to 10 gr. three times a day after six days, and four days later, after 180 gr., normal rhythm was restored. Three days later the dosage was reduced to 5 gr. three times a day, and this was continued for three months while he was attending as an out-patient. A year and a half later the rhythm is still normal, and he states that he feels fit to return to his work as a stevedore. There are still occasional ventricular extrasystoles.

I would suggest that in this case the treatment of the cause of the heart disease was an important factor in the success.

(6) A man, aged 26, was admitted in July, 1921, with dyspnoea, œdema and auricular fibrillation which had probably been present for six weeks only. No clear evidence of valvular disease was obtained and no evidence of any infection other than malaria five years before with recurrent attacks up to three months before admission. He responded well to digitalis and there was but little enlargement of the heart. Quinidine sulphate 5 gr. three times a day was increased to 10 gr. three times a day after twenty-four hours and on the third day, after 55 gr., normal rhythm was restored. The dose was reduced to 5 gr. three times a day two days later, and this was continued for ten days more. The normal rhythm has continued ever since, and he is now working as a labourer, but has occasional precordial distress.

In this case it appears probable that the cause of the auricular fibrillation, whatever it was, had ceased to be active when the patient came under observation.

The auricular fibrillation associated with exophthalmic goitre or other thyro-toxic conditions is of peculiar interest, since the thyro-toxic condition can be treated, and the next three cases are examples of the action of quinidine in different stages of thyro-toxic activity.

(7) A man, aged 40, with exophthalmic goitre had had auricular fibrillation probably for five years. A partial thyroidectomy was performed in October, 1920, in

Australia without relief and the signs of heart failure were pronounced unless the patient remained at complete rest in bed. He responded well to digitalis at the National Heart Hospital under the care of Dr. Strickland Goodall. A further thyroidectomy was performed by Mr. T. P. Dunhill, in October, 1922, at St. Bartholomew's Hospital, with great improvement in the thyro-toxic symptoms, but the auricular fibrillation was still present three weeks later. He was put on quinidine sulphate 5 gr. three times a day, and after four doses the rhythm became normal. There was no evidence of valvular disease, and the apex beat was in the fourth space $4\frac{1}{2}$ in. from the mid-line. The quinidine was continued in diminishing dosage for a further ten days. The rhythm is still normal and the patient is steadily increasing his activities.

The removal of the toxic thyroid gland probably removed the cause of the fibrillation, and the quinidine was of value in disturbing a mechanism that tends to be stable when once developed.

(8) A woman, aged 45, had exophthalmic goitre seven years ago. After various medical treatments the general thyro-toxic condition gradually subsided, but without improvement in the palpitations, œdema and shortness of breath. It is probable that auricular fibrillation had been present for some years. On admission in July, 1922, she had œdema of the legs, back, and bases of the lungs, and the apex beat was in the fifth space $4\frac{1}{2}$ in. from the mid-line. The response to digitalis was good and she was discharged a month later. In October, 1922, she was readmitted to try the effect of quinidine, and it was found that the thyro-toxic symptoms were rather more pronounced than in the summer. After six doses of quinidine sulphate $7\frac{1}{2}$ gr. three times a day, the rhythm became normal. The quinidine was gradually reduced and discontinued four weeks later, and when the patient was discharged the apex beat was in the fourth space $3\frac{1}{2}$ in. from the mid-line. While at home she developed a bad cough and auricular fibrillation returned. On readmission in February, 1923, a great increase in the thyro-toxic symptoms was found to be present, and a partial thyroidectomy was performed on February 22. As she was still fibrillating on March 1, quinidine sulphate 5 gr. three times a day was commenced and normal rhythm restored after four doses. She is still on quinidine, and a further thyroidectomy may be necessary before all thyro-toxic symptoms disappear and before a stable normal rhythm is obtained.

(9) A woman, aged 46, with large multiple cysts of the thyroid gland of many years' duration, had severe heart failure and auricular fibrillation for four weeks. There were thyro-toxic symptoms and the basal metabolic rate was from + 35 per cent. to + 50 per cent. The response to digitalis was good, but the auriculo-ventricular rhythm that resulted from quinidine was unstable, nodal rhythm, auricular extrasystoles and ventricular extrasystoles interfering with the regularity. During a bad attack of quinsy with purpura the thyro-toxic symptoms increased and fibrillation returned in spite of the continued quinidine administration. The quinidine was stopped, and after the subsidence of the acute infection its administration was recommenced and normal rhythm was again established. This persisted through the strain of tonsillectomy and the removal of all her teeth, which were very septic, and is still present two months later. She declines to have the thyroid gland operated upon and continues to take quinidine 5 gr. three times a day.

These two cases suggest that so long as the cause of the auricular fibrillation remains active, the normal rhythm resulting from quinidine treatment will not be stable, and in the next case this suggestion is supported by the result of treatment in a patient suffering from rheumatic infection of the heart.

(10) A woman, aged 26, with mitral stenosis and auricular fibrillation gave a history of rheumatic fever four years previously, and of a probable onset of the fibrillation two months previously. There was slight œdema of the ankles, severe dyspnoea on exertion, palpitations on exertion and frequently also when at rest. The apex beat was in the fifth space $3\frac{1}{2}$ in. from the mid-line. The response to digitalis

was good. Quinidine sulphate 5 gr. three times a day caused a return to normal rhythm after seven doses, with cessation of the palpitations and precordial distress that had persisted on digitalis, but the dyspnoea was still severe. After thirteen days the dose was reduced to $2\frac{1}{2}$ gr. three times a day, and she was discharged on this amount. A month later an attempt was made to relieve the dyspnoea by adding tincture of digitalis 5 minims, and later 10 minims, three times a day, but this was without effect on the symptoms and did not disturb the rhythm. Three months after leaving hospital she had a bad cold and fibrillation returned for a few days, and a month later pains in the knees, pains over the precordium and attacks of palpitation occurred with a return of auricular fibrillation. The quinidine was stopped and sodium salicylate and digitalis were commenced. When the attack, probably one of rheumatic infection, had subsided, quinidine again restored normal rhythm. During the last nine months she has continued to take quinidine 5 gr. three times a day, but has fibrillated for a few days on three definite occasions. On one of these she had much worry in connexion with the death of her mother, on another she had tried to do some heavy work, and on the third she had a bad cold.

It seems probable that the rheumatic infection is continuously or intermittently active and that the quinidine is just sufficient to prevent fibrillation except during a relapse, when another infection is present, when worry or mental strain occur, or with physical strain. It is difficult to decide that she has a raised efficiency when the rhythm is normal, but she is certainly far more comfortable and contented.

I have seen one case of thyro-toxic auricular fibrillation in which normal rhythm was restored a few days after partial thyroidectomy, without other interference, and have heard of several others; and I have seen cases in which auricular fibrillation occurred only for a few hours after thyroidectomy during the stage when excessive excitement is seen. These cases suggest that the auricular fibrillation is due to an active toxic action, and that normal rhythm may result simply from the cessation of this toxic action. The difficulty in obtaining a stable normal rhythm with quinidine in active toxic cases, and the stability obtained after partial thyroidectomy, indicate clearly that in this type of case in order to obtain a stable normal rhythm the thyro-toxic factor must be treated.

In the other types of cases the indications of a similar active agent are not so complete, but the accumulation of more evidence might well decide in favour of a similar state of affairs in them also. The stability of the syphilitic case in which anti-syphilitic treatment was given supports this view. The stability in the case in which no definite valvular lesion could be found, and in which the only known infection was malaria, may well be due to the cessation of the causal factor, whatever it was, before the patient came under observation.

The instability of the rheumatic case when quinidine treatment was continued for nine months is suggestive of a constantly or intermittently active factor tending to produce fibrillation. This idea is fully in keeping with the theory derived from the clinical study of these cases, that the rheumatic infection continues for years, causing gradually increasing stenosis of the mitral valve and death, often without well defined attacks of acute rheumatic infection. In this case it is of interest to note the additional factors present during the relapses into auricular fibrillation, namely worry and mental strain, physical strain, and infection of the upper respiratory tract. Unfortunately we have no successful method of treating the rheumatic infection of the heart.

In addition to active agents, toxic and infectious, it is probable that auricular fibrillation can result from permanent structural changes in the heart, in which case a stable normal rhythm after cessation of the quinidine treatment would be unlikely.

I would, therefore, conclude that:—

(1) Quinidine treatment will result in a stable normal rhythm if the cause of the auricular fibrillation can be successfully treated or has ceased to be active.

(2) Cases in which there are extensive structural changes in the heart, as evidenced by heart block, severe valvular disease or great enlargement, are not suitable for quinidine treatment, especially if the response to digitalis is not good. In such cases unpleasant and even dangerous disturbances may result.

(3) In some cases at least, the comfort and efficiency of the patient are greater when a normal rhythm results from quinidine treatment, than when digitalis treatment alone is used.

(4) In the cases in which the results are satisfactory larger doses than 10 gr. of quinidine sulphate at intervals of six hours are not necessary, nor smaller initial doses than 5 gr. three times a day.

(5) If necessary to maintain normal rhythm quinidine may be continued to be administered indefinitely, and it is probably advisable in all cases to continue it for some weeks after normal rhythm has resulted, though in gradually decreasing doses.

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Dr. A. N. DRURY.

Professor Fraser has brought out very many clinical points with regard to the treatment of auricular disease with quinidine. I think that the purpose of this discussion will be better fulfilled if I leave this aspect alone, and speak only of the toxicology of the drug, and outline especially the effect upon the cardiac musculature.

Before entering upon this, I must mention in passing certain symptoms, not peculiar to patients who have cardiac disease, but common to all who are under quinidine therapy.

The first is the sudden collapse, with unconsciousness and failure of respiration, which occurs after a small amount of the drug has been taken. This idiosyncrasy, which is very alarming, is fortunately not common and is tested by giving a small dose of the drug before treatment. The unpleasant symptoms of quinidine poisoning, such as headache, nausea, diarrhoea, and vomiting, which also occur, are not unduly common during quinidine administration, but must be expected to appear in certain cases; the possibility of their occurrence must always be borne in mind.

With regard to the effects which are produced in the cardiac musculature when quinidine is administered. The information, which has been obtained by experiments upon animals, where doses comparable to those employed clinically have been used, has been fully borne out by clinical findings; and it can be safely assumed that similar changes are produced in the human cardiac musculature. A study of these changes enables us to understand more clearly the cause of the restoration of the normal rhythm and of its failure; the untoward events which occur during quinidine administration and how to guard against poisoning the heart to a dangerous degree. In short, quinidine lengthens the refractory

period, and slows the conduction of the excitation wave from point to point in the muscle. These effects are witnessed in both auricular and ventricular muscle, and occur also in the auriculo-ventricular node and bundle; it also paralyses the vagus nerve to a certain degree. There is now ample proof, I think, that in both the conditions of auricular fibrillation and flutter, there is a continuously circulating wave present in the auricle. In flutter the wave follows a regular course; in fibrillation an irregular one. Now the maintenance of this circulating wave is dependent upon the balance of three factors. The length of the path taken by the circulating wave, the time taken for the muscle to become responsive after it has contracted—the refractory period; and the speed at which the wave travels from point to point—the conduction rate. Now the important part of such a circulating movement is the gap which exists between the head of the oncoming wave and the wake of the receding wave. A gap of some degree is essential, if the wave is to maintain its movement. If the gap can, by any means, be closed up, the on-coming wave will find refractory tissue ahead of it and must die out. The three events, any one of which may close this gap, are a shortening of the path, a quicker rate of conduction, or an increase in the duration of the refractory period. The first event need not be considered, as the original path is always open. Quickening of the rate of conduction does not occur under quinidine but the reverse happens. The refractory period, however, is lengthened, and this would adequately explain why the circus movement is stopped, for the oncoming wave finds refractory tissue ahead of it, the gap is closed. But quinidine does not always stop this circus movement and this is anticipated when it is appreciated that the conduction rate is being simultaneously slowed; lengthening of the refractory period closes the gap, the slowed conduction opens it. It is only when the first factor exceeds the second that the gap will be closed. There is also another reason why the circulating wave should not always cease. Closure of the original gap may cause the circulating movement to select a longer path; a further increase in refractory period may now prove effective or not, it depends upon the largest available path. We can now anticipate the events likely to occur in the auricle during the treatment of fibrillation or flutter. Partly on account of slowed conduction, and partly on account of the closure of the gap forcing the movement on to a longer path, the auricular movements will become slower and in one case there will be an abrupt termination of the disorder, in another a progressive slowing of auricular movement only. This slowing of auricular movement plays an important part in the production of the palpitation which is seen usually at the commencement of treatment, and occasionally subsequently. If an auricular rate of beating is above 200 per minute, any increase in its rate is accompanied by a slower ventricular rate. This is exemplified in the two conditions, flutter and fibrillation. All other things being equal, the ventricular rate associated with the former, where the auricular movement is 300 per minute, is higher than the latter where the movement is about 450 per minute. For this reason alone if the auricular movements of auricular fibrillation which range around 450 per minute are slowed, the ventricular rate of beating will rise, and if the rate of movement is forced to about 200 or lower there is danger that the ventricle may suddenly respond at the same rate. The partial paralysis of the vagi will also diminish any degree of block which existed between auricle and ventricle in respect of vagal tone and allow a higher ventricular rate. On the other hand, there is a direct action of the drug upon the conducting tissue, for in many cases which are restored to normal rhythm

the *P-R* interval is longer for the first twenty-four hours, and generally returns to a normal figure when the drug has been completely excreted ; and this will help to maintain a low ventricular rate by increasing the block between auricle and ventricle. It would appear that the slowing of auricular movement is a very early event, for on the first two days high ventricular rates are common : later the conduction between auricle and ventricle is depressed, and the remainder of the treatment is often not interrupted by such high rates, except in those cases where a very low auricular rate of 200 brings out a full ventricular response. These attacks of palpitation are usually not sufficient to hinder the treatment, but may, on occasions, force a case which is on the borderline into failure. They can be combated by giving a preliminary course of digitalis with the idea of interposing a block between auricle and ventricle.

There is also another type of palpitation the recognition of which is important. Studied electro-cardiographically, it commences as an occasional ventricular extrasystole interposed between normal ventricular complexes. There may be coupling, as is seen in digitalis poisoning ; later, the extrasystoles occur in pairs, or short runs, and finally a run of ventricular tachycardia about 160 per minute may occur and last for minutes or hours. The appearance of such a tachycardia warns us that a degree of poisoning is present which is dangerous : such tachycardias arise easily in ventricular muscle when there is a lengthened refractory period and a slowed conduction rate, and are always associated with low rates of auricular movement, about 250-300 per minute. They are the precursors of ventricular fibrillation and are an indication for withdrawal of the drug. As I have outlined above, quinidine exerts a profound effect upon the cardiac musculature, and it is apparent that it is not a drug to be used haphazard and without thought. Where it is used the conditions should be strictly controlled. It is, at the moment, a treatment for the wards rather than for the out-patient department. But, as one thinks of the patients who have gained, according to their own evidence, a new lease of life, owing to the treatment, one does not feel that it is justified to withhold the treatment because of its limitations, disadvantages and dangers. Judged by results it is seen to be eminently suitable for certain cases, and the success of the drug will be in the selection of the suitable cases ; and understanding of the effects which are produced in the musculature of the heart during the administration must help greatly in the correct selection of those cases.

Dr. A. E. CLARK-KENNEDY.¹

(I) *Introduction.*

In the large majority of cases, the onset of auricular fibrillation is associated with a sudden increase in the signs and symptoms of cardiac decompensation. It is, therefore, generally accepted that this condition is one of the factors in the pathogenesis of heart failure.

We now have two methods of treating auricular fibrillation. By giving digitalis the conductivity of the auriculo-ventricular bundle can be depressed. The ventricular rate is thereby artificially controlled, but the auricles are left to fibrillate. Or, by giving quinidine, in the majority of cases, normal rhythm can be restored. On theoretical grounds alone, the restoration of sino-

¹ A more detailed account of the observations on which this contribution to the Discussion is based is published in the *Quarterly Journal of Medicine*, April, 1923, p. 201.

auricular rhythm with quinidine should be a more successful therapeutic procedure than the artificial control of ventricular rate with digitalis.

Commencing in August, 1921, I have now investigated the action of quinidine in forty-five cases of auricular fibrillation. In thirty-seven (82 per cent.) I have seen normal rhythm restored. In twenty-nine of these normal rhythm persisted after the patient was discharged from hospital, but in eight relapse occurred within a few days.

An arbitrary age limit of 65 has been adopted and patients in whom œdema persisted in spite of prolonged digitalis treatment have not been treated with quinidine. Apart from these considerations no selection of cases whatever has been exercised. All cases admitted in rotation to the Wards of the Medical Unit at the London Hospital, and to a London Poor Law infirmary, have been treated with quinidine. A thorough initial course of digitalis was administered to all these. This was then discontinued and quinidine given on an arbitrary but standard system of dosage: 5 gr. six-hourly was given for three days, and then the dosage of the drug increased 4 gr. per diem, i.e., the patients received 6 gr. six-hourly on the fourth day, 7 gr. six-hourly on the fifth day and so on. When normal rhythm had been restored the dosage was gradually decreased to 5 gr. twice daily, which was then continued indefinitely as a prophylactic against relapse. In all cases quinidine was pushed until symptoms of failure or severe toxic symptoms supervened.

The maximum amount of quinidine which was found necessary to effect the restoration of normal rhythm was 392 gr. with a maximum dosage of 15 gr. six-hourly. In another case a similar amount failed to produce a successful result. But the majority of cases which have reacted to quinidine have reacted to small doses, and in practice it will probably be advisable to limit the maximum dosage on the standard system described to 9 gr. six-hourly or thereabouts.

(II) *The Ill-effects of Quinidine Administration.*

These are numerous, and may be best classified as follows:—

(1) *The General Symptoms of Quinidine Intoxication.*—Headache, nausea, vomiting, diarrhoea and abdominal pain have been common but frequently pass off in the early stages without reduction in dosage of the drug. Excessive vomiting, in one instance associated with dangerous collapse, only occurred in two cases, but in both normal rhythm was only transiently restored. A mild degree of pyrexia coincided with the maximum of quinidine administration in ten cases. All these patients were relatively young and nine were definitely rheumatic. Whether this is due to a direct action of the drug on the heat-regulating centre or to the lighting up of a low grade infection is uncertain. In one case the patient developed glycosuria twenty-one days after quinidine had been discontinued. Two cases developed papular scarlatiniform rashes, one urticaria, and a third an eruption closely resembling that of measles. Visual symptoms and attacks of faintness were fairly frequent during the maximum of quinidine administration and were almost invariably associated with the fall in blood pressure that the drug is known to produce at this stage. No retinal changes were observed.

The large majority of patients have tolerated the prophylactic administration of the drug over a year or more without symptoms.

(2) *Embolism, consequent on the Restoration of Auricular Function.*—This occurred shortly after the restoration of normal rhythm in four cases. In one of these the embolus was pulmonary and the patient died suddenly. Autopsy,

however, showed that it had been derived from thrombosis in the iliac vein and *not* from clots in the auricular appendices. In our very first case simultaneous infarction of the lung and kidney and embolic blocking of a popliteal artery occurred. The patient, however, recovered completely, normal rhythm has persisted for over a year and a half and she remains practically free from cardiac symptoms. In the other two cases the embolus was splenic. No doubt I have been fortunate in that no case of cerebral embolism has been seen. Excluding the fatal case, which is irrelevant to the present discussion, the other three patients all had mitral stenosis and auricular fibrillation of eighteen to thirty months' duration.

It is not surprising that the auricle, when it first starts to contract after several months of fibrillation, should expel thrombi into the pulmonary or systemic circulations. But the incidence of embolism in the early cases of my series was particularly high, and my more recent cases and the results of other workers would tend to show that the risk of embolism is not as great as Dr. Ellis and myself first supposed. Possibly indeed little greater than under digitalis treatment alone.

(3) *The Symptoms due to the Toxic Action of the Drug on the Myocardium.*—In all the unsuccessful cases, to which large doses of quinidine were administered, symptoms of cardiac decompensation were produced. No fatality occurred under such circumstances, and these symptoms quickly passed off when the drug was discontinued. In all these cases quinidine treatment had to be abandoned on this account rather than on account of the general toxic effects produced. The patient in one case, however, a female with rheumatic heart disease, a large heart, mitral stenosis and chronic high blood-pressure, received 180 gr. of quinidine with a maximum dosage of 9 gr. six-hourly. This failed to restore normal rhythm and the drug had to be discontinued on account of symptoms of failure. A long course of digitalis now caused coupled ventricular beats. Digitalis was now stopped and quinidine immediately re-administered. The patient died suddenly after the second 5-gr. dose. Post-mortem examination failed to reveal the cause of sudden death.

To give quinidine, immediately after digitalis had produced coupling, was certainly a therapeutic mistake on my part. But it seems to me, that in this case death should be attributed at least as much if not more to digitalis than to quinidine. This is the only fatality that I have to report in my series of forty-five cases. One other patient, however, a woman aged 45, with pulmonary stenosis and a large heart, had a sudden syncopal attack in which she nearly died.

The pathogenesis of death in these fatal cases has received various explanations, but autopsy, as might be expected—save to exclude embolism—has yielded no information. Heart block, ventricular fibrillation, and failure of the sino-auricular node to begin initiating stimuli, when fibrillation ceases, have all been suggested.

(4) *Symptoms of a Cerebral Nature.*—I have occasionally observed these. Three of my patients suddenly felt confused, seemed to lose themselves and then shrieked out. One patient, according to the nurse's account, suddenly became unconscious, rolled her eyes and frothed at the mouth. A little later she became extremely restless, she shrieked out wildly and morphia had to be administered. These attacks occurred either shortly after the restoration of normal rhythm or during the maximum of unsuccessful quinidine administration. I am inclined to attribute them to transient interference with the blood

supply to the brain rather than to embolism or the direct action of the drug on the cerebrum.

Von Frey has described the occurrence of respiratory paralysis during the administration of quite small doses of the drug. No such case has come under my observation.

(III) The Restoration of Normal Rhythm as a Therapeutic Procedure.

Of the cases in which normal rhythm persisted after discharge from hospital, fourteen had received no digitalis treatment before admission. These cases were admitted to hospital with failing compensation, but auricular fibrillation had been of relatively short duration. In all these cases digitalis and rest in bed at least partly restored compensation, caused œdema to disappear, and reduced the pulse-rate below 80.

All these cases, with one exception, remained as well up and about after the restoration of normal rhythm as they had been when resting in bed with fibrillating auricles, and the corresponding ventricular rates controlled with digitalis.

In three out of these thirteen cases, objective signs of heart failure became less pronounced when normal rhythm was first restored. But in the remaining ten, although they mostly experienced relief from subjective sensations in the chest—signs and symptoms of failure being absent before the restoration of normal rhythm—no evidence of rise in cardiac efficiency at that time could be adduced.

Of these thirteen patients ten have returned to work, and all with one exception had mitral stenosis. In these ten cases it has been concluded that cardiac efficiency had been restored to its original level before the onset of fibrillation, because the patients' capacity to do work without the onset of cardiac symptoms was now the same as before the known date of onset of fibrillation.

In the remaining three out of the thirteen cases, although compensation has been maintained, the exercise tolerance of these individuals has not returned to what it was before the known date of onset of fibrillation. These three patients had no mitral stenosis. One had an adherent pericardium and mitral incompetence, while the other two had no valvular lesions. These cases are probably cases of active myocardial disease.

In the one exception referred to, the restoration of normal rhythm failed to maintain compensation, and though sino-auricular rhythm persisted with a normal pulse-rate, œdema and signs and symptoms of failure appeared. Whether this patient would have been any better under digitalis, if fibrillation had been allowed to persist, is doubtful. It seems more likely that active myocardial disease was in progress.

(IV) The Relative Merits of Quinidine and Digitalis in the Treatment of Auricular Fibrillation.

Of the cases in which normal rhythm persisted after the patient was discharged from hospital, eleven were cases of auricular fibrillation of relatively long duration, which had received previous digitalis treatment. In this group of patients only is it possible to compare the relative values of digitalis and quinidine in the treatment of this condition.

Of these eleven cases, in seven the restoration of normal rhythm with quinidine maintained a higher level of cardiac efficiency than had previously

existed when the auricles were fibrillating, although the ventricular rate was controlled with digitalis. In all these cases there was mitral stenosis, but in all the aortic valve was intact. Of these seven cases, in five clinical improvement was striking, and the date of onset of auricular fibrillation being known it was considered that cardiac efficiency had been restored to its original level before the occurrence of this event. But in the other two cases, clinical improvement was not pronounced, and although they were definitely better than before quinidine treatment, cardiac efficiency had probably not been restored to its original level before the onset of fibrillation.

In two out of the eleven cases the restoration of normal rhythm with quinidine seemed to maintain about the same level of cardiac efficiency as digitalis. One of these patients had syphilitic aortitis and aortic incompetence, while the other had no valvular lesions. The latter patient felt as well after discharge from hospital with fibrillation as after his discharge the second time with normal rhythm. On the first occasion he stopped taking digitalis, and compensation failed within two months, but since the second occasion, normal rhythm with adequate compensation has persisted for over a year and a half, and he has returned to the heavy work of a carman.

Two cases out of the eleven were definitely worse as the result of restoration of normal rhythm. Signs and symptoms of failure previously absent now appeared. Cardiac efficiency in these two patients was now at a definitely lower level than when fibrillation was present, the ventricular rate being controlled with digitalis. In both these cases there was mitral stenosis, and both developed tachycardia with the return of normal rhythm. The association of simple tachycardia, mitral stenosis, and venous congestion is common. The explanation of the tachycardia is doubtful, but it seems fairly certain that it serves no useful purpose. When we restore normal rhythm in such cases, we return to the unsatisfactory condition in which we have no means of controlling the ventricular rate. In removing the embarrassing effects of disorder of rhythm we have substituted the worse handicap of tachycardia. Unfortunately the previous clinical histories of these two patients is not definitely known, but it seems probable that they belong to this particular group. In both cases quinidine was discontinued in the hope that relapse into fibrillation might occur. This has supervened in one case, and under digitalis the symptoms of failure have largely disappeared. In the other normal rhythm with decompensation has persisted in spite of heavy doses of digitalis.

(V) *The Prognosis and Tendency to Relapse in Successful Cases.*

All cases of this series, with a few exceptions, have continued to take quinidine regularly as a prophylactic against relapse.

Of the twenty-eight successful cases, in eleven relapse occurred after normal rhythm had persisted for periods of one year to one month. All these with one exception took quinidine regularly.

In the remaining seventeen cases normal rhythm has persisted up to date with a maximum duration of a year and a half. Three of these have not troubled about taking quinidine, but the remainder have continued the drug regularly.

Of the fifteen cases in which the patients were discharged from hospital with normal rhythm over a year ago, in nine normal rhythm is still persisting. On the whole, although there seems to be considerable evidence to show that discontinuing the drug frequently leads to relapse, normal rhythm may persist for long periods in certain cases without the patient taking quinidine.

(VI) The Indications for Quinidine Therapy.

Many cases of auricular fibrillation are unsuitable for quinidine treatment. The selection of cases may be considered under four headings, as follows:—

(1) That quinidine will restore normal rhythm in 70 to 80 per cent. of cases. A successful pharmacological action of the drug is to be expected in patients with auricular fibrillation of recent onset, whose hearts are not considerably enlarged, and who are middle aged rather than young.

(2) That granted a successful pharmacological action of the drug, a good therapeutic result is not necessarily to be anticipated. This is, however, to be expected in cases with auricular fibrillation of short duration and with rheumatic heart disease rather than general cardio-vascular degeneration. It may also be anticipated when the onset of auricular fibrillation has synchronized with the first appearance of cardiac symptoms, when marked subjective symptoms of irregular heart action are present, when mitral stenosis is present rather than aortic incompetence, and in cases of exophthalmic goitre when the acute stage of the disease has subsided naturally or after thyroidectomy.

(3) That cases in which normal rhythm has been restored tend to relapse into fibrillation. This is to be expected in cases of long standing fibrillation with large hearts in which active myocardial disease is in progress.

(4) That the administration of quinidine is attended with certain ill effects and even with considerable danger. In this connexion the following considerations must be borne in mind: (a) That the toxic effect on the system generally is approximately proportional to the dosage. (b) That the toxic effect of the drug in producing decompensation or sudden death is to be expected in patients with large hearts who have remained poorly compensated on digitalis therapy. (c) That embolism is to be expected in cases of mitral stenosis with long standing fibrillation and presumably in patients who have previously experienced embolic manifestations.

Therefore in determining whether a given case is suitable for quinidine treatment, we have first to estimate the probability of effecting the restoration of sino-auricular rhythm. This procedure in itself is not necessarily therapeutic. If a successful physiological action of the drug seems probable, the possibility of producing a good therapeutic result must next be considered. Against this we have to set the likelihood of speedy relapse and the possible dangers the patient incurs as the result of taking quinidine.

Summary.

My chief conclusions may therefore be summarized as follows:—

(1) That the value of restoration of normal rhythm must be judged, not by the immediate results obtained, but by the patient's efficiency when he has returned to his ordinary vocation.

(2) That in the absence of active myocardial disease, the restoration of normal rhythm with quinidine restores cardiac efficiency to its original level before the onset of auricular fibrillation.

(3) That in the majority of cases this procedure raises cardiac efficiency to a higher level than the digitalis treatment of the same condition. In about half these cases clinical improvement is striking but in the other half not pronounced. In a small minority of cases cardiac efficiency is definitely reduced.

(4) That the quinidine treatment of auricular fibrillation is contra-indicated during the acute stage of exophthalmic goitre, but that it may be instituted with benefit during the more chronic stages of the disease or after thyroidectomy.

(5) That in the successful cases, the prophylactic administration of quinidine should be continued indefinitely. If this is practised relapse does not necessarily occur over quite long periods.

(6) That the following system of procedure should be adopted: In the absence of the special contra-indications already discussed, quinidine should be administered in all cases of recent onset (one year or less). It should not be administered in cases of long-standing fibrillation (three years or more) who remain well compensated on digitalis. In cases of moderate duration (one to three years) and in doubtful cases, the therapeutic possibilities of the treatment and the risks incurred must be explained to the patient, who should be allowed to make his own decision.

(7) That the maximum dosage on the standard system described should be limited to 9 gr. six-hourly.

Finally, I believe that the therapeutic results, which are obtained by the careful administration of quinidine, justify the slight risks incurred in the treatment of selected cases.

Dr. T. F. COTTON.

My remarks are based on the experience which I have had during the past two years in the treatment of patients with quinidine outside the wards of a public hospital and on the results of treatment in the wards at University College Hospital, in the clinic of Sir Thomas Lewis.

My impression is that quinidine therapy in private practice is very much in the hands of the consultant. This, I think, is desirable in the present state of our knowledge. The general practitioner can and does recognize auricular fibrillation without the aid of the galvanometer or the polygraph. He knows from the many articles appearing in the medical journals that quinidine given by the mouth in cachets will restore a normal rhythm in a percentage of these cases. He has read of the contra-indications and the danger signals, and hesitates to undertake treatment on his own responsibility. He fears sudden death, the onset of failure and embolism. He seeks the advice of a consultant and is glad to entrust to him the management of the case. I think this is the experience of many of us in dealing with auricular fibrillation.

The main object of establishing a normal rhythm is to recover the ventricular rate which was present before the onset of auricular fibrillation. The ventricular rate is usually high when the auricles are fibrillating, and in consequence of this high rate an extra load is placed upon the myocardium already damaged by disease. This is one of the principal causes of heart failure of the congestive type. One can keep the ventricular rate within normal limits by giving digitalis. Quinidine obviates the necessity of continuous digitalis administration. It is no hardship, nor is it inconvenient for the patient with signs of failure, to take digitalis for the remainder of his life. Some patients of this class cannot tolerate digitalis in any form, and the ventricular rate remains high. One would like to give quinidine. I tried quinidine in one of these cases, but with the very high ventricular rate alarming signs of failure developed and I was obliged to discontinue its administration after two days.

In patients with considerable enlargement of the heart, it is possible to induce a normal rhythm. I have given quinidine in these cases not so much with the object of reducing the heart rate, for I could do this with digitalis, but with the hope of relieving distressing palpitation so common in these patients. I have combined digitalis and quinidine in these cases, as I feared the onset of failure from the high ventricular rate if quinidine were given alone. In all cases in which I have been able to restore a normal rhythm it has been of short duration.

In patients with slight or moderate enlargement and mitral stenosis, with no signs of failure or infection, I have never seen the development of symptoms that were in any way alarming, except in one case. A normal rhythm returned in about half of them; some reverted to fibrillation. In a few the normal rhythm was present when they were last seen.

The most suitable case for quinidine is the young person without enlargement of the heart and no valvular disease, who develops auricular fibrillation after an infection or from some other cause. He is fit in every way until the onset of auricular fibrillation. With the new rhythm established, physical exercise easily produces signs of distress. His exercise tolerance is poor; he is no longer able to continue with his occupation if it is in any way arduous, and games like tennis, which he enjoyed, he can no longer play. With the normal rhythm restored, he is able to resume his normal life.

Another type of case suitable for treatment is the patient with a history of paroxysmal fibrillation, and auricular fibrillation established, i.e., of a few months' duration; the chances of a return of the normal rhythm are better than 50 per cent. in this class.

In paroxysmal fibrillation the attacks are fewer and of shorter duration if quinidine is given in small doses over a period of months. I am not sure that this holds good in simple paroxysmal tachycardia, though I have seen considerable benefit in such cases.

I have not seen any good results from quinidine administration in patients with extrasystoles.

With a history of embolism I would not attempt to restore a normal rhythm. The risk is, I believe, too great, of dislodging a mural thrombus when the auricles have again begun to contract.

Briefly, these are my views concerning quinidine therapy. The indiscriminate use of this drug in auricular fibrillation is not justifiable. It should be given a trial in those cases of auricular fibrillation of recent origin presenting few signs of structural disease, i.e., little or no enlargement, no valve disease or early mitral stenosis. The failures and successes will be about equal in this group. A large majority of those with a normal rhythm will again be fibrillating within two years. If, after six months of normal rhythm, the auricles again fibrillate, quinidine may be expected to restore the normal rhythm a second time, the heart reacting to the drug very much as when first given. If the auricles fibrillate within a period of six months, I do not think much is to be gained by again restoring the normal rhythm.

In the treatment of paroxysmal tachycardia with the auricles contracting regularly or fibrillating, quinidine has an important place. In those cases with paroxysms of long duration and frequent occurrence, much may be expected from quinidine administration given in small doses and over a long period. Paroxysmal tachycardia is a serious disability where there are signs of structural disease present. The results from quinidine in these cases are quite good.

I am not sure that it is not an advantage to continue with the quinidine after the normal rhythm has returned, with a view to preventing the recurrence of fibrillation. In paroxysmal fibrillation and simple paroxysmal tachycardia, I give quinidine in small doses, 0.2 gm. three or four times daily for months. This may be good practice in all cases: I have certainly not seen any harmful results from quinidine given in this way.

I have had one case of sudden death after 2.0 gm. of quinidine had been given in the usual way. The patient was a woman aged 50 with moderate enlargement, no valve disease, and a ventricular rate of 140-150 after slight effort. There were no signs of venous engorgement and no breathlessness at rest. The nurse had left her reading a book and quite comfortable; when the nurse returned a quarter of an hour later, she found her dead, lying in the same position. There was no autopsy. Death may have been due to ventricular fibrillation. I have seen the same thing happen with digitalis. I do not know whether quinidine was responsible for death in this case.

Signs of embolism have not occurred in any of my cases.

I have always given quinidine to private patients with auricular fibrillation in a nursing home. Few patients require longer than a week's stay. If at the end of a week the normal rhythm has not returned, I dismiss the case as a failure. I insist on absolute rest in bed with the avoidance of all unnecessary movements. These precautions are taken to prevent giddiness—a symptom of which complaint is often made—and to ward off syncopal attacks which are particularly alarming to the patient, the nursing staff, and sometimes to the physician. The giddiness I attribute to the rapid heart action—a quinidine effect—and the syncopal attacks are probably vagal in origin.

A test dose of 0.2 gm. quinidine sulphate is given on the day of admission, and if there is no idiosyncrasy for the drug, on the following day I give four doses of 0.4 gm. at intervals of three hours. On the third day I add an extra cachet, and from this time onwards the patient has 2 gm. daily. It is, of course, not possible to observe the effect of quinidine on the auricular rate without a galvanometer and chest leads. This is really not a serious handicap. With a return of the rhythm to normal it is easy to recognize the change with a stethoscope. Occasionally, the heart is irregular after auricular fibrillation has ceased from extrasystoles. It is best in these cases to make certain of the type of irregularity by taking a polygraphic tracing.

Dr. Ilescu has kindly given me the figures to date of cases treated in the clinic of Sir Thomas Lewis at University College Hospital. Quinidine was given in forty-two cases of established auricular fibrillation. The patients reverted to their normal condition in twenty-five (or 59 per cent.) of these cases. Ten have remained normal; seven for more than a year. Fibrillation returned in 50 per cent. of the cases with a normal rhythm restored within two weeks. Apparently, in these cases the successes and failures seemed to be quite independent of age, venous congestion, infection, type of valve disease, or degree of enlargement. Those with a long history of fibrillation were restored to their normal state as quickly as others with fibrillation of short duration.

Dr. F. PARKES WEBER

said he wished to draw attention to a possible danger in connexion with quinidine treatment for auricular fibrillation.

A well-built Englishman, aged 60, was admitted to hospital with cardiac irregularity typical of auricular fibrillation. Swelling had been noticed in the legs in the evenings.

The heart was slightly enlarged to the left, but there was no definite murmur. The hepatic dullness extended too far downwards. The treatment, at first, was by preparations of the digitalis class, including intravenous injections of Boehringer's strophanthin; but the cardiac irregularity persisted till quinidine sulphate was given (in doses of 0.2 grm. thrice daily). Then, within a week, the pulse became perfectly regular (confirmed by careful sphygmograms). By ordinary examination there seemed to be nothing wrong with the man, excepting that his liver dullness still extended somewhat too far downwards; but his blood-serum was found to give a strongly positive Wassermann reaction. It was then ascertained that he had had a chancre thirty-four years ago, for which he had been treated, but he could not remember having had any symptoms of secondary syphilis. His wife had had one miscarriage, not long after marriage, and had never become pregnant again. Antisyphilitic treatment was commenced. The auricular fibrillation in this case may have been due to cardiac syphilis, and no history of rheumatic fever could be obtained.

There seemed to be a real danger in such cases that the disappearance of the auricular fibrillation under quinidine treatment might occasionally lead to the overlooking of syphilis as the main cause of the cardiac trouble. Thus, the cardiac disease might be allowed to progress, owing to the syphilitic factor remaining unrecognized, until antisyphilitic treatment was no longer of any avail.

Dr. Weber said he was indebted to his colleague, Dr. E. Schwarz, for the history of the first part of the above case, during a time when he (Dr. Weber) had been away from London.

Dr. B. T. PARSONS-SMITH

said that the subject of quinidine therapy was of extreme importance, for in spite of very extensive investigation the indications and usage of the drug had not yet emerged from their probationary stages.

His opinion was that quinidine was a valuable and at the same time a somewhat dangerous drug, in essence a powerful depressant and poison to cardiac muscle tissue, and in some degree to the autonomic nervous system controlling the heart's mechanism. Certain unfortunate features were invariably associated with quinidine therapy: First, the uncertainty of its action, which would appear to depend in great measure upon the inability to assess during life the varying types of pathological anatomy which underlay auricular fibrillation, and, secondly, the possibility of serious disturbances of rhythm, ventricular tachycardia, premature contractions, ventricular standstill, &c., during the transition stages. He (Dr. Parsons-Smith) particularly emphasized the remarks of Dr. Drury that quinidine therapy should, for the present, only be employed in those circumstances under which strict observation and graphic control of the cardiac mechanism could be conveniently practised, and this would involve full in-patient treatment in every case.

Extreme care should be exercised in the selection of cases for treatment by quinidine. Those patients in whom the fibrillation had been of recent onset and in whom the signs of myocardial degeneration were not advanced in degree, should be especially chosen; and at the same time cases of long standing with signs of gross failure should not be wholly excluded if there was any reasonable chance of subjective improvement following the reversion from fibrillation to the normal auriculo-ventricular rhythm.

He thought it very necessary to bear in mind that quinidine was by no means a substitute for digitalis in auricular fibrillation; the respective values

42 Parsons-Smith: *Quinidine in Cases of Cardiac Disease*

of the two drugs, as it happened, being frequently complementary, were set in entirely different spheres, and depended upon the production of effects which were diametrically opposed in certain important respects.

He (Dr. Parsons-Smith) would lay stress upon one other noteworthy factor, namely, the lowered excitability of the whole cardiac musculature which quinidine in certain cases had a tendency to produce. Clinical experience had already suggested to him that the diminished excitability was by no means a temporary event, but that on the other hand a raised threshold of excitation was likely to persist for a varying period following quinidine medication. This was well exemplified by certain cases of fibrillation, in which the heart failed to revert to the normal rhythm under quinidine, but was later controlled by smaller doses of digitalis than had formerly been needed under similar conditions of work and daily routine.

He fully endorsed Dr. Cotton's remarks with regard to the possibilities of quinidine in cases of paroxysmal tachycardia, his experience in these types of disorder leading him to consider that permanent improvement might ensue, and further that a full trial of the drug was warranted in serious cases of this nature.

Section of Therapeutics and Pharmacology.

President—Dr. W. LANGDON BROWN.

The Idiosyncrasies to Drug Tolerance of Animals as compared with Man.

By W. HAMILTON KIRK, M.R.C.V.S., Captain R.A.V.C.

PROBABLY only a small minority of men can claim the distinction of presenting to the Fellows of this Society, any facts which are entirely new to science; but in presenting this subject to the Section this afternoon, I hope that the comparisons I shall draw, will, at least, interest clinicians, and especially those who are unacquainted with the effects of drug administration on the domesticated animals.

That many species of animals, besides individuals of the same species, no less than human beings, show marked idiosyncrasies to the actions and doses of various drugs, is well known.

Perhaps the best illustration one can give, is the extraordinary effect of *morphine* upon the pig and the cat. In these animals it causes excitement and convulsions, though it proves an excellent hypnotic for man and the dog. Again, a peculiar power of resistance to the action of *atropine* seems to be inherent in monkeys and rabbits. In the human, canine, and equine species, *belladonna* accelerates cardiac action on account of its paralysing action on the vagus, the normal restraint or control exercised by this nerve over the heart being thus counteracted. But in rabbits, the vagus plays so small a part in the regulation of the heart beat, that far larger doses of *belladonna* may be given them without any appreciable increase in the rapidity of the heart beat being produced. It would require as much as 15 gr. of green extract to poison a rabbit. For a similar reason, a marked difference is observed in the action of *amyl-nitrite* on rabbits as compared with its effect on dogs. Such observations are not only interesting in themselves, but appreciably help to enhance our comprehension of the action of drugs.

Tartar emetic, a few grains of which cause almost immediate emesis in dogs and pigs, has no such physiological effect on either horses, cattle, or sheep; a fact which is readily understandable as regards ruminants, but which is somewhat more difficult of explanation in the case of the horse. Again, *apomorphine*, which promptly causes vomiting in dogs, has no emetic action on pigs. It is thought by some authorities that the insusceptibility of the horse to the action of emetics is ascribable to an inaptitude of the vagus nerve to receive and convey the special irritation; but more probably it is due to imperfect development of the vomiting centre (oblongata). Many substances which exert an emetic action on men and dogs are supposed to produce

sedative effects when given to horses in sufficient doses.¹ But with one or two exceptions, the many sedatives available in human and canine practice operate uncertainly and imperfectly on horses, for which *aconite* still remains a reliable and valuable sedative medicine (cardiac and respiratory).

"A medium-sized Scotch terrier was once given 30 minims of Fleming's tincture. In five minutes, painful and active vomiting ensued, which must have effectually emptied the stomach. The retching and vomiting continued, however, for half an hour, when the animal was so exhausted and paralysed in its hind extremities as to be unable to walk, except by supporting itself on its fore limbs and dragging the hind quarters. It gradually recovered, however, in about two hours, in spite of the phenomenal dose (normal dose $\frac{1}{2}$ minim to 1 minim). In some cases a drachm of Fleming's tincture has destroyed dogs with as much rapidity as an equal quantity of prussic acid."²

Cattle require very large doses of all medicinal agents in order to produce any effect upon them, and considerable quantities of some of the irritant and corrosive poisons can be given them with comparative impunity. This is largely accounted for by the fact that the stomach of an ox is divided into four portions, and as regards its first three divisions it is less vascular. Its function is much more mechanical and less chemical than the corresponding portion of the alimentary canal of men, dogs, or horses. Further, the first and third compartments (rumen and omasum) always contain food, often in large quantity. Such a set of circumstances would explain why purgatives, unless in large doses and in solution, are so tardy and uncertain in their effects. *Aloes* when given to cattle even in the fluid state and in doses of several ounces, fails to produce copious evacuations; and nothing under 1 lb. of *mag. sulph.* is of much avail for the cow as an aperient. A notable exception to the rule that cattle can tolerate much larger doses of irritants than horses is their susceptibility to *calomel*. Irritant effects, followed by constitutional disturbance, are produced in horses by 3 to 4 dr.; but in cattle by 2 dr.

On dogs, medicines generally operate much in the same way as upon man, but to this rule there are also some remarkable exceptions. Dogs, for instance, take 30 gr. to 1½ dr. of *aloes*, which is several times larger than the dose usually given to human patients; but they might be seriously injured by half as much *calomel* or *oleum terebinth* as is prescribed for man. The opinion generally held, that medicines may be given to dogs in the same doses as to man cannot therefore be safely entertained without some reservations. In consequence of the relatively large size of the dog's stomach, combined with a short and straight intestinal tract, purgatives act upon them with far greater rapidity than they do upon other veterinary patients. Among them all, we find that most medicines work with greater certainty and effect upon well-bred animals than upon coarsely-bred mongrels, and it is a matter of conjecture as to whether the same distinction could be drawn in human practice. Having made a short survey of the subject I will now proceed to particularize briefly on two or three selected alkaloids.

Morphine.—The effects of morphine may vary in the same individual according to the dose given, and in man and the lower animals, according to the relative development of the several parts of the central nervous system. In man morphine acts specifically and primarily upon the higher brain centres,

¹ Tartar emetic has a sedative effect on horses.

² "Experiments at Royal Dick Veterinary College."

which are depressed, and the patient is usually calmed and sleeps, and after large doses becomes comatose. In some of the lower animals, a stimulation of the locomotor centres of the brain, and of the reflex centres of the cord, is manifested; thus, instead of being quieted and hypnotized, they are at first excited, exhibiting irregular involuntary movements, and after toxic doses, tetanic convulsions and a coma, from which however they can be readily roused. In some veterinary patients the prominent phenomena are agitation or unrest, inco-ordinated movements, vomiting, diminished sensibility to pain, and in toxic doses—convulsions, coma, and death by respiratory arrest. The more highly an organ is developed, the more susceptible does it become to the action of certain drugs, and this general law explains why the highly developed human brain is specially susceptible to the effects of such cerebral medicines as opium and chloral; and why frogs and cats, whose spinal systems are better developed than their brains, are so susceptible to strychnine, which acts specially on the cord. Whilst the human cerebrum is, I believe, more than seven times the weight of the mesencephalon and cerebellum, we find in domestic animals that the cerebrum is only five times the weight of the posterior parts of the brain, the cord being relatively larger than in man. These differences of development explain how such drugs as opium, chloroform and chloral produce blunted intellectual function and deep stupor in man, whilst in the lower animals they produce less marked depression of brain function, but with more marked derangement of motor function and convulsions. *Horses*, with less development of these higher brain centres, have relatively more development of the locomotor centres and of the reflex centres of the spinal cord, and upon these lower centres, opiates in equine subjects exert their primary stimulant effects. Full doses produce, at first, restless involuntary movements of the head and limbs, pawing the ground, or walking continuously round the box; this is followed by sleepiness, disinclination to move, and when the horse is moved, by staggering. Excessive doses cause tetanic convulsions, although I have repeatedly injected as much as 20 gr. of the hydrochloride subcutaneously into a horse without producing the slightest toxic symptom. This would occur, of course, during a severe attack of colic, when a horse is able to withstand much larger doses than it would in the absence of pain. The average dose ranges from 3 to 10 gr. Horses will, with impunity, take by the mouth about one hundred times as much opium as would poison a man. *Ruminants* are affected in very much the same way as horses, that is, they become excited and restless. Cattle bellow, and sometimes digestion is impeded and tympanites frequently supervenes. One cannot readily induce sleep in them, except by the influence of full and repeated doses. *Dogs* exhibit effects more nearly comparable to those observed in man. Relatively to their body weight they take eight to ten times the dose prescribed for human beings. They show less involuntary muscular movement than the horse, and little or none of the excitability of the cat. Sleep is never profound; in fact, dogs are quite easily aroused. They seem to dream and have hallucinations, and, after a full dose, remain stupid for a whole day. The average hypodermic dose of morphine is 1 gr., but terriers can tolerate 2 gr. quite safely, whilst some of the larger breeds will stand as much as 5 gr. without exhibiting toxic symptoms.¹

¹ Hobday considers that the range between the narcotic and toxic dose in the dog is so great, that it is almost impossible to produce poisoning. He once administered 27 gr. to an old poodle suffering from cancer in the throat, the dog recovering some twenty-two hours later sufficiently to be able to walk and drink.

The pupil is not dilated, as frequently happens in the horse and cat, nor continuously contracted, as in man; but it has been observed to be in a state of contraction during narcosis. *Cats* become very excited under the influence of morphine, tearing round the room, or running up the curtains in an alarming fashion; hypnosis is produced in them with such difficulty, that morphine plays no part in feline practice. In *rabbits* also the motor and spinal centres are prominently affected, and convulsions are more common than hypnotism. *Birds* are curiously insusceptible, for relatively to their weight they are stated to take three hundred times the human dose and do not sleep or show any alteration of the pupil. Morphine convulses *frogs*, but, even in large doses, has no effect on pigeons except in reducing their temperature.

Cocaine.—I read that the outstanding symptoms of a toxic dose of cocaine in man are vertigo, faintness, small rapid pulse, and prostration, and that the antidotes recommended are stimulants, amyl nitrite, and artificial respiration. As I do not indulge in the practice of administering overdoses to my own patients, I cannot produce any extensive statistics of personal observations in cocaine poisoning. But to show the idiosyncrasies of some dogs in regard to the amount of cocaine they can tolerate, I will relate an instance which recently occurred in my practice, in which a dose of this drug—regarded usually as a normal and safe amount—proved to be thoroughly toxic. The patient was an aged terrier, in somewhat poor condition, and of about 25 lb. body-weight, afflicted with multiple neoplasms. I injected 25 minims of an 8 per cent. solution into various parts of the body surface, and within three minutes a brief stage of clonic convulsions was manifest, which almost immediately became tonic, the eyes rolling and the dog profusely salivating and licking its lips. Only a very few more minutes elapsed before opisthotonos was well marked, and all limbs rigidly extended and cold; respiration considerably slowed and the pulse hardly perceptible. Syrup of chloral was at once administered *per os*, and followed up by the application of chloroform to the nostrils. As life appeared to have become extinct, artificial respiration was commenced and persisted in for half an hour; signs of recovery then became evident, and the dog's state was considered safe. The after-effects were very curious, the animal being quite unable to remain still a moment, or to decide how or where to rest in its basket. It moved constantly in a circle, sometimes getting one leg over the side and occasionally falling out. It would crouch as if in fear when shouted at, or when pretence was made to strike it. The dog would then remain still for a few moments, only to resume its extraordinary gyrations. Another peculiarity was the repeated attempt made by the dog to raise its hindquarters in the air; it then stood only on its front legs, and as often as not would lose its balance. Although the operation was performed at 6 p.m., the after-effects had not wholly subsided until about 10 o'clock the next morning. I had used 2 gr. of cocaine on many previous occasions without any more alarming symptoms than salivation and licking of the lips, both of which seem inseparable from the use of this drug. In no text-book other than Hobday's "Veterinary Anæsthetics" have I seen reference to this constant manifestation—salivation, although it is a symptom which can be demonstrated in dogs almost immediately after every administration of cocaine, whether in toxic dose or not. The toxic phenomena observed in this clinical case are not strange to canine practitioners, and they serve to show the great dissimilarity of the manifestations of cocaine poisoning in man as compared with those seen in members of the genus *Canidæ*. *Horses* may be given any quantity up to 10 gr., but even as much as 80 gr. injected

subcutaneously has been sometimes found insufficient to kill. Such an amount would render a horse very restless and excited; it would paw with its fore feet, neigh, and appear timid; the pulse would increase in rapidity to about double the normal rate (90); salivation would occur, and the pupil would dilate. At the end of an hour, however, its condition would be one of frenzied excitement, with a greatly augmented reflex activity. Probably after the lapse of a further two hours, these effects would disappear. *Cattle* respond in a similar fashion, and the injection of 1 dr. is stated to produce excitement bordering on madness, and continuing for four hours, but gradually passing off, and leaving no injurious effect. *Dogs* should never receive more than $\frac{1}{10}$ gr. of cocaine per pound of the body weight; those receiving more than this amount exhibit psychical excitement, tetanic and clonic spasms, epileptic fits, loss of co-ordination and dyspnoea. The spasms and more prominent symptoms, however, do not occur when potassium bromide or ether have previously been given. Hobday states that 12 to 15 gr. kill small dogs in ten minutes. It may, perhaps be mentioned in passing that the South American Indians and the natives of several other countries have for centuries been in the habit of not only eating coca leaves themselves, but also of giving them to their horses, to diminish the sense of fatigue, thirst, &c. From this custom has doubtless arisen the more modern practice of doping racehorses, in order to produce in them such temporary excitement and vigour as will enable them to win.

Strychnine.—The action of strychnine is almost identical throughout the vertebrate kingdom, but there are marked differences in the amounts which the various animals can tolerate with safety. As I have already mentioned, human posology more nearly resembles that of the dog than of any other animal, but in the case of strychnine, dogs are decidedly more susceptible than man. Thus, whilst an adult terrier should receive no more than $\frac{1}{80}$ gr., man can safely take $\frac{1}{15}$ gr. Dogs may be destroyed in twelve minutes by $\frac{1}{8}$ gr. of strychnine, and in two minutes by $\frac{1}{4}$ gr. (Christison). I knew of a case in which a Pekingese picked up and inadvertently swallowed an Easton's syrup capsule, the contents of which were presumably equivalent to 1 dr. of the syrup, and was dead within ten minutes. This, however, is rather exceptional. The toxic dose for an average dog may be regarded as about $\frac{1}{15}$ gr. Cats, which usually tolerate half the dose for the dog, would be greatly endangered if given more than $\frac{1}{8}$ of the canine dose. In practice I rarely prescribe a greater dose of strychnine for cats than $\frac{1}{800}$ gr. Horses and cattle are not by any means so readily affected as men and dogs. Given hypodermically, the toxic dose for horses is stated by Fröhner and Kaufmann to be from 3 to 6 gr.; whilst the safe medicinal dose is 1 gr. To prove the assertion that cattle withstand larger doses than do horses, when administered by the month, Macgillivray (a veterinary surgeon) gave an aged cow 30 gr. of strychnine, and shortly afterwards 60 gr., both doses in solution, with the result of a few spasmodic tremors which continued for about twenty minutes. Very much smaller doses are fatal when the strychnine is quickly absorbed. Thus Kaufmann states that the lethal hypodermic dose is 5 or 6 gr.; but several cases have occurred recently in which practitioners have administered only 1 gr., with unexpected fatal results. Consequently it is now a much more common belief that cattle are, in reality, more susceptible to strychnine than are horses, though I am at a loss to explain why they should be. Pigs are violently convulsed and poisoned by $\frac{1}{8}$ to $\frac{3}{4}$ gr., whilst the domestic fowl tolerates comparatively large quantities without symptoms.

PROCEEDINGS
OF THE
ROYAL SOCIETY OF MEDICINE

EDITED BY
SIR JOHN Y. W. MACALISTER

UNDER THE DIRECTION OF
THE EDITORIAL COMMITTEE

VOLUME THE SIXTEENTH

SESSION 1922-23

SECTION OF TROPICAL DISEASES & PARASITOLOGY



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1923

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SECTION OF TROPICAL DISEASES AND PARASITOLOGY

CONTENTS.

November 6, 1922.

	PAGE
Lieutenant-Colonel H. MARRIAN PERRY, O.B.E., R.A.M.C.	
Some Observations on the Occurrence of <i>Leishmania</i> in the Intestinal Tissues in Indian Kala-Azar ; on the Pathological Changes occasioned by their Presence, and on their possible Significance in this Situation 	1
M. KHALIL, M.D., Ph.D.	
A Description of <i>Gastrodiscoides hominis</i> , from the Napu mouse deer ...	8

February 5, 1922.

ARTHUR POWELL, M.B.	
Framboesia : History of its Introduction into India ; with Personal Observations of over 200 Initial Lesions 	15

March 5, 1923.

G. M. VEVERS, L.R.C.P.Lond. ; M.R.C.S.Eng.	
The Lung Flukes of the genus <i>Paragonimus</i> : A Demonstration ...	43
R. J. ORTLEPP.	
Life-history of the Gape-worm (Abstract) 	44
Wing-Commander H. E. WHITTINGHAM, R.A.F.M.S.	
Life-history of the Sandfly, <i>Phlebotomus papatasi</i> (Abstract) ...	45
W. BROUGHTON-ALCOCK, M.B.	
Case of Spirochaetal Dysentery (Abstract) 	46
J. B. CHRISTOPHERSON, O.B.E., M.D.	
Remarks upon a Photograph of an Endemic Focus of Bilharzia Disease in Portugal; Specimens of the Intermediary Host, <i>Planorbis</i> <i>dufourii</i> (Graells) 	47

ANDREW ROBERTSON, M.B.	PAGE
Specimens from a Human Case of Infection with <i>Dientamoeba fragilis</i> , Jepps and Dobell, 1917	48

May 7, 1923.

A. E. HAMERTON, C.M.G., D.S.O., Lieutenant-Colonel R.A.M.C.	
The Establishment of an Antirabic Institute in the Tropics	49
Exhibited by J. B. CHRISTOPHERSON, C.B.E., M.D.	
Specimens of (1) <i>Schistosoma bovis</i> and of its Snail Carrier; (2) the Intermediate Hosts of <i>Schistosoma mansoni</i> , Brazil	56

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Section of Tropical Diseases and Parasitology.

President — Sir LEONARD ROGERS, C.I.E., M.D., F.R.S., I.M.S.

Some Observations on the Occurrence of *Leishmania* in the Intestinal Tissues in Indian Kala-Azar; on the Pathological Changes occasioned by their Presence, and on their possible Significance in this Situation.

By Lieutenant-Colonel H. MARRIAN PERRY, O.B.E., R.A.M.C.

SINCE the discovery by Leishman, in 1900, of the infecting organism of Indian kala-azar, the cutaneous and visceral infections caused by these parasites have attracted considerable interest.

Notwithstanding the widespread research into the possible methods of transmission of the visceral form of the disease, we are still without information regarding the means of its transference from man to man. There are, however, other aspects of this infection on which more exact information is available. The pathological changes encountered in fatal cases of visceral leishmaniasis are now well recognized, and the situation and distribution of the parasites in the tissues have been more or less defined. In any consideration of the pathology of the disease, it is evident that the most predominant feature is the early increase in size of the liver, and more particularly of the spleen. The great enlargement of the latter organ, resulting from an increase in the splenic pulp, is caused by the enormous proliferation of lymphatic and vascular endothelium. The distribution of the *Leishmania* bodies is typically as intracellular parasites of these hypertrophied endothelial cells, and, although occurring in largest numbers in the spleen, liver and bone marrow, these protozoal organisms have been demonstrated in almost every other tissue of the body. They are fairly abundant in the bone marrow, and have also been observed in smaller numbers in the kidneys, the suprarenals and in the pancreas. It is a matter of comment that less attention appears to have been directed to the distribution of the parasites in the gastro-intestinal system, and to the changes resulting from their presence in this situation.

In reviewing the literature dealing with this aspect of the pathology of the infection, it is noted that most of the observations appear to have been directed to the gross lesions which are found in the large intestine in many cases of the disease. Christophers [1] comments on the fact that it is common in India to find a widespread multiple ulceration in this part of the bowel, the ulceration extending deeply into the muscular coat of the intestine. In some of his cases amœbæ have been found in the ulcers and associated liver abscess

2 Perry: *Observations on the Occurrence of Leishmania*

has been present, evidently coincident infections with *Entamoeba histolytica*. Jemma and Di Christina [2] note the constant occurrence of an enterocolitis in cases of infantile kala-azar, and the presence in the large intestine of circular ulcers with elevated edges. These latter observers have further found on microscopic examination that these lesions were associated with a small round-celled infiltration of the mucosa and muscular coat, together with dilatation of the lymphatic vessels and hypertrophy of the endothelium in which *Leishmania* parasites were sometimes present. Christophers also records the presence of *Leishmania* as intracellular parasites of endothelial cells lining the blood-vessels in the granulation tissue of ulcers in the large intestine, whilst Archibald [3], writing on the pathology of the infection, states that in his experience of the visceral form of the disease the stomach and small intestine are not commonly affected, but that the large intestine may show congested and ulcerated areas in the greater part of its length.

In this brief review of the pathology of the disease as it affects the gastrointestinal system, it will be noted that no observations are recorded relative to changes in the small intestine. It is in connexion with certain microscopic appearances seen in this portion of the intestinal tract in two fatal cases of the Indian variety of the disease that this communication is made.

The material available for investigation, and on which the observations that follow are based, consisted of small portions of the jejunum, which, as far as macroscopic appearances are concerned, would not have been considered abnormal at post-mortem examination. On closer investigation, the mucosa could be seen to be slightly thickened, the other coats of the intestine being normal in appearance. No evident breach of surface or ulceration was apparent in the mucous coat of the portions of tissue examined. The microscopic investigation of sections made from various parts of these tissues demonstrated a very consistent and interesting pathological picture. The changes observed can be shortly summarized as follows:—

The histological appearance of the serous, muscular and submucous coats differed in no detail from that normally seen in the small intestine. The most striking change was that evident in the mucous membrane owing to a very definite and remarkable alteration in the villi. These processes of mucous membrane had undergone a complete metamorphosis, and, instead of appearing as slender narrow fimbria as seen in the normal intestine, each villus was transformed into a swollen, distorted and polypoid body connected with the submucous tissue by a constricted stalk formed of a few fibres of connective tissue. The columnar epithelium covering the villi had disappeared and the basement membrane furnished a delicate limiting sheath for each little swelling. The internal structure of the villi was completely altered owing to an intense proliferation of the endothelial cells lining the lymph channels. This proliferation of endothelium, although marked in the base of the villi, became more pronounced towards the centre and extremities of these structures, and the enlargement and distortion was caused by these tightly packed accumulations of hypertrophied cells. In the greater number of villi the basement membrane was intact, but in many instances it had ruptured from over-distension and liberated the enclosed endothelial cells.

The distribution of *Leishmania* bodies in the intestine was very striking. They could be demonstrated in scanty numbers in the submucous coat; in that position they occurred in endothelial cells evidently derived from vascular endothelium. They were present in larger numbers, in the same intracellular situation, in the base of the villi. In the centre of the villi they had under-

gone rapid multiplication, and they were present in enormous numbers in the endothelial cells distending the extremities of these structures. In many of the villi numbers of endothelial cells had broken down, and the parasites were lying free in the villus, mixed with the débris of necrotic cells. Reference to the figures illustrating the changes observed will enable the striking alteration in appearance of the villi to be appreciated. I have had the opportunity of examining spleen and liver sections from a large number of cases of visceral leishmaniasis, and have not in any case observed an infection of endothelial cells which would compare numerically with the great numbers of parasites present in these endothelial villous tumours.

A comparison of the cellular reaction observed in the small intestine in these cases of visceral infection with that evident in the subcutaneous tissues in cutaneous leishmaniasis illustrates the close analogy between the pathology of the two conditions.

In both infections the type of cell involved is the lymphatic and vascular endothelium, and any difference observed depends entirely on the localization of the parasites. In the visceral disease, at least as far as the above cases are concerned, the intestinal villi were heavily infected, and the resulting endothelial proliferation had formed a series of endothelial villous tumours. In the cutaneous form of the disease, an oriental sore at its inception is nothing more than a cutaneous endothelial tumour. The continued accumulation of endothelial cells, either in the villi or in the cutaneous tissues, eventually leads by mechanical pressure to a deficiency or obliteration of the blood supply and consequent atrophy and necrosis of these cells, which finally terminates in ulceration.

DISCUSSION ON THE POSSIBLE RELATION THESE OBSERVATIONS MAY HAVE ON CERTAIN ASPECTS OF INDIAN KALA-AZAR.

(1) *On the Clinical Course and Symptoms of the Disease.*

In any clinical description of the disease the progressive wasting and, finally, the extreme emaciation which occurs in the established infection is emphasized. Thus, in compiling a table illustrating the differential diagnosis between Indian kala-azar and chronic malaria, Knowles notes that in kala-azar emaciation is very marked and sometimes extreme, whilst in chronic malaria it is less noticeable. Further, the constant recurrence of symptoms referable to the intestinal tract, such as enteritis, are very commonly observed during the course of the disease.

That progressive and extreme emaciation should be a common feature in the clinical picture appears to me not to be surprising if the pathological changes in the small intestine described above can be shown to be usually present.

It is obvious that the profound alteration of the intestinal villi must almost arrest, or at least considerably reduce, the absorption of nutritive substance. The destruction of the epithelial covering of the villi, combined with the vascular and lymphatic stasis caused by the pressure of accumulated endothelial cells in their interior, must produce a complete perversion of their normal physiological function and render them useless for purposes of nutrition. In a similar manner it is possible that the recurrent attacks of diarrhoea, so typical during the disease, may find their explanation in this alteration in structure of the mucous lining of the bowel.

4 Perry: *Observations on the Occurrence of Leishmania*

(2) *On Methods of Transmission of the Disease.*

In discussing the relationship which these observations on the distribution of *Leishmania* parasites in the tissues of the small intestine might be conjectured to have on the problem of transmission of the disease, I do not wish it to be assumed that I favour the theory of direct infection of the human subject by the alimentary route. The knowledge that the infecting parasite of kala-azar is a flagellate included in the genus *Herpetomonas*, and that the known species of this genus find their primary habitat in the intestinal tract of an insect host, is a very strong argument against the direct transmission of the organism through the agency of infected faecal material. Admittedly, however, there are only two possible routes by which the parasites can escape from the human tissues, i.e., either from the peripheral blood, through the agency of some blood-sucking ecto-parasite, or from the alimentary canal in the faeces.

The researches undertaken with the view of the incrimination of an intermediate insect host are ably discussed by Patton [4], who is strongly in favour of this method of transmission of the disease. He cites numerous experiments illustrating the longevity and development of the parasites in the intestinal canal of bugs belonging to the genus *Cimex*, and quotes the developmental changes observed by Cornwall in preparations containing the flagellate stage of the parasite and portions of the mucous membrane of the stomach of *Cimex rotundatus* (Hemiptera) "as furnishing final proof that *Cimex* is the true inter-vertebrate host of *Herpetomonas donovani*."

The recent work of Mrs. Adie [5] in India on the development of flagellates in the cells of the mid-gut of *Cimex hemiptera* which had been fed on infected splenic pulp has been criticized by Wenyon [6] on the grounds that these observations had been made on bugs which had died after the infecting feed and had not been examined until after the lapse of some days. The further observation of Mrs. Adie on the presence and multiplication of the parasites in the salivary glands of *Cimex* have not been accepted, the bodies observed being neither *Leishmania* nor a developmental phase of some other flagellate.

The possibility of the dissemination of infection by mosquitoes, sand-flies, lice and ticks has been closely investigated by many observers, but up to the present the investigation has yielded negative results.

The fact, however, that viable forms of the parasite can be shown by cultivation to occur in the peripheral blood, and that the incidence of the organisms in this situation is very much higher than was formerly believed, favours the view that infection is probably spread from this source by some insect host. Thus Patton states that it was the exception to fail in finding parasites in the peripheral blood of his cases in Madras, and Knowles [7] records the presence of *Leishmania* in blood films in 45 per cent. of the cases under his investigation in Shillong.

The present position regarding the problem of transmission of infection through the intermediary of an insect host can be summarized by the statement that no conclusive evidence is at the moment forthcoming which would incriminate any of the above agents.

The second possible method of elimination of the parasite from the human body being by the faeces, the possibility of direct faecal transmission of the disease from infected cases to healthy human subjects has received some consideration and support. Knowles, in discussing this aspect of the problem, cites the case of a municipal sweeper in Nowgong whose only apparent contact with infection was in connexion with his conservancy duties. The same

observer figures small oval cytoplasmic bodies found in the dysenteric mucus of several cases of kala-azar suffering from intestinal symptoms, but he could not reconcile their nature with any known form of the parasite. Mackie [8] also noted and recorded the presence in faecal mucus of small cytoplasmic bodies with a chromatin nuclear structure which he considered indistinguishable from *Leishmania*: and Minchin, to whom the preparations were submitted, expressed the opinion that they resembled *Leishmania*. To obtain a series of controls, Mackie examined the faeces of twenty-six healthy individuals living in the same area as the kala-azar cases in which the presence of these puzzling "bodies" was observed. In none of these individuals could he demonstrate structures of similar appearance. A limited number of feeding experiments with mucus containing these bodies was undertaken, dogs and monkeys being employed, but these experiments yielded negative results. Patton, on repeated examinations, has failed to find any bodies of the nature of *Leishmania* in the faeces, and concludes that if the parasites appear in the excreta they do not occur in their usual round or oval forms. Both Knowles and Patton have failed to obtain any evidence of development in cultures made from intestinal mucus. This latter fact is, however, of little importance owing to the impossibility of obtaining cultures uncontaminated with bacteria.

Feeding experiments with intestinal mucus from infected cases have failed to give positive results in a limited number of instances as noted above, but Archibald and others have recently been successful in infecting monkeys by feeding them on infected splenic pulp.

The possibility of dissemination of the parasites by a helminthic agency has been investigated, but has failed to help in the elucidation of the problem.

The above very limited survey of the painstaking research work carried out on the transmission of kala-azar has been given to emphasize the fact that the problem of the method of spread of this disease still awaits solution, and that any fresh observations relative to the pathology of the infection deserve consideration.

In an earlier part of this paper it has been recorded, in the description of the pathological appearances observed in the small intestine in two cases of the disease, that the localization of the parasites was mainly in the intestinal villi, and that there was evidence of intense multiplication in this situation leading to an enormous increase in their numbers in the extremities of these structures (fig. 2, p. 7). The delicate nature of the sheath enclosing these little swollen processes of mucous membrane was mentioned, also the fact that in many of them this had ruptured and liberated the enclosed parasites and endothelial cells into the lumen of the intestine (fig. 3, p. 7). It is possible, and indeed even probable, that in many instances this rupture of the villi had occurred in the preparation of the sections, but there is little doubt from the swollen and dilated appearance of the majority of these processes that over-distension of the limiting membrane was sufficient to cause rupture during life. It is evident, therefore, that myriads of *Leishmania* parasites must have been liberated into the intestinal contents during the course of the disease, and it is interesting to conjecture as to the possibility of their survival in the faeces.

The fact that the flagellate stage of the organism is incapable of living for any length of time in cultures which have been contaminated by bacteria is against the presumption of the continued viability of the parasite in faecal material in this phase of its development. Is it possible that some hitherto undescribed encysted form is developed in the faeces? The contaminative

6 Perry: *Observations on the Occurrence of Leishmania*

cycle of development of the various species of *Herpetomonas* of insects, to which attention has often been directed in this connexion, is a tempting analogy. Again, if the parasite in some resistant form can withstand the inimical nature of its surroundings in the fæces, have the possibilities of its ingestion by some fæcal-feeding insect been exhausted?

Further research alone can decide whether the insect-borne or alimentary theory will prove to be correct in defining the exact ætiology of the disease,

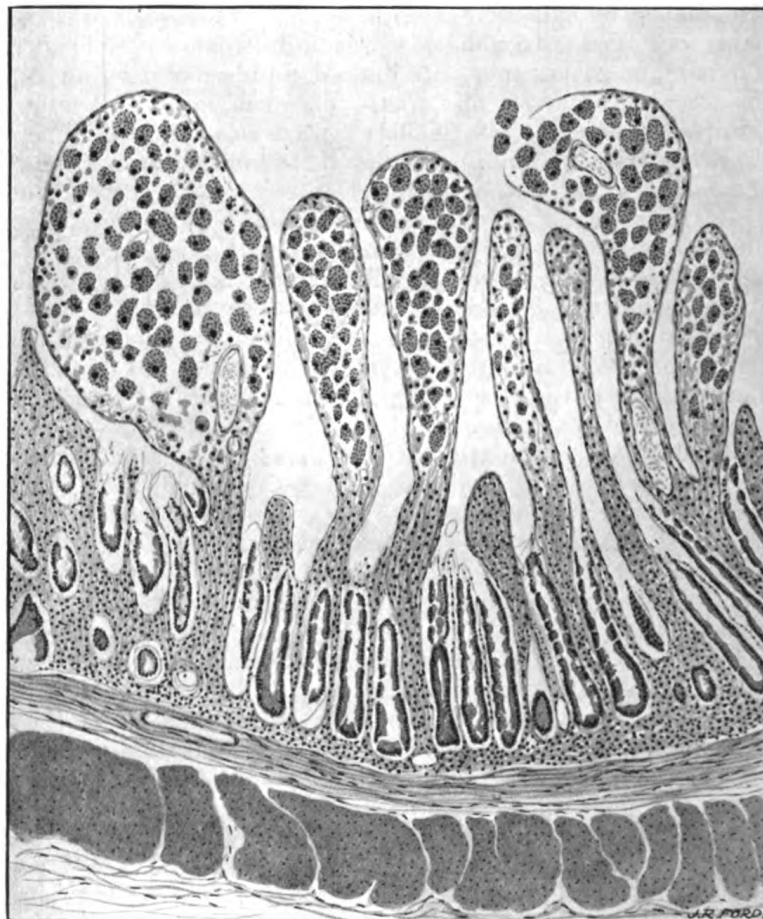


FIG. 1.¹

but the fact that, in at least some cases of kala-azar, there occurs an intense elimination of parasites into the intestinal canal may stimulate renewed research into every possible method of dissemination of infection through the medium of infected fæces. In this connexion the summing up of Knowles [7] on this aspect of the question may be quoted: "This possibility (i.e., fæcal transmission) deserves, perhaps, more careful consideration than it has hitherto received. In Assam, at least, the distribution and incidence of kala-azar is

¹ For the loan of the blocks illustrating this paper the author is indebted to the Editor of the *Journal of the Royal Army Medical Corps*.

closely associated with insanitary surroundings and an absence of all conservancy methods."

In conclusion, I wish to emphasize the fact that I do not suggest that these findings are of usual, or even of frequent occurrence, in the small intestine in kala-azar. They appear to me, however, to be of sufficient interest and importance to bring forward in the hope that, attention having been directed to them, information may become available as to the extent to which they occur in fatal cases of this infection.

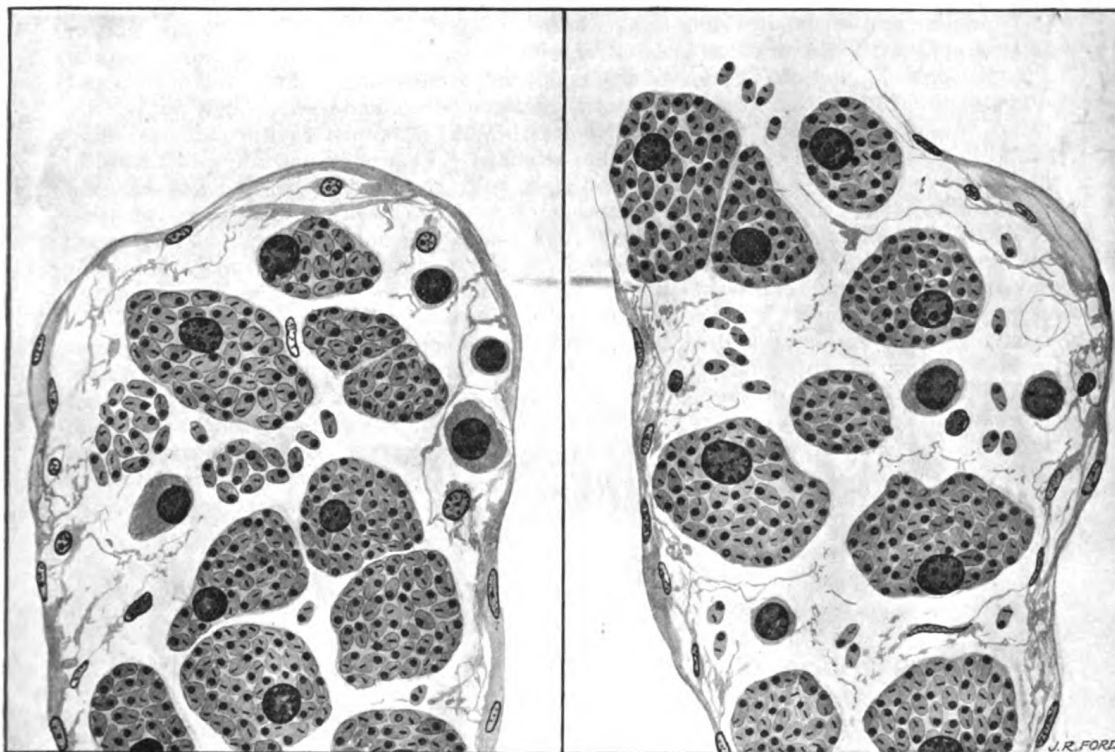


FIG. 2.

FIG. 3.

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Dr. Row (Bombay) said that Colonel Perry's contribution was most important in the study of the complex phenomena observed in leishmaniasis. A study of the illustrations and lantern slides which he had shown convinced one of the importance of a close and co-ordinated study of pathological processes in viscera apparently healthy, even in the absence of gross post-mortem changes in the organs. Apparently no regular or routine examination of the small intestines which showed no obvious lesions (e.g., ulceration) had been yet made, and all the attention of workers had been directed to

the naked-eye and microscopic study of the ulcers found in the large bowel to which Colonel Perry had drawn attention. It was only natural that, in the absence of microscopic data of apparently healthy mucosa of the small intestines in kala-azar, no record of the remarkable invasion of the villi with the Leishman-Donovan parasites could be found in any of the memoirs on this subject. It seemed that in the light of what Colonel Perry had shown them, it was essential to examine every tissue and every organ even when these gave no suspicion of any pathological changes. The difficulty of identifying anything resembling the Leishman-Donovan parasites in the gut contents was only to be expected when one remembered how the parasite could pass through morphological changes, beyond recognition, even in artificial cultures—a fact which he (Dr. Row) had communicated before the Royal Society of Tropical Medicine last month—and especially when one bore in mind the massive microbic proportion of the gut content. There was the greater reason to suspect cryptic forms of *Leishmania* in the gut contents, and such forms, it was conceivable, might be taken up by insects and disseminated in some way unknown; and, as Colonel Perry had so cautiously put it, only future investigation would settle these points. With reference to the parasite being found in the peripheral circulation to the extent of 45 per cent. according to Knowles, he would state his own experience on this subject although he had not had the opportunity of studying a very large number of cases. Out of twenty-five consecutive cases he had obtained cent. per cent. positive results by adopting a slight manoeuvre in the technique, which consisted in diluting the peripheral blood from a kala-azar patient with a large volume of citrated saline solution and using the deposit of the corpuscles for culture. This was done with the object of diluting the serum and reducing to a minimum the destructive influences of any antibodies or other agencies in the blood serum antagonistic to the flagellates of the scanty parasites, or parasites weakened by the grip of the leucocytes in the blood. In any case it seemed that more attention had been drawn to the presence of parasites in the blood and less to the presence of the parasites in the intestinal contents; and Colonel Perry's paper, therefore, opened up a wide field for further investigation of the ætiological factors of leishmaniasis, which up till then had eluded the observation of all workers.

A Description of *Gastrodiscoides hominis* from the Napu Mouse Deer.

By M. KHALIL, M.D., Ph.D.

(From the Department of Helminthology, London School of Tropical Medicine.)

WHILE acting as Honorary Parasitologist to the Zoological Society of London, I found a large number of living specimens of *Gastrodiscoides hominis* in the cæcum of the Napu mouse deer ("*Tragulus napu*") that died in the Zoological Gardens. The animal belonged to the Prince of Wales' collection from the Malay States. In addition, the abundant material from man in the Helminthological Department of the London School of Tropical Medicine was available for comparison. Whole mounts and serial sections of specimens from both collections were examined and compared.

Synonyms: *Amphistomum hominis*, Lewis and McConnell, 1876. *Amphistomum* (*Gastrodiscus*) *hominis*, Sonsino, 1895. *Gastrodiscus* (?) *hominis*, Fischöeder, 1902. *Gastrodiscus hominis*, Stephens, 1906. *Gastrodiscoides hominis*, Leiper, 1913.

(I) HISTORICAL REVIEW.

The parasite was first discovered and described by Lewis and McConnell [10] in 1876. They found it in the cæcum of man. Their description of the internal structure is inaccurate and incomplete. They claimed that the parasite had one testis and one ovary. As Stephens suggested, it is probable that Lewis and McConnell overlooked the presence of the relatively small ovary and interpreted the posterior testis as an ovary.

Stephens (1906) redescribed the parasite. His material was also from man in Assam. His description is brief and his figures are very diagrammatic. He amplified the description of the parasite and corrected some of Lewis's and McConnell's statements as to the internal anatomy.

The parasite was first placed in the genus *Amphistoma*, which has since lapsed as a synonym. It was tentatively placed in the genus *Gastrodiscus* (Leuckart, 1877) by Fischöder in 1902. This was confirmed by Stephens [15] (1906). Leiper [8] re-examined the parasite in 1913 and created the genus *Gastrodiscoides* for it. This genus has not been widely accepted, and recently Clayton Lane [5] criticized some of the characters upon which the genus *Gastrodiscoides* is based and retains the parasite in the genus *Gastrodiscus*.

(II) EXTERNAL CHARACTERS.

(a) *Colour*.—Fresh specimens are reddish. The colour of preserved material differs according to mode of preservation. Specimens fixed in Schaudinn's fluid are whitish. In formalin they have a dirty brown tinge.

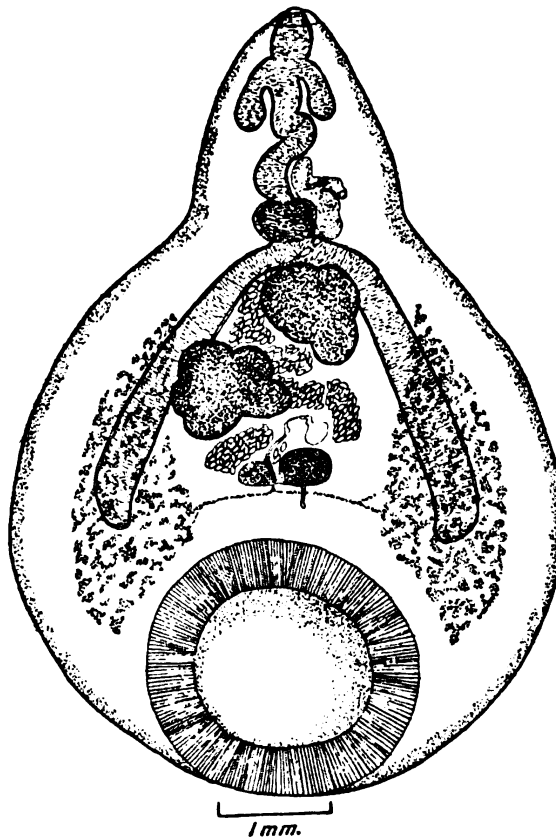
(b) *Size*.—Living specimens vary a good deal in size; the body is very contractile, and can be elongated to a length of 1 cm. The distinction between the anterior elongated part of the body and the posterior discoidal part is ill-defined. Specimens preserved in alcohol are shrunken in appearance and vary in length. The length of specimens preserved in formalin varies between 5 mm. and 7 mm. When fixed in Schaudinn's fluid, the specimens have uniform shape, and are very constant in size, being 5 mm. in length. In the following description measurements refer to specimens fixed by the last-mentioned method.

(c) *Form*.—When preserved in Schaudinn's fluid, the body is divided into an anterior conical portion and a posterior discoidal portion, quite evident to the naked eye. (See figure, p. 10.) The anterior portion is about 2 mm. in length and is flattened dorso-ventrally. The anterior extremity is bluntly rounded, and the transverse diameter increases in width towards the junction with the posterior discoidal portion. The anterior surface is flattened, and may be a little concave. In the middle line, nearer the posterior than the anterior extremity, protrudes the prominent genital papilla, distinctly visible to the naked eye. This characteristic is constant in all specimens preserved in Schaudinn's fluid, although it is less marked in specimens preserved in formalin. The dorsal surface is convex. The discoidal portion of the trematode is practically circular, being a little elongated in the long axis of the body. The ventral surface is deeply excavated with a thick overhanging rim, which becomes less marked at the junction with the anterior conical portion. The dorsal surface is very convex.

(d) *Acetabulum*.—The acetabulum is in the caudal portion of the body. Its aperture is directed ventrally. It is 2 mm. in diameter. The aperture of the acetabulum is 1.75 mm. in diameter. The cavity extends rather towards the

head than caudally. The rim of the acetabulum is overlapped by the surrounding tissue, but is separated from it by a deep narrow groove. The cavity of the acetabulum is 1.42 mm. long and 0.5 mm. deep.

(e) *Cuticle*.—The cuticle is smooth and unprovided with spines. The discoidal portion of the worm is devoid of papillæ or pseudo-suckers, as seen in *Gastrodiscus*.



Gastrodiscoides hominis. Camera lucida tracing of parasite flattened, while living, with two slides and fixed in 70 per cent. alcohol. Genital cone pressed to one side.

(III) DIGESTIVE TRACT.

The oral aperture which pierces the cephalic extremity of the worm is not provided with a sphincter. It leads directly into a muscular sucker. The oral sucker is globular; length 0.42 mm., and breadth 0.35 mm. Its posterior end is slightly constricted. The two pharyngeal pouches join the pharynx at this point. Each pharyngeal pouch is pear-shaped; length 0.5 mm., diameter 0.25 mm. The pharynx is elongated, and pursues a rather tortuous course; length 1 mm., diameter 0.25 mm. It ends posteriorly in a strong pharyngeal bulb. The pharyngeal bulb connects the pharynx with the two intestinal cæca. It is globular, with a diameter of 0.45 mm. The two intestinal cæca curve gracefully, caudally, around the anterior testis, and then follow a straight course to their termination, diverging gradually from one

another as they are followed to their termination. The termination of each cæcum is slightly bent inwards, and lies on a plane slightly cephalad of the acetabulum. The length of the cæca is 2.75 mm. Their termination is 2.5 mm. apart. In their caudal half the cæca are surrounded by the vitellaria.

(IV) THE GENITAL SYSTEM.

The male and the female genital organs, with the exception of the vitelline glands, lie mainly between the intestinal cæca with the testes slightly overlapping the gut.

(a) *Male Organs*.—There are two large slightly lobulated testes which lie close to the bifurcation of the gut. The testes are strictly diagonal, but their zones and fields overlap to a great extent. In longitudinal serial section the central portion of the worm will show a considerable portion of one testis lying directly in front of the other. This appearance probably led both Stephens and Leiper to assume that such was the position of the testes. The anterior testis is faintly marked into four lobules. It is 0.75 mm. in average diameter. It is situated at the junction of the two intestinal cæca with its main portion to the left of the sagittal plane. It slightly overlaps the intestinal cæca in the neighbourhood. The posterior testis is faintly marked into five lobules. Its average diameter is 0.75 mm. Its bulb lies to the right of the sagittal plane, overlapping the right intestinal cæcum. Its main portion is situated posteriorly to the anterior testis. The narrow space between the two testes is about 0.2 mm. in width. In sections, owing to the contraction and shrinkage of the worm, the testes may actually be seen in contact with one another. Both testes lie nearer the ventral than the dorsal surface. They are entirely within the discoidal portion of the worm. From the dorsal aspect of each testis the corresponding vas efferens proceeds, both uniting to form the common vas. The vas inclines towards the ventral surface. It is dilated in part of its course to form apparently a seminal vesicle. There is no evidence of the presence of cirrus pouch with pars prostatica. The duct opens on the summit of the genital papillæ just below the opening of the female duct.

(b) *Female Organs*.—The ovary lies in about the middle line of the body somewhat nearer the ventral than the dorsal surface. It is a little cephalad of the acetabulum and posterior to both testes. It is oval, with diameter about 0.25 mm. The shell gland is smaller than the ovary and lies to its right postero-laterally. Its diameter is 0.18 mm. The oviduct begins in the narrow angle between the ovary and the shell gland. There is a large receptaculum seminis cephalad of the ovary. The uterus lies mainly near the dorsal surface, curving from side to side but restricted to a narrow zone about the sagittal plane of the body. A coil passes between the two testes on their dorsal aspect. The uterus is compactly filled with eggs, and opens on the top of the genital papilla caudad of the opening of the male duct. Laurer's canal commences dorsally by the ovary and passes directly caudad, opening on the dorsal surface by the body 0.17 mm. caudad of the ovary. The vitelline glands consist of two fairly compact masses each surrounding the corresponding intestinal cæcum. In pressed fresh specimens, the vitelline glands are confined to the posterior discoidal portion of the body. Their cephalad limit lies in the region of the anterior testis about its centre. Caudally the glands stretch to the level of the centre of the acetabulum. The outer border of the glands lies parallel to the lateral margin of the body and 0.5 mm. away

from it. The inner border is straight, crossing the intestinal cæca about their middle. Both these borders meet cephalad at a narrow apex. The caudal limit of the glands lies parallel to the border of the acetabulum and about 0.2 mm. from it. From each group of glands on either side a duct passes towards the middle line just caudad of the ovary. The two ducts meet in the angle between the ovary and shell gland. The ova are passed in the single cell stage with a large amount of vitelline material. The shell is fairly thick with a double outline. The ova are $152\ \mu$ in length, with a maximum diameter of $60\ \mu$. The eggs are distinctly operculate.

(c) *The Genital Cone*.—The papilla on which the genital tubes open is a very striking feature of the parasite and is seen distinctly with the naked eye. The papilla is broadly blunt at its apex where box-ducts open. In section the cuticle on the papilla is occasionally corrugated into folds which appear as a knob-like structure. In whole specimens cleared, the papilla is always seen to be smooth. The papilla projects about 0.2 mm. above the surface.

(V) EXCRETORY SYSTEM.

The elongated excretory vesicle lies wholly dorsal to the acetabulum. The excretory duct opens on the surface in the middle line at the caudal pole.

(VI) HABITAT.

Cæcum and large intestine.

(VII) HOSTS.

The parasite was first found in man and although from the beginning an herbivorous host was suspected none was reported till 1913, when Brau et L. Bruyant [2] found it in 5 per cent. of pigs in Cochin-China. The parasite in the pig was determined by these authors as identical with that found in man. They found in certain localities very small forms not exceeding 3 mm. in length; probably these were immature forms although the authors do not allude to the state of their sexual maturity. Professor Leiper has drawn my attention to the fact that in his paper (Leiper, 1913) [8], he erroneously attributed the recording of this parasite in pigs to Mathis and Leger. As this mistake has been copied into some text-books he has asked me to make the correction in this communication.

The Napu mouse deer is thus a new herbivorous host. It is difficult to decide which is the normal host of the parasite, but it is probable that man becomes infected more from the pig through the intermediary of a yet unknown molluscan intermediary host.

Manson-Bahr [13] suspects that species of the genera *Bullinus*, *Planorbis* and *Physa* may be intermediate hosts. He bases his opinion on analogy with members of the genus *Gastrodiscus*. In this he overlooks the fact that parasites belonging to different species of the same genus may have snail intermediate hosts belonging to widely different genera or even different families. This is quite evident from our knowledge of the intermediate host of *Schistosomum hæmatobium*, *Schistosomum mansoni*, and *Schistosomum japonicum*, for the snails which transmit these parasites are very widely different from each other. This statement of Manson-Bahr is to be regretted as it may lead some observers to confine their search to these particular genera of snails. In the

search for the intermediate host of *Gastrodiscoides hominis*, all local snails must be taken into consideration, and it is only local distribution of snails in relation to the prevalence of the infection either of man or animals that will afford any real help.

(VIII) GEOGRAPHICAL DISTRIBUTION.

The parasite has a very limited range. It is confined to the Malay States, Assam, Cochin-China, and India—practically south-eastern Asia. Cases reported from other parts of the world, e.g., British Guiana, are traced to immigrants from the endemic area.

(IX) PATHOGENESIS.

Very little is known regarding the effect of this parasite on man or animals. More observations are needed.

(X) TREATMENT.

The parasites are easily expelled by thymol given in the same manner as for hookworm.

REMARKS ON THE GENUS *Gastrodiscoides*.

In 1913 Leiper [8] proposed the genus *Gastrodiscoides* with the type species *hominis* and thus excluded this parasite from the genus *Gastrodiscus* in which it was formerly incorporated. The main points on which he based his new genus were: (1) tuberculated genital cone; (2) position of genital orifice; (3) smooth ventral disc; (4) testes in "tandem" position.

Stephens in "The Animal Parasites of Man," refers the parasite to the genus *Gastrodiscus*, although he alludes to the genus *Gastrodiscoides* in a footnote. Manson-Bahr [13] in Manson's "Tropical Diseases" (seventh edition, 1921) accepts the genus *Gastrodiscoides*, while Castellani and Chalmers [4] still adhere to the genus *Gastrodiscus*. In these three text-books the respective authors do not discuss fully the reasons which led them to adopt the particular attitude they have taken up.

In 1922 at a laboratory meeting of the Royal Society of Tropical Medicine and Hygiene, Clayton Lane [5] showed sections of a type specimen of *Amphistomum hominis* in which he discovered that the genital ducts open into "a genital pro-atrium in the shape of a saucer surrounded by a tuberculated area." This was supposed to dispose of one of the chief characters on which Leiper based his genus. I have examined fifty specimens with the aid of a lower power of the microscope and in every one I was able to see distinctly a protruding genital cone. The specimens were fixed in Schaudinn's fluid and preserved in 70 per cent. alcohol. In serial longitudinal section the apex of the protruding genital cone is found to be flattened and occasionally having a slight depression in the centre. An exaggeration of that appearance owing to different methods of preservation apparently explains Lane's "pseudo-sucker" but it may be noted that it is always at the top of the protruding genital papilla. Thus I think that Leiper is justified in taking this character as one of his differentiating points for this genus.

The question of importance is rather—Do the differences between the species *Gastrodiscoides hominis* and *Gastrodiscus ægyptiacus* suffice to place them in two different genera or no? My own opinion is that the genus

Gastrodiscoides is justified by the absence of pseudo-suckers from the ventral aspect of the discoidal portion and the presence of the genital pore on the elongated cephalic position of the body, but that Leiper's definition of the genus ought to be modified as regards relative position of the testes. These are not "tandem" but are definitely diagonal.

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Section of Tropical Diseases and Parasitology.

President — Sir LEONARD ROGERS, C.I.E., M.D., F.R.S., I.M.S.

Framboesia: History of its Introduction into India; with Personal Observations of over 200 Initial Lesions.

By ARTHUR POWELL, M.B.

THE history of yaws in India taken alone seems to me sufficient to prove that syphilis and yaws are two distinct diseases.

Syphilis has existed in India for centuries and is specially prevalent among the coolies recruited from distant parts of India to the tea estates of Assam. In the estates under my care from 7 to 10 per cent. of the population were syphilitic.

Although prevalent in the Dutch Indies, Malaya and Ceylon, no case of yaws had been observed among the three hundred million inhabitants of British India till in December, 1899, after a residence of one and a half years in Assam, I met my first two cases. From these two the disease spread by direct contact till in ten years I personally observed and treated in a narrow strip of land 22 miles long, by 4 wide, 653 cases in a population of about 6,000.

In 1887 a coolie woman came from Ceylon with three daughters, the youngest being infected with yaws. The other two girls in turn became infected, and were constant visitors to the lines in which cases were first seen by me. These four women called the disease "faranghi" while all the other coolies called it "The New Disease."

I left Assam in 1900 and lived in Bombay for nineteen years. During that time I travelled much in the country, where children run about naked up to the age of 8 or 9—the period at which yaws is most prevalent. The working classes are so scantily clad that an eruption like yaws can scarcely fail to be noticed by the most casual observer. Yet in all that time I saw only one case of yaws—a pilgrim from Sumatra on his way to Mecca. I had a large venereal practice, and all kinds of unsightly skin diseases were brought to me by the police in my capacity as Inspector of Lepers, but no other case suggestive of yaws was ever seen.

The map (fig. 2) shows the infected gardens under my care. The infected coolie from Ceylon lived at Konapara. The first cases seen by me were at Digabar, 2 miles away, where thirty cases arose in seven families before a case arose elsewhere. By 1901, there were 256 cases on this garden. In 1891 a woman with a yaws-infected child went from Digabar to Dankargul, about 17 miles distant, where fifty-five of a population of about 200 became infected. In 1893 a family went from Dankargul to Nuncherra, 2 miles

distant. Within a fortnight one of the children was found to have yaws. By 1899 of the 400 inhabitants sixty-five had contracted yaws.

In 1896 Mulagul garden which used the same bazaar as Dankargul, less than half a mile distant, became infected. In four years there were forty-seven cases in a population of 350.

A woman from Digabar came to Konapara in 1893. A week later Case I, her son, aged 4, was found to have yaws. He died of malaria a month later. No other case arose here till January, 1897, when Case II, a woman who frequently visited a paramour in Digabar, was found to have a large primary yaw on a gummatous ulcer of the right leg which was soon followed by a general eruption. Case III, her baby, aged 2, had been vaccinated on December 3, 1896. There was a primary yaw on the vaccine scar and a

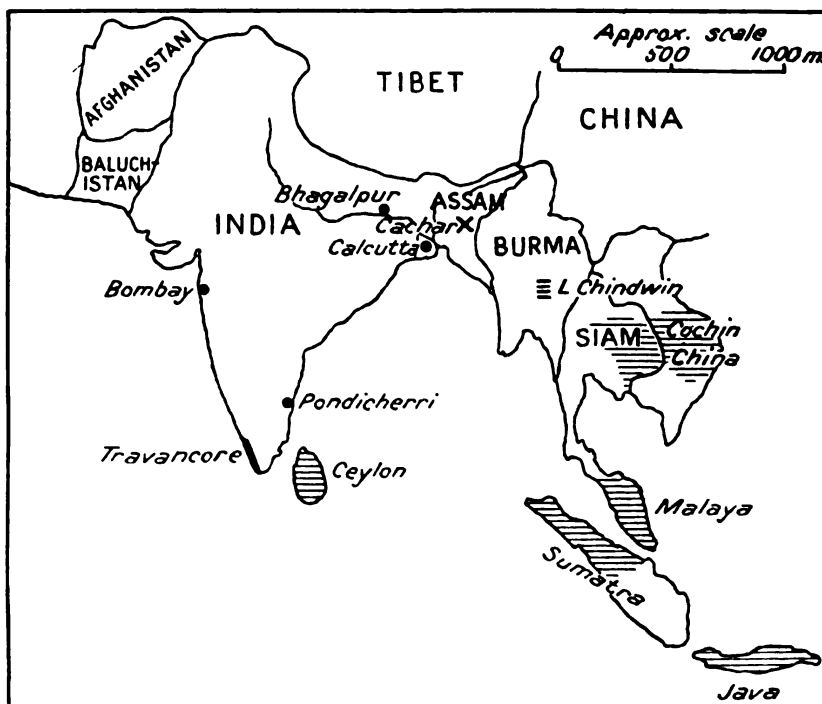


FIG. 1.—x = infected area in author's charge. — = other infected areas up to the year 1900.

general eruption followed in January. In five months' time all six members of her household were infected. Of 400 inhabitants, by the year 1900 seventy-two had yaws. Magenta, almost contiguous with Konapara, became infected in 1897. Fifty-three cases arose here.

In 1894 a woman from Digabar brought to Craigpark hospital Cases III and V, two children with a general eruption of yaws. I advised the manager to send her back, but she ran off. Four years later I recognized her at Barkhola where Dr. Chartres showed me some cases of yaws.

Early in 1896 the disease spread to Kauakauri, the nearest village to Digabar. From there it quickly extended to the almost contiguous garden, Hilara.

Case VI.—A lad, aged 16, introduced the disease into the village, not garden, of Kalaincherra. In January, 1897, he showed me a granuloma in size and appearance

Seventeen other cases occurred in this village. In four the initial lesion appeared on the site of ankylostome vesicles on the feet.

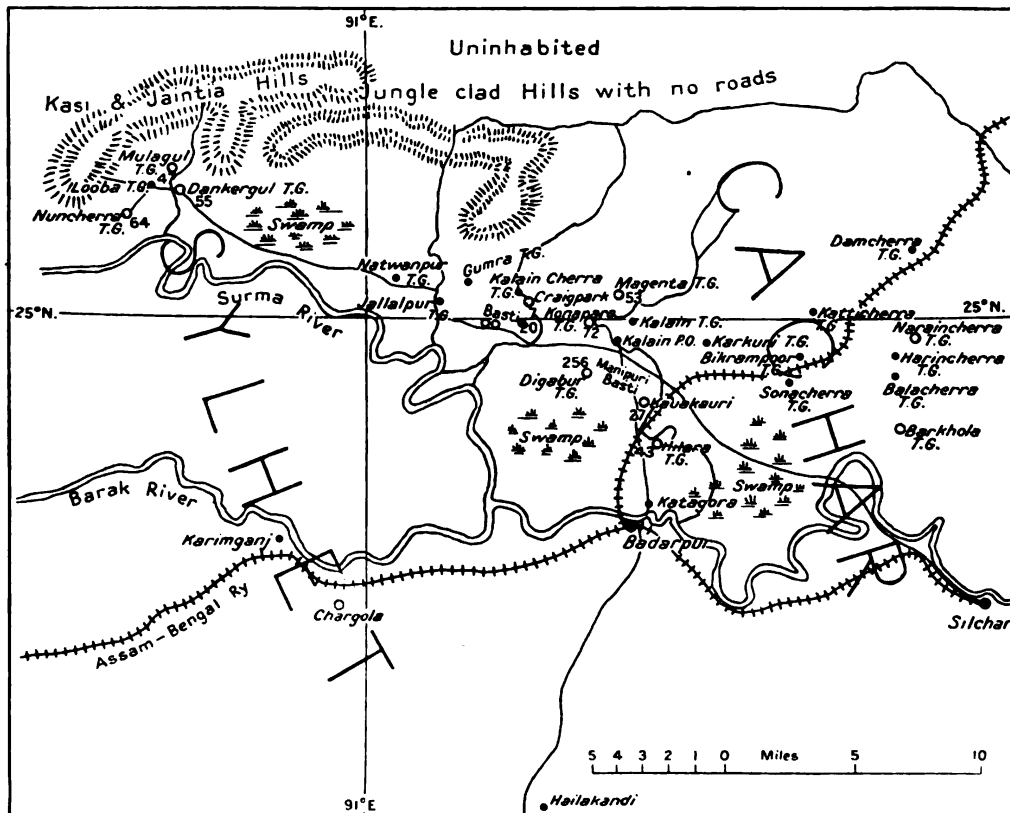


FIG. 2.—o = infected gardens or villages. Figures indicate number of cases to 1900.

In 1897 Dr. Chartres showed me some cases at Naraincherra and at Barkhola where I identified the mother of one case as a coolie from Digabar, 15 miles distant.

In 1900 Sir Walter Buchanan wrote me that he had admitted to Bhagalpur Jail a prisoner suffering from yaws from an Assam tea estate. The prisoner would not say from what garden he came.

In 1906 Powell Connor [7] reported cases to the south-west corner of

Manipur, the corner nearest to my district. I may point out on the map (fig. 2) the little Manipuri settlement between the three most infected gardens. Many hundreds of Manipuris came to me for surgical treatment from Manipur and lodged in these houses while waiting for admission to hospital.

I think all the above cases may fairly be looked on as originating from the Digabar focus.

Hirsch [17] says the sole reference to yaws in India previous to 1881 was made by Huillet [18]. Reference to the original "Hygiène de Pondichéry" shows that personally Huillet saw none, but some years before 1861, the late Monsieur Beaujean saw "une sorte de frambæsia, petites taches pointillées, larges comme une pièce d'un franc, ayant une teinte légèrement plus pâle que celle de la peau environnante." These were cured in a few days by the application of a solution of mercury perchloride. This may have been tinea versicolor, but all will agree that it was no sort of frambæsia.

In October, 1894, Maitland [23] reported two cases from Travancore, the furthest possible point in Southern India from Digabar. These were probably imported from the neighbouring island of Ceylon.

In December, 1894, Nolan [31] reported an epidemic in the Lower Chindwin district of Burma. Although as the crow flies this is only 300 miles from Digabar, I feel sure the two epidemics arose independently, as in those days there were no roads or communication between the two districts. Swamps, rivers, and especially the Lushai Hills inhabited by head-hunting savages, intervened. In 1906 M'Carthy [22] reported 431 cases in the Lower Chindwin district.

The Englishman reported that several cases of yaws among coolies returned from the West Indies had been landed in Calcutta in May, 1895.

In 1896 Pilgrim showed four cases of yaws, in coolie children returned from Fiji, to the Calcutta Medical Society, none of whose members had ever seen similar cases.

In 1904, Gouzien [11] reported cases in Pondicherry.

THE PRIMARY LESION.

Results on 205 Breaches of Surface continually observed till Yaws ensued.

The primary sore of yaws is not likely to be seen often by physicians practising in a large city. It is when a coolie has an extensive eruption that he comes for treatment. I was in a singularly fortunate position for observing the patients at, and even before, the appearance of the first lesion, as nearly all my patients were indentured coolies or their children. On each garden the coolies parade each morning at roll-call, when a native doctor is present to examine and treat all suffering from illness. Whenever a case of yaws appeared in a garden this doctor was ordered to register any ulcer, wound, leechbite or other breach of surface on all uninfected coolies, which might serve as a site of inoculation. Such breaches of surface were then reported to me and observed from day to day to see if yaws would ensue.

I have now notes of the initial lesion in 205 cases and have lost the notes of some hundreds of others. I will preface my observations by saying that not in a single case have I seen a primary lesion even remotely resembling the ulcers described by Numa Rat [37] or in the text-books of Byam and Archibald [4], Manson-Bahr [24] or Castellani and Chalmers [5].

In forty-three cases the first sign of the disease was a general eruption in from thirty-five to one hundred and twenty-one days, no change beyond

healing being observed of the breach of surface by which the *Treponema* was presumed to have entered.

Case IX.—On December 11, 1896, a girl, aged 15, came to live in lines where there were several yaws cases. She then had an ulcer on the right shin. This healed normally by February 1, 1897. On March 4, 1897, she had a general eruption as a first sign. Nothing suggestive of yaws was seen on the scar.

In seven of these forty-three cases, though no lesion could be seen at the site of presumed inoculation, an ordinary fungating granuloma appeared at a short distance proximal to it; others still more proximal followed in a centrifugal direction till finally a general eruption developed.

Case X.—A man whose wife and four children were suffering from yaws, cut his left index finger with a billhook; this healed in about ten days. On the forty-second day a small raised papule was noticed on the flexure of the left wrist. This grew into a fungating granuloma. A week later three began in front of the elbow: about the end of the second month there were five granulomata about the left shoulder, two over the left scapula. The eruption then became general and copious.

Case XI.—A boy, aged 7, while bathing, cut the sole of his left foot on a broken bottle. The wound healed with some suppuration in three weeks. A week later three yaws appeared on the inner side of the left ankle: then two below the knee: two months later the eruption was general. No yaw was ever seen on the sole.

Case XII.—A child, aged 3, whose two brothers had yaws, contracted itch on both hands and wrists, where there were many large pustules. This was promptly treated with sulphur. No yaw appeared on either hand or wrist, but a fungating granuloma developed four weeks later over the middle of the left radius: a week later four or five yaws appeared on the inner aspect of the elbow (a frequent site for the granulomata). The whole of the upper arm was dotted with yaws by the end of a month. Later the eruption became general. It is possible there may have been an itch burrow at the site of the initial yaw, but none had been observed or noted by me or by my assistants.

In 162 of these cases a raised fungating button, identical in all respects, perhaps with the exception of size, with those of the secondary eruption, appeared at the site of the presumed inoculation after an interval of from seventeen to sixty-eight days. In at least eight of these cases the secondary eruption appeared almost simultaneously.

A fungating granulomatous button takes some time to develop. If the breach of surface by which the *Treponema* entered has skinned over, the "baby" granuloma will be covered by cuticle and appear as a papule or small nodule. If the ulcer be still unhealed the first indication of yaws will be a raw granulation like a sentinel of "proud flesh." If the original wound is healing under a scab, removal of this will show the raised granuloma. I have never seen an initial yaw assume the annular form.

You will understand that when, without qualification, I use the term "button," "granuloma," or "yaw," with reference to the initial lesion, I mean a fungating papillo-granuloma identical with those of the secondary eruptions, and I shall not waste time by saying it began as a papule, or that it was small before it was large.

The crust covering the granuloma is often characteristic. It is at first tough, thin and strong, its outer surface dry, yellowish or greyish, and at first smooth, resembling the bit of parchment pharmacists tie over the cork and neck of a bottle (fig. 16, p. 37). Like this parchment it is firmly attached at its periphery and difficult to remove. Yet on removing the periphery from the skin

the crust is found to have little attachment to the surface of the granuloma. Usually there is little secretion from the raspberry-like surface, but occasionally as much as a drop of thin, whitish, sticky secretion is found under the crust. In situations like the palms, axillæ and perinæum, where skin is opposed to skin, there is often no crust. When the crust is removed from an actively growing yaw on an exposed position, a new crust generally forms. In other cases when the crust is removed or falls off, the granuloma may become of the mummifying type; it becomes dry, its large papillæ tend to separate and look warty (see fig. 12, p. 27).

In other cases the secretion from the underlying yaw thickens and dries on the under surface of the crust: or if the latter has been cracked or partly detached by scratching or friction, on both sides. In this way a more or less rupia-like scab may result, but on its removal a granuloma is exposed, more or less raised above the skin (fig. 14, p. 35). A rupial ulcer was never seen. When the yaw heals there is no scar left unless the granuloma has been subjected to trauma or caustic applications. If the patient's skin is darkly pigmented, a dark stain is left which may last for a year or two. The dark colour is explained by the great proliferation of the interpapillary layers which manufacture pigment in the dark-skinned races; when this hyperkeratosis subsides in the course of healing, a superabundance of pigment is left behind and takes a long time to become absorbed. If the patient be of light complexion no excess of pigment is formed, and the remaining "tache" is pale. If the granuloma has been subjected to caustics, scraping or other trauma, there may be a white scar due to the destruction of tissue. Some authors say that the primary yaw is distinguished from the secondary eruption by its white scar. This is sometimes true, but the scar is due to the ulceration which allowed the implantation of the *Treponema*, not to the granuloma set up by the *Treponema*. If the site of inoculation has been a puncture or wound that healed without much suppuration, the site of the initial yaw after healing is pigmented just like that of the secondary eruption.

Case XIII.—This woman slipped and fell, smashing an earthen jar she was carrying under her right arm. A fragment cut the inner surface of the arm. The primary granuloma is in all respects like that of a secondary eruption (fig. 3).

Case XIV.—Photograph (fig. 4) is that of a woman who fell backward on a newly pruned tea-bush. A chisel-like branch stabbed her in the left loin. This wound had just healed when on the twenty-fourth day a yaw appeared on the scar. A secondary eruption followed in about six weeks. You will see very little difference between the primary yaw and the half dozen large secondary yaws. Note the papules or "baby" yaws on the inner aspect of the right elbow. These grew into yaws resembling the primary.

Case XV.—A boy, aged 10, was bitten by a leech on the left calf, January 3, 1897. This bite became inflamed and irritable. The boy scratched it repeatedly till a small ulcer resulted. January 31, 1897: A papule was evident, which increased and became a large fungating button. A general eruption of smaller granulomata followed in May. In June, 1897, Mr. G. Darby made a life-size sketch of the initial granuloma, which was then 28 mm. in diameter (fig. 5). Later it reached a diameter of 4 cm. The secondary granulomata for the most part became annular.

Case XVI.—A villager came to a cock-fight at Konapara. A cock spurred him on the right calf. Seven weeks later he showed me on the scar of the wound a primary yaw, almost an exact copy of Case XV.

Case XVII.—This woman, while pruning a tea-bush, cut her right index finger (December 27, 1897). Thirty days later the cut had healed, but a fungating yaw was

seen on the site of the scar. Photograph (fig. 6) was taken on the fifty-sixth day, when the commencement of the secondary eruption can be seen as a papule on the flexure of the left elbow. On April 4, 1899, she gave birth to a healthy boy.

Case XVIII.—Her husband tripped and cut his right shin on a sheet of corrugated iron, April 2, 1898. The wound suppurated, and at the end of the month was the size of a halfpenny, its lower half being covered with very tall granulations. By June 1, the whole wound was covered by a cauliflower-like granuloma overlapping the healthy skin "like the top of a champagne cork." A secondary eruption appeared in July. When last seen, January, 1900, the secondary eruption had healed, but the primary granuloma persisted as a rather dry, warty growth.

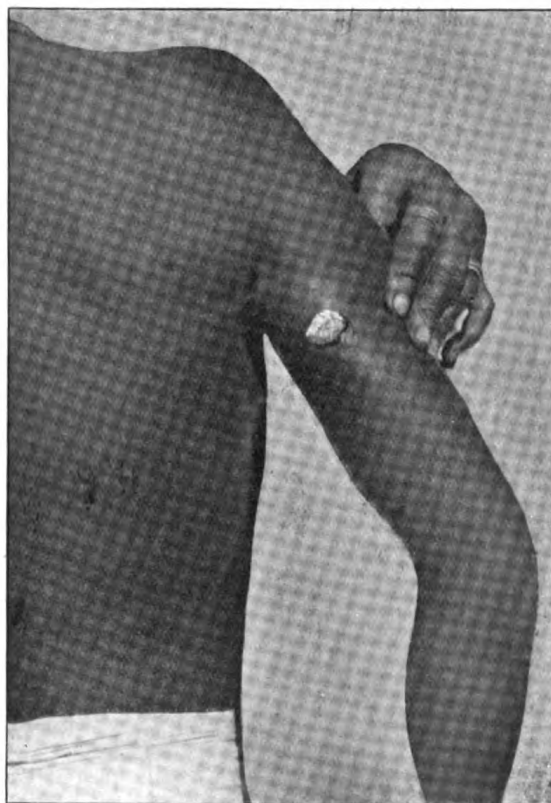


FIG. 3.—Primary yaw.

Case XIX.—A woman, all of whose three children were suffering from yaws, was wounded on the left side of the neck by some falling debris during an earthquake on June 6, 1897. The wound was in a position likely to be touched by the children's arms, on all of which there were many granulomata. On June 28, 1897, she was admitted for "acute rheumatism." This was, of course, the initial fever of yaws. I first saw her on July 2, 1897, and observed a small papule on the healed wound. This had grown into a circular granuloma 2 cm. in diameter, 8 mm. above the skin surface in September, 1897, when I excised it as a typical initial lesion, and sent it to Sir Jonathan Hutchinson. A general eruption broke out on July 23, 1897. On March 23, 1899, she was delivered of a healthy child, which remained so till last seen, February 1, 1900. The mother still had a few yaws on the back and thighs.

Case XX.—This lad had herpes of the second left intercostal nerve. You can see the pale scars running down the course of the intercosto-humeral nerve, as far as the wrist. Later a yaw developed on the scar of a vesicle on the forearm. In photograph (fig. 7) some papules are seen developing on the chin and cheek. A general eruption ensued. Two years later all the granulomata had disappeared, their sites being indicated by dark pigmented patches—a striking contrast to the white scars of the herpes.

Case XXI.—A woman, aged 27, two of whose children had yaws, had a whitlow of the left index finger incised by a hospital assistant on July 14, 1895. Photograph (fig. 8) shows the primary lesion of yaws on September 15, 1895. A general eruption did not appear till about October 15, 1895.

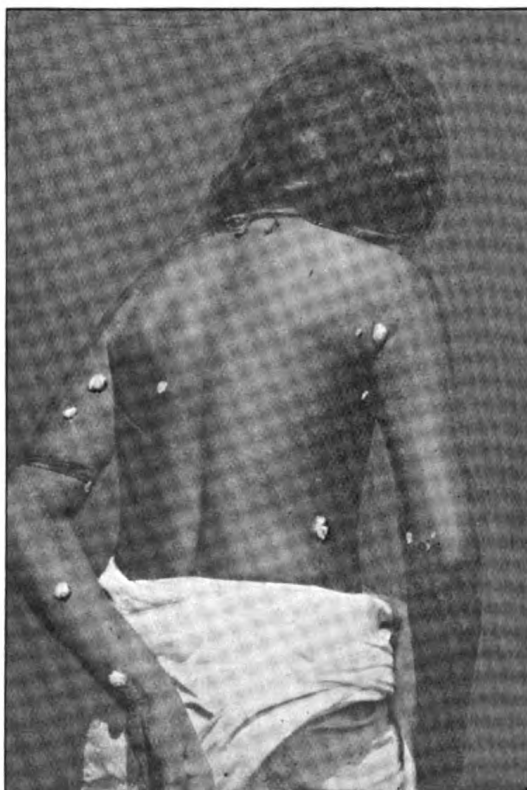


FIG. 4.—Primary yaw on left loin.

Case XXII.—A girl, aged 5, ran a thorn into her left palm. Her father, who is shown with an eruption of yaws, holding her hand, extracted the thorn with his fingers. The photograph (fig. 9) was taken on the twenty-third day, when a papule had formed, too small to be well shown. In time it became a raspberry-like yaw, and a general eruption followed six weeks later.

In several of these cases the primary yaw became very much larger than any of the secondary yaws. In most cases it lasted as long as the secondary eruption and in not a few it was the last to heal. My best photographs of such yaws have faded, but Henggeler's [16] Plate VIII, shows a primary yaw 3 in. in diameter, very similar to cases I have seen. His Plate VII shows another on the chin. His Plate III one near the left ankle.

In a few cases the primary yaw receded after a few days, but returned, perhaps simultaneously with the general eruption.

The Satellite Type.—A primary yaw may appear and enlarge. A ring of



FIG. 5.—Primary yaw.

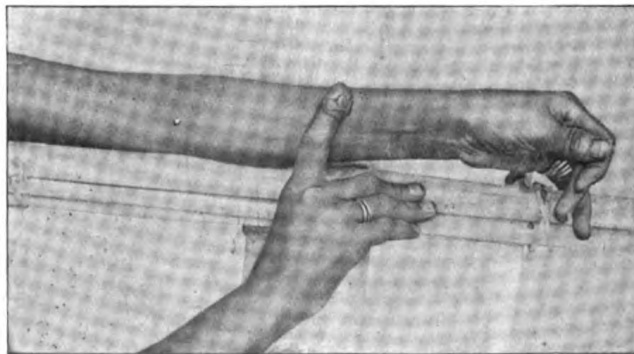


FIG. 6.—Primary yaw on left index.

satellites may form round this. The little constellation may possibly be the only sign, but as a rule it is succeeded by a general eruption.

Case XXIII.—A girl, aged 6, fell cutting her right eyebrow on a brass *lotah*. The primary yaw appeared thirty-two days later; within a fortnight it was surrounded by

a halo of twenty-four smaller yaws. A general eruption followed in about a month. In photograph (fig. 10) several of the satellites have coalesced.

Case XXIV.—A girl, aged 16, had her name tattooed on the left forearm on March 18, 1898. On May 1, 1898 I found a yaw the size of a pea developed on what should



FIG. 7.—Primary yaw on scar of herpes.



FIG. 8.—Primary yaw.

have been a letter. Around this there were thirty-four papules scattered in a circle nearly 3 in. in diameter. With two exceptions these were not on the tattooed lines and were therefore not the result of external inoculation. In August the central planet was as big as half a hazel nut, the satellites either from absorption or coalescence were reduced in number to twenty-six and enlarged in size to diameters of from $\frac{3}{8}$ to 8 mm. There were about a dozen small yaws elsewhere on the body.

Case XXV.—A healthy female baby, whose mother and two brothers were then in full eruption, showed a small pea-sized granuloma at the navel twenty-one days after birth. This completely healed in about ten days after treatment with silver nitrate. When the baby was eight weeks old, the granuloma recurred and increased till it was the diameter of a threepenny-piece. By the end of the third month a halo of

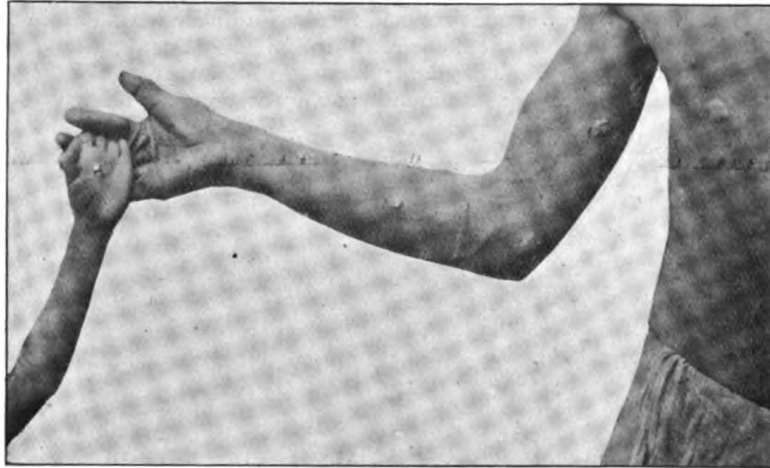


FIG. 9.—Primary yaw on child's palm : general eruption on her father.



FIG. 10.—Primary yaw on mother's breast : satellite type on girl's left brow.

twenty-two pea-sized granulomata formed with the navel as the centre. The whole presented the appearance of a symmetrical target, $2\frac{1}{2}$ in. in diameter. For over two months no other general eruption appeared. This is the youngest case of yaws I have seen.

Case XXVI.—A woman, while carrying her child slung on her back, fell backwards

and the child received a punctured wound on the left buttock from a stump of a newly pruned bush. The mother who had a general eruption of yaws, repeatedly wiped off the blood with her fingers. About four weeks later she noticed several small yaws around the site of the wound. When photograph (fig. 11) was taken there were twenty-four almost contiguous yaws on this site and only thirty-six elsewhere on the body.

Case XXVII.—Fell on a cement floor and received a linear wound an inch long on the right eyebrow. Photograph (fig. 12) shows nine almost confluent granulomata which preceded the general eruption. They later became still larger and quite confluent, surviving the general eruption by some months, as a warty growth off which individual



FIG. 11.—Primary yaw on mother's breast: satellite primary yaw on child's buttock.

dry papillæ were often knocked. The ultimate scar was no larger than the original wound.

If yaws be inoculated on a sloughing or very septic ulcer it is impossible to say what the resulting picture will be. We had epidemics of tropical phagedæna in the district, but fortunately few cases in the yaws-infected gardens. In Looba, among 1,500 coolies, over 400 had phagedæna in the year 1896.

I show photographs of a few of these ulcers, but it should be remembered that they have no more to do with yaws than a cut throat or a shrapnel

wound ; either will serve as a site of inoculation. One cannot foretell what will happen to such ulcers. They may heal slowly or they may slough till tendon and bone necrose. I have even seen the limb fall off. If such a sore be inoculated with yaws it would be absurd to describe the ultimate condition—healing or gangrene,—as the “primary sore of yaws.”

Cases XXVIII and XXIX.—Two patients, each with an ulcer about the size of a half-crown, stayed some nights in a house where children had yaws. In one the phagedænic sore, the presumed site of inoculation, was now a crater 7 in. by 4 in. exposing on its floor the necrosed tibia. Nothing like a granuloma was seen on its margin, which was a smooth, indolent white line about 3 mm. wide. In the second case the ulcer was healing, the peroneal tendons had sloughed, and a glazed,



FIG. 12.—Very old primary satellite, primary yaws on right brow. All the eruption is dry, warty, uncovered by scabs.

indolent surface secreted much thin clear fluid. The margins were white and showed no eminence or granuloma suggestive of yaws. Each case had a general eruption.

Case XXX.—Female, aged 22, with a stationary phagedænic ulcer on the right shin, became mistress of a yaws patient. Three months later a fungating tumour the size of an acorn was present on the margin of the ulcer. The ulcer was otherwise very much as it had been three months previously. Later a general eruption ensued.

In many cases if the site of inoculation be a chronic ulcer, the primary yaw is very large, covering the whole surface of the ulcer and overhanging the margin “like the top of a champagne cork.” Such granulomata usually survive all the general eruption.

Case XXXI.—Female, aged 50, had a large carbuncle on her back which after much sloughing left a large irregular ulcer. In April, 1896, a large fungating yaw appeared on its margin; this became confluent with some smaller satellites on the edge of the carbuncle. For two years a secondary eruption existed and then disappeared, but the primary yaw was still present in January, 1900, as a kidney-shaped excrescence, 35 mm. by 20 mm., with dryish projecting papillæ standing 12 mm. above the skin, the whole looking like a split fig. The scar of the carbuncle was puckered and white.

Case XXXII.—The boy shown in photograph (fig. 13), came to the infected lines in Konapara in 1897. He then had an ulcer, possibly phagedænic, larger than a five-shilling piece, on the ventral aspect of the left leg, a little above the ankle. The



FIG. 13.—General eruption : primary yaw above left ankle.

initial yaws lesion consisted of a partial ring of coalesced granulomata round the upper, inner and lower circumference of the ulcer. At the end of three years, the secondary eruption had healed, leaving pigment marks, but there was still a large granuloma at the distal side of the ulcer. The scar of the ulcer was white. Note the femoral bubo in this case and in No. 53—in both the result of septic absorption from the ulcer.

The rapidity with which the disease can spread when appropriate sites for inoculation are present is illustrated by a family of seven, who lived in one of a group of huts near Digabar.

Case LIV, female, aged 5, and *Case LV*, female, aged 1½, were vaccinated on April 1, 1895. When first seen on May 15, 1895, both had large cauliflower-like yaws on the vaccine scars. On June 15, 1895, both had a general eruption.

Case LVI.—Their father had a primary yaw the diameter of a threepenny-piece on his scrotum where he had been bitten by a leech seven weeks earlier.

Case LVII, female, aged 10; *Case LVIII*, male, aged 7; *Case LIX*, female, aged 8, all had extensive scabies. On July 13, 1895, the boy had a well-defined granuloma above the left anterior superior iliac spine where several itch pustules had been chafed by his waist-string. *Case LVII* had a pea-sized granuloma on the web between her second and third left fingers. *Case LIX* had a fungating primary yaw in the left gluteal fold. In all a secondary eruption followed. Six of the family of seven were attacked within three and a half months.

Case LX.—The mother, aged 35, escaped till, in August, a carbuncle developed on her nape. In September on the site of the carbuncle was an ulcer about the size of a sixpence overhung by a granuloma which later became cauliflower-like and of the diameter of a half-crown. Secondary eruptions followed and waned, but the primary granuloma persisted for nearly three years, becoming in its later stage of a dry papillomatous nature.

Case LXI.—Female, aged 10, sister of *Case IX*. A small abscess below the left patella burst about December 22, 1896. On February 14, 1897, a primary yaw the size of a large pea had appeared, this grew to a large circular cauliflower-like growth 20 mm. in diameter. A general eruption followed, many yaws on the neck, chest and abdomen becoming annular.

Case LXII.—Aged 16; the eldest sister, had an abrasion-contusion over the left inner malleolus on February 2, 1897, which took about ten days to heal. On March 8, 1897, a papule was seen which grew to a granuloma about 15 mm. in diameter. In May she had a copious general eruption. Both she and the patient in *Case IX* had considerable fever and joint-ache both before the primary yaw and the general eruption.

Case LXIII.—Brother, aged 8, drove a splinter of wood under the nail of the left middle finger on March 4, 1897; a whitlow resulted and was incised on March 12, 1897. On April 15, 1897, a fungating mass uncovered by any crust was overlapping the nail. A general eruption was present on July 1, 1897. The younger sisters, *Case LXIV*, aged 5, and *Case LXV*, aged 7, frequently evaded inspection, but both were found on June 24, 1897, to have a general eruption of yaws, and an extensive eruption of scabies. I was unable to identify a primary yaw in *Case LXV*. There was a large granuloma between the third and fourth left toes of *Case LXIV*, which the family said preceded the others. In less than five months all six children were infected. The mother was the only member of the family to escape.

Case LXVI.—Patient stubbed his left second toe on a splinter of wood which his sister, *Case II*, extracted on March 3, 1897. A small granuloma was seen on this site on April 14. Later it formed a large cap covering the tip of the toe. In June the eruption was general.

Case LXVII.—His wife, aged 16, had itch, which was cured by sulphur by the middle of April. On May 1, she had fever and pains in the joints, followed by a general eruption, without any initial yaw.

Case LXVIII, female, aged 6, and *Case LXIX*, male, aged 8, children of *Case II*, when first seen had scabies. In the case of the girl the first yaw appeared above the right knee very little in advance of the general eruption. In that of the boy a raspberry-like yaw appeared between the right fourth and fifth toes about five weeks in advance of the general eruption.

In the next hut but one dwelt six people—

Case LXX.—A boy of 12, had a traumatic ulcer on the left inner malleolus, the size of a half-crown. A month later the lower border of this ulcer became beaded with raised granulomata which coalesced into three large yaws. In March 1897, a general eruption developed.

Case LXXI.—His brother, aged 9, got a thorn into his left sole on April 2, 1897, which his brother (*Case LXX*) extracted: cellulitis followed. On May 1 he had a painful swelling suggestive of an abscess; the cuticle over this ruptured and a granulating base, looking like an ulcer, was exposed, surrounded by raised sodden cuticle. A week later, after poulticing, this cuticle was peeled off, a typical raised yaw being exposed. Three months later he had a copious general eruption.

Case LXXII.—Their baby brother in June showed a primary yaw on the plantar surface on the right great toe. No wound or puncture had been observed. About six weeks later he had a general eruption, the lips, as is often the case in sucklings, being severely affected. In October—

Case LXXIII.—His mother, showed a fungating granuloma at the base of the left nipple. No previous lesion had been seen on this site, but probably a fissure had been overlooked. In December she had a sparse general eruption. The only inhabitant of the house to escape was the father.

In a house 20 yards away—

Cases LXXIV and LXXV, two boys, developed a general eruption without any primary sore as far as I could observe. Their brother (*Case LXXVI*) fell, cutting his knee; this healed slowly, but thirty-two days later a small yaw was observed on the scar. Later it grew to the size of a big walnut. A general eruption followed.

Case LXXVII.—In the next house a female child, aged 5, had her right thigh severely scratched by a thorn. Forty-five days later a primary yaw formed. In two months a general eruption followed. Her parents and a brother were not affected.

Case LXXVIII.—Female, aged 14. Was first seen to have two granulomata on the right arm, one on the left, which all arose simultaneously. Fresh yaws gradually appeared till about thirty were present.

Case LXXIX.—Her elder sister three months later showed a typical yaw above the anterior superior spine of the left ilium as a result of inoculation on a belt of pustulating ringworm. In a house 50 yards away—

Case LXXX, a boy, aged 12, fell and wounded his right knee; this healed in a fortnight. He eluded observation for six weeks, but on the fifty-second day a granuloma 6 mm. in diameter, raised 3 mm. above the surface, was noted. A general eruption followed.

Four other dwellers in the hut escaped infection.

Case LXXXI.—Male, aged 10 months, was found to have a general eruption of yaws. I could recognize no initial lesion nor did his relatives allege any.

Case LXXXII.—Patient's mother got an abscess of the right breast. A sinus resulted and this was not healed when a granuloma the size of a pea was seen, uncovered by cuticle or scab. This rapidly enlarged and a general eruption was out a month later.

Case LXXXIII.—Male, aged 16, son of previous case; a general eruption on the arms and forearms was first seen September 1, 1897. No primary lesion was recognizable, but the patient attributed the disease to scratching an eruption of prickly heat.

In a line of six huts about 100 yards away—

Case LXXXIV.—Male, aged 8, was bitten by a leech on the left ankle on June 16, 1897. The bite inflamed and suppurated; before it healed on August 3, a granuloma was observed on the site. A general eruption followed.

Case LXXXV.—Male, aged 12, his brother, while riding a buffalo, was accidentally gored on the right flank. A large lacerated wound took two months to heal. A primary yaw was first recognized on the granulating wound on the fifty-sixth day after the accident.

Case LXXXVI.—Their grandmother had an initial yaw on the scar of a boil between the scapulae.

Case LXXXVII.—Their mother next had a general eruption in which neither she nor I could recognize an initial yaw.

THE LYMPHATICS.

A general enlargement of the glands was not noticeable in any of my cases except in those complicated by recent syphilis. In cases like Nos. XXXII and LIII where the initial yaw was implanted on an ulcer or septic wound the corresponding glands were often enlarged, soft and painful.

AUTO-INOCULATION.

In eleven cases I have taken the secretion from a papule or small granuloma and used it as in the method of vaccination with cow-pox on the patient from whom it was taken. In no case did any positive result occur.

In reviewing the literature on the subject of the primary lesion, we should do well to ignore authors who have not personally seen what they describe and to note that with exceptions like Castellani [5], Jeanselme [20], Halberstaedter [13], Neisser [29], and Henggeler [16], few have special knowledge of dermatology.

The description adopted by so many text-books, "an ulcer with perpendicular edges and a granulating base," is so tautological as to bear evidence that it did not originate with more than one observer. It means "an ulcer with an ulcerating base," as it is the function of all ulcers to granulate. Possibly "granulating" was originally a misprint for "granulomatous," an adjective which would convey some meaning.

Numa Rat [37] describes the initial lesion as: (1) "An ulcer with perpendicular edges and a clean base." (2) When the contagion has entered through a granulating wound or an ulcer the primary lesion is a "mass of granulated tissue similar to that of the tubercles of the secondary stage."

Numa Rat's second, and according to my observation, correct description is overlooked by most authors. The first was fastened on by Sir Jonathan Hutchinson [19] and has been copied from text-book to text-book though it is not based on Rat's own observation, but on the hearsay evidence of his unscientific negro patients. His cases "illustrating the initial local lesion" are only two: "Case I, Present Disease, Dermatitis of Sole." The previous history given by a negress patient supplies the description of the initial lesion at least seventeen months previously, i.e., before Dr. Rat's arrival in Dominica, as stated in his preface. "Case II, Present Disease, Dermatitis of the Sole," seen by Rat in July, 1890. The negro patient in September, 1881, nearly nine years before Dr. Rat saw him, "noticed a small pimple on the edge of the prepuce. On the top of this pimple a small water vesicle formed, and the pimple ulcerated and formed an ulcer about the size of a florin. The ulcer was red and clean." Is it likely that a negro suffering from large cauliflower excrescences would be dermatologist enough to recollect and describe a pimple he had eight or nine years previously?

In the *Journal of Tropical Medicine*, 1904, Rat [37] describes three initial lesions all of the fungating type.

How many authors who quote Charlouis's [6] "scientific" inoculations have read his essay? The primary sores which he caused were not the result of treponemata only, but also of an "X" quantity of variegated filth, containing numerous pyogenic microbes. His *modus operandi* was to make a pocket 1 cm. deep below the skin with a bistoury and then fill it with scab

or blood expressed from an ulcerating yaw. He wrote in 1881, before the days of antiseptics. You can easily imagine the result if you bury in the tissues a chunk of scab which has possibly existed for months on the dirty skin of a yaws patient. "Gleich nach der Operation entstand Reaction, die sich als eine ausbreitete Röthe mit lokaler Temperaturhöhung zeichnete." Pustules with scabs resulted, on the removal of which painful ulcers were exposed; on the floor of these were papillary outgrowths which became "magnificent fungating tumours." One ulcer was 3 cm. deep. On this ultimately developed a fungating tumour. The patients complained of painful swelling of the neighbouring glands: the whole area was intensely painful from the ulcers to the armpit, so that the weight of the shirt was unbearable. Rigors and fever often accompanied the first symptoms. Many of these ulcers produced fungating tumours, but those which did not left a whitish, thick, hard scar. Is it necessary to point out that the fungating tumours and the secondary eruptions were the result of the *Treponema*, and the ulcers, phlegmons, rigors, &c., the result of the staphylococci and streptococci inoculated? He inoculated two Javans on one breast with yaw scab, on the other with acne pus. Similar pustules resulted on both sides. It seems strange that Charlouis's cases should be quoted as typical of the initial lesion of yaws when transmitted naturally.

Charlouis was an honest observer. I confess I find Paulet's [32] work here and there suggestive of a page from De Rougemont or Munchausen. He induced more than 230 healthy negroes to have connexion with negresses suffering from yaws. Thirty negroes showed him "une belle éruption" twenty-five to fifty days after connexion, their genital organs remaining intact. More than 200 showed tubercles, some on the body, some on the genitals.

Jeanselme [20] describes the initial lesion as a papilloma, "une sorte de furoncle dur, couronné d'une croûte, sous laquelle se développe une ulcération tenace dont les bourgeons exubérants font dans la suite une saillie en forme de choufleur. L'accident initial au niveau de la porte d'entrée ne diffère pas des éléments qui apparaissent ultérieurement."

Neisser [28], to whom I sent photographs and related my observations on the initial lesion and failure to find tertiary signs, wrote on his return from the Dutch Indies that his observations were identical with mine. He inoculated two gibbons and five *Macacus* monkeys with yaws. In each case there developed at the inoculated spot "on a slightly infiltrated base a moderately thick, honey-yellow, very characteristic crust, on the removal of which an easily bled, well-developed papilloma was exposed exactly like the frambæsia eruption in man."

Halberstaedter [13] continued Neisser's experiments with like results on a total of eleven monkeys, two gibbons and an ourang-utan. On the ourang a general eruption followed. Henggeler [16], an accurate observer who worked on the plantations in Sumatra from 1896 to 1903, has written an excellent article, beautifully illustrated. He says: "On any little wound or epidermal defect which is the source of entrance, the granulations begin to grow rapidly; on the sore a fungose tumour of variable size, more or less raised above the skin level, develops. The edge of the sore is never undermined, never clean cut (scharf abfallend)." Though the growth was sometimes slightly infiltrated, he has never found any hardness that would give the slightest resemblance to a hard chancre. He is emphatic that he diligently searched for, but never found, (a) any prodromal furfuraceous eruption, (b) any affection of the mucosa, (c) any tertiary signs.

The careful observations of these distinguished investigators are worthy of more attention than the crude experiments of Charlouis [6] and the childhood's recollections of Dr. Rat's [37] illiterate negroes.

Of writers on tropical diseases generally, Scheube [41], whose practice was in Japan, had little opportunity for personal observation. He follows Charlouis's [6] description of the primary sore with which I have already dealt. He, then, on the authority of Bestion [2] describes a second form of initial lesion as a vesicle increasing in size from a pin-head to a 20-centime piece; around this new vesicles form and run together, while a papule develops from the central vesicle. I have never seen anything like this.

In Bestion's original paper [2] we find he describes a disease in the Gabon called "aboukoué" which has an incubation period of seven weeks, followed by fever lasting a month. At the end of the second month severe pains in the joints and bones are invariable. At the beginning of the fourth month a vesicle the size of a pin-head appears and would scarcely be noticed except for its itching which causes much scratching. At the end of ten to twelve days, the vesicle has become a bulla the size of a 20-franc piece, surrounded by a halo of new vesicles. An ulcer results, its base becomes irregular and ultimately fungates. My view of this "tandem" disease is that the bullæ are the *terminal* stage of "aboukoué" on which the *initial* yaw is inoculated in the process of scratching.

Manson-Bahr [24] says that "the primary lesion may appear as an isolated papule or a bulla developing a few days later into an undermined ulcer with a raw base." Like Neisser [29] and Henggeler [16] I have never seen anything in the least resembling this.

Castellani's descriptions vary greatly [5]. In his and the late Dr. Chalmers' book (p. 1542), it is said: "If after some days the crust is removed the primary sore will appear as an ulcer, not rarely of large dimensions, with clean cut edges and a granulating fundus." In "The Practice of Medicine in the Tropics," edited by Byam and Archibald [4] (vol. ii, p. 1314), he says the "primary sore appears in the form of an ulcer of varying and often considerable size with a granulating surface." These we may take as deference to the "classic" description, for in his own experiments on monkeys the primary lesion was a papule, and in his description of tropical phagedæna (p. 2185) he says it often "becomes infected with frambæsia virus, takes a papillomatous appearance and is followed by a general eruption of frambæsia granulomata." His last description on page 1321 is perfect: "The primary lesion does not differ from the elements of the generalized granulomatous eruption."

Halberstaedter [13] says, "Die Krankheit beginnt mit einer primären Effloreszenz an der Infektionsstelle, welche sich von den späteren Effloreszenzen des Eruptionsstadiums kaum unterscheidet." Brocq [3] says the initial lesion appears "sous la forme d'un bouton isolé, siègeant au point précis où l'inoculation du virus s'est faite." He adds that Henggeler describes the initial lesion as occasionally a pustule. I can find no such statement in Henggeler's paper [16], but having said that the majority of the primary lesions among his own bare-legged coolies naturally occur on the lower extremities liable to leech-bites, cuts and scratches from jungle thorns and spear-grass, he adds that they may occur on "irgendeinen Epidermisdefekt. In anderen Fällen entwickelt sich die Muttereffloreszenz aus einem 'Eiterbläschen' (wie in Falle III), oder aus einem kleinen Furunkel die wegen Jucken gekratzt und dabei wohl infiziert worden waren."—"Owing to its itching the pustule gets scratched and in this way inoculated." Describing Case III he makes a

similar statement. "Die Infektion auf natürlichem Wege setzt als Vorbedingung das Bestehen eines Epidermisdefektes voraus, der in irgendeiner Wunde am Fusse einem offenen Eiterbläschen (Fall III) einer Schrunde an der Brust bestehen kann" (p. 259).

Nicholls [30] thinks a general eruption is often the first sign, but "should there be any growth at the site of inoculation it differs in no essential particular from the characteristic eruption."

Nattan-Larrier and Levaditi [27] as the result of inoculating a chimpanzee produced an "ulceration with a granular and budding base." I may here remark that many French authors use the word "ulcère" when describing the secondary "boutons." For example, Rochas [38] speaks of "Ulcères saillies, en bourrelet"; Paulet [32] of "ulcères, convexes, mamélonnés."

THE GENERAL ERUPTION.

Though in all cases I have diligently searched for it, I have never found a prodromal furfuraceous eruption.

In health the young coolie's skin is usually well washed and oiled, giving it a smooth and glossy appearance. Should he fall ill from any fever, malarial, influenzal, frambæcial or other—his skin loses its gloss. Planters often use the language of the stable, saying a coolie must be ill "because his coat is staring." Tinea versicolor is widely prevalent. Jeanselme [20] says when there is much fever in yaws the skin "devient rude et perd son lustre."

Since the above was written Napier in the *Indian Medical Gazette*, December, 1922, contrasts the signs of kala-azar with those of other fevers. He found "roughness of the skin" in 141 of 258 cases of kala-azar, and in 49 of 141 cases of other fevers. In a total of 382 cases of all fevers 190, practically half, 49.73 per cent., showed roughness of the skin.

Goodman [10], Henggeler [16], Neisser and his colleagues [28], Baermann [1], and Halberstaedter [13], all say they have searched for, but never found, any furfuraceous eruption.

As a rule in children there is little fever or joint-ache either at the beginning of the initial sore or of the general eruption. In my adult cases the proportion of those suffering from fever and osteocopic pain was smaller than appears the rule according to text-books.

The general eruption begins in the form of small papules about the size of pin-heads. Some of these may abort, the distended cuticle dropping off as a small scale. This is the only approach to desquamation I have seen. The papules which persist grow in height and breadth till we get the button-like granulo-papilloma above described. The covering crusts are similar to those of the primary lesion.

In many cases the granuloma assumes an annular form. In rare cases this may be formed by the coalescence of smaller granulomata, but in the vast majority it is caused by outgrowth from the periphery of a button while healing takes place in the centre. In a few cases the crust covers the healed centre as well as the ring of active granuloma.

An attack of continued high fever such as pneumonia, measles, small-pox or remittent, often causes the eruption to recede temporarily, and in rare cases to disappear altogether.

Case XXXVIII illustrates these last two points. In February, 1897, an initial button appeared on the site of an abrasion in the left popliteal space.

Photograph (fig. 14) shows the general eruption in an early stage in May. Photograph (fig. 15) taken in December shows several of the yaws covered by a rupia-like scab.¹ When these scabs fell off rings of granuloma enclosing healthy skin remained, resembling those shown in photograph (fig. 17). In January, 1898, he had left lobar pneumonia. Twelve days from its onset the granulomata had completely disappeared, their sites being marked by dark pigmented patches. In January, 1900, neither scarring nor pigmentation remained, the skin being perfectly normal. In March, 1898, his wife gave birth to a healthy daughter.

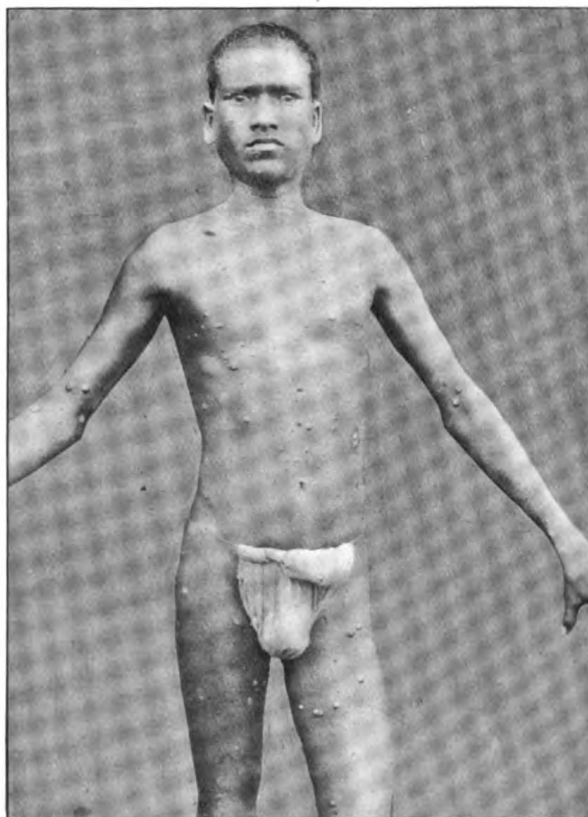


FIG. 14.—Early general eruption covered by thin parchment-like scabs.

In Case XXXIX an initial yaw appeared on an ulcer near the right ankle. Photograph (fig. 15) shows the general eruption in the early button stage. Photograph (fig. 16), taken ten weeks later, shows many of these buttons have assumed the ring form, while many new papules are seen, especially on the lower limbs.

PLANTAR GRANULOMATA.

Photograph (fig. 18) shows a case of three granulomata on the sole looking like ulcers. Some authors describe these as deep ulcers, forgetting the great thickness of the plantar cuticle of those who walk barefoot. These yaws are

¹ Sir Jonathan Hutchinson, to whom I gave a copy of this photograph, reproduced it as a case of rupial yaws in the New Sydenham Society's Fasciculus 14 (fig. 11).

generally painful, so that the patient is constantly poulticing and fomenting his feet, causing the cuticle to become sodden and swollen. In this example I peeled off the cuticle, which over the heel was eleven millimetres thick. On its removal the granuloma was seen to stand 7 mm. high above the cutis, though at first its upper surface looked like the floor of an ulcer 4½ mm. deep.

TERTIARY SEQUELÆ.

With the exception of three cases, in two of which yaws was contracted by syphilitic patients, I have never seen any tertiary signs that could possibly be attributed to yaws.

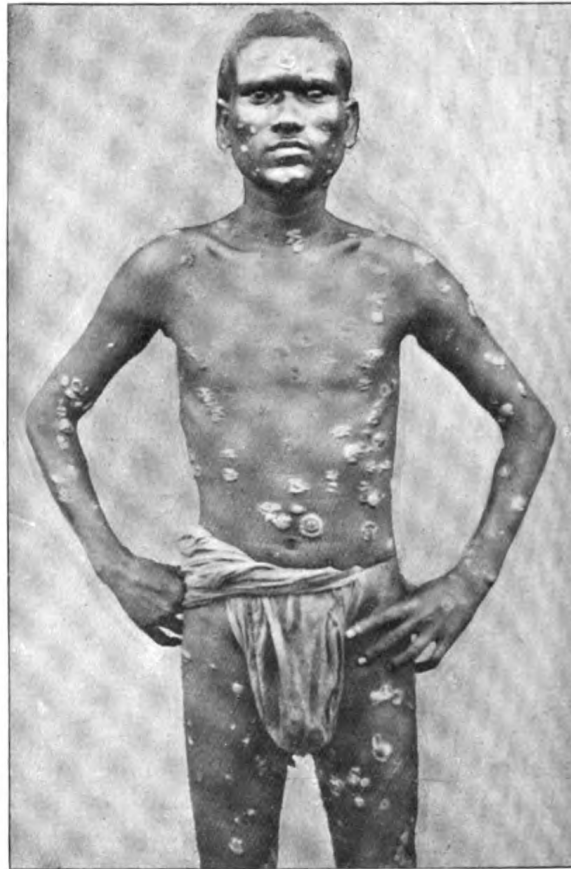


FIG. 15.—A later stage of fig. 14.

I ask you not to attach too much value to my evidence on this point as the disease was new in my district, and with the exception of the family who introduced it from Ceylon, no patient was under my observation more than ten and a half years after inoculation.

Case XXXVII.—S., female, aged 28, had yaws when 18. An abortion was brought on by a coolie midwife, who thrust a stick into the uterus. Severe septicæmia with secondary abscesses set in, and all the larger joints became swollen. When last seen in 1901 the patient was emaciated, almost crippled and unable to straighten the knees. I attribute her condition to septicæmia, but others may regard it as a sequel of yaws.

The other two cases I published in the *Indian Medical Gazette*, 1898, and sent photographs and a fuller account of them to the late Sir Jonathan Hutchinson. His errors in their description in the New Sydenham Society's "Atlas" and the *Polyclinic* necessitate my giving the history of the family in detail:—

Case XXXIX.—B., male, aged 7, living in yaws-infected lines, cut his right knee on July 2, 1894. A papillomatous button appeared on the scar by the end of the month and a general eruption of yaws followed.



FIG. 16.—Early stage of ring-shape eruption, shown in fig. 17.

Case XL.—S., his sister, aged 16 months, was vaccinated on September 7, 1894. In October a large fungating yaw was found on the vaccine scar. A general eruption of yaws followed, and lasted eighteen months. The facies of this child, fig. 25, p. 18, New Sydenham Society, Fasciculus 14, is typical of syphilis as diagnosed by Sir J. Hutchinson. It shows the bridge of the nose sunken, the frontal bosses prominent and scars at the angles of the mouth. Osteitis of the phalanges set in in November, 1897, when the child was aged 4 (fig. 16, New Sydenham Society, Fasciculus 13).

Case XLI.—R., aged 83, their mother, had an abscess of the breast incised, December 5, 1895. Early in February, 1896, an initial yaw appeared on the resulting sinus. Photograph (fig. 10, p. 25) shows the initial lesion and secondary eruption. Another photograph shows a ring of yaws surrounding the mouth of the child. Simultaneously

with her child, S., osteitis of the phalanges set in in November, 1897. With mercurial inunction and iodide of potash the osteitis in mother and child quickly subsided.

Case XLII.—A male child was born on April 15, 1898. Contracted scabies, November, 1898. In January, 1899, a primary yaw appeared between the right index and ring fingers. A general eruption followed and lasted till death from dysentery in November, 1899.

F., daughter, aged 6. *Case XXIII* above, developed a primary yaw on a wound of the eyebrow (fig. 10, p. 25).



FIG. 17.—Later stage of eruption shown in fig. 16.

Case XLIII.—B., father, cut his left index, July 5, 1895 (photograph shown). In twenty-two days a primary papule was noticed which grew into a large cauliflower mass and a general eruption ensued.

Case XLIV.—J., female, aged 8, the remaining child contracted scabies. A raspberry-like papilloma appeared in February, 1898, on the site of an itch pustule in the left groin and in May was followed by a general eruption.

In his comments on these cases Sir Jonathan Hutchinson fell into many errors, e.g., he attributes the case to Daniels, not to me. He says the child was 18 months old when the photograph was taken. She was $4\frac{1}{2}$ years old. He

says the mother had suffered from yaws when pregnant of this child, whereas the child was $2\frac{1}{2}$ years old when she definitely inoculated her mother with yaws on a breast abscess. His contention is that yaws is only syphilis, and that in these cases the granulomatous eruptions and the osteitis or dactylitis are both symptoms of syphilis. We all agree with him that the child has inherited syphilis. Father and mother therefore were certainly syphilitic, and some if not all the sisters and brothers probably syphilitic. Is it possible to have a more convincing proof than the history of this family that yaws and syphilis are separate diseases conferring no mutual protection?

Adopting Sir Jonathan's diagnosis we have (1) a probably syphilitic boy,



FIG. 18.

aged 7, contracting a primary chancre of "framboesoid syphilis" on a wound of the knee. (2) Inoculating his definitely syphilitic sister, aged $1\frac{1}{2}$, with a primary chancre on a vaccine sore, who in turn with her lips covered with secondary eruption inoculates (3) her certainly syphilitic mother with a "chancre" on the sinus of a breast abscess. (4) The mother bears another son who is certainly the inheritor of syphilis, yet again, when 8 months old, develops a primary "chancre" on an itch pustule between the fingers. (5) The father, certainly syphilitic, cuts his finger and develops a second primary "chancre" on his index finger. (6) A daughter, aged 6, probably the inheritress of syphilis, contracts a chancre on a wound of the eyebrow. (7) Another daughter, born

in 1890, and therefore most probably inheritress of syphilis, when aged 8, contracts a primary chancre on an itch pustule. All seven develop a severe granulo-papillomatous secondary eruption in their "second attack of syphilis" and never show any other form of syphilide.

Cases in which syphilis was inoculated on patients while suffering from yaws, were J., aged 40, and his wife, M., aged 30:—

Case XLV.—B., their eldest son, aged 10, was first seen by me in August, 1891, when he had a general eruption of yaws said to be of three months' standing. The rest of the family was at once inspected for either yaws or a breach of surface likely to become inoculated. I found:—

Case XLVI.—M., the mother had a cracked and pitted form of plantar hyperkeratosis. About three weeks later an initial yaw was evident on a fissure in the flexure of the left great toe. A general eruption ensued and lasted over two years.

Case XLVII.—B., aged 6, another son, had a small cut on the right calf which had been present at least ten days. On September 15, 1891, he had an initial yaw on the scar. A general eruption followed and lasted about a year.

Case XLVIII.—J., the father, was under treatment for itch. At the beginning of October, 1891, a primary yaw was seen on the left wrist and a general eruption followed. In June, 1892, he was found to have a hard chancre on the prepuce. In September a maculo-papular syphilide appeared and co-existed with the yaws eruption till his death in 1898 from dysentery, probably aggravated by mercurial poisoning at the hands of a *Kobiraj*. As soon as J.'s chancre was seen, his wife was inspected at intervals, till in December, 1892, a chancre was found on the left labium. Mercury was given at once. Two months later the lymphatic glands generally were hard and enlarged. In April, 1893, a papulo-squamous syphilide appeared and only lasted a few weeks. The yaws eruption lasted till June, 1894.

Case XLIX.—Bh., aged 8, another son, was wounded by the fall of a heavy stone on the right instep. Thirty-six days after the accident a primary granuloma appeared on the scar. A general eruption of yaws followed. Only one child of this family escaped infection with yaws.

Case LII.—P., a woman, aged 22, living in a house with her mother, sister, brother-in-law, and two nephews who all had yaws, got a severe scratch on the right thigh in May, 1898. An initial yaw developed on this site and a general eruption followed. In October, 1898, I found a snail-track condition of the fauces, and later ulcers on the tonsils. She then informed me she had for the past four months been mistress of S., one of my own syces. On examination a healing chancre was found on the fourchette. Her paramour had a hard chancre in January, 1897, and had been treated by me for secondary syphilis and double iritis.

Of syphilitic cases who became infected with yaws I may mention Case II, the woman cited above who reintroduced yaws to Konapara. The large primary yaw appeared on a gummatous ulcer. Her two-year-old child, doubtless the heir of syphilis, developed a primary yaw on a vaccine scar.

Case I.—S., female, aged 14, had a hare-lip operation in 1890, when I entered on her case sheet, "inherited syphilis. White streaks radiating from the angles of the mouth, sunken nose, teeth irregular, but scarcely characteristic." A gumma broke down on her shin in July, 1896, when there were three cases of yaws in her line. On August 2, 1896, she was admitted to hospital by my locum tenens with an initial yaw on the site of the gumma. She remained under my treatment till December, 1897.

Case LI.—Her father, blind from iritis with occlusion of pupils since about 1882, had been under my care for nodes and gummata. In December, 1898, he had a vesiculo-pustular eruption on both feet, doubtless due to ankylostome larvæ. In January 1899, he had a primary yaw below the left inner malleolus, followed by a general eruption. He died of ankylostomiasis in December, 1899.

Case LIII had a gumma on the right leg which broke down leaving an ulcer. Some months after a primary yaw appeared on the ulcer. In spite of vigorous cauterizing with nitric acid a general eruption followed. Some of the scarring is due to the nitric acid.

Castellani describes a cracked and pitted form of plantar hyperkeratosis as a sequel of yaws. This form of hyperkeratosis is very common in Assam, where the coolies call it "pachwari." It is not rare elsewhere in India.

I show you a photograph of a case in Bombay, where yaws does not exist. I cannot in any way regard this condition as due to yaws. I have seen it in hundreds of Indians who never had yaws, and I have seen four of these patients develop a primary yaw on a fissure or pit on the sole and some others become infected with yaws elsewhere. Another photograph shows a similar condition of the palms which had existed for years. A sparse eruption of yaws has just broken out on the trunk.

GANGOSA.

In Assam and in Bombay I have diagnosed several cases as tertiary syphilitic destruction of the face, which were regarded and even published by my colleagues as "Gangosa." None of them ever had yaws. All were in races among whom yaws had never been seen. I show a photograph of a Mussulman Bengali who had had syphilis seven years previously. He was treated with iodides and calomel fumigation; the necrosing bones were removed, and the result is seen in the photograph. The first Bengali to have yaws was the doctor at Nuncherra, who in 1899 contracted the initial yaw on a "hang-nail" on the right ring finger. His two children and a Bengali servant were infected later.

HEREDITY.

I have never seen any case of hereditary transmission. With the exception of *Case XXV*, the youngest I have seen was seven months old, and, as well as I can recollect, in all children below the age of eighteen months the primary lesion and the breach of skin on which it was inoculated were identified. I have seen seventeen healthy babies born of parents who had yaws during pregnancy. They remained healthy, except for malaria, or dysentery in two cases. With the exception of scabies there was no rash in any case. I show four photographs of children from 8 to 16 months old, all of whose parents had yaws when the children were born. They all look "bonny," and might serve on a patent food advertisement.

Goodman [10] and Schüffner [42], by means of the X-rays, have seen bone or periosteal changes at the onset of the general eruption. If this be so, it would not be surprising to find bone lesions later on. The absence of tertiary changes in my cases may be exceptional, yet much of the literature describing tertiary yaws is very unconvincing and much of its logic deplorable.

With many it is an axiom that there is no syphilis in Fiji, *therefore* all nodes, gummata and destructive lesions of bone *must* be due to yaws. Thirty years ago I had a patient who assured me he had contracted syphilis from a Fijian woman. A quarter of a century ago there were in Fiji about ten thousand Indian coolies of the same class and from the same districts as those of Assam. I cannot believe they were all free from syphilis. Thirty years ago missionaries were deploring the spread of syphilis in Fiji. Its prevalence was one of Mr. Ghandi's arguments for the abolition of the Indenture Acts. Nearly a quarter of the population twenty years ago was made up of Indians,

Europeans, Chinese, Polynesians and half-castes, these last a proof of the frequency of inter-racial intercourse. Finucane says syphilis is extremely common among the coolies of Fiji, that the Fijians are very immoral and have ample opportunities of contracting syphilis. "Typical cases of hereditary syphilis are seen," also "eruptions in Fijians which, if seen in a European, he would have pronounced syphilis." But obsessed by the "axiom" he calls them yaws! Most of the photographs of "Tertiary Yaws Lesions in Fiji" I have seen are those of Indian coolies.

Harper [15] agrees with Daniels that syphilis is absent, and *therefore* the few cases of tabes he has seen must be due to yaws! Maxwell says the tertiary signs of yaws he saw were "in those suffering from leprosy or the *malus corporis habitus*," which seems to me to be a polite term for syphilis.

Rat [37] describes as tertiary yaws "severe forms of ulceration resembling excedent lupus" which, "as they generally occur in the scrofulous, usually present the appearances common to that disease." Why, then, not call them lupus or scrofula rather than tertiary yaws?

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Section of Tropical Diseases and Parasitology.

President—Sir LEONARD ROGERS, C.I.E., M.D., F.R.S.

The Lung Flukes of the Genus *Paragonimus* : A Demonstration.

By G. M. VEVERS, L.R.C.P.Lond., M.R.C.S.Eng.

(Beit Research Fellow.)

THE genus *Paragonimus* contains all the mammalian lung flukes of America and the Far East. At present there are five known species:—

		Host.	Geographical distribution.
(1)	<i>Paragonimus rudis</i> Diesing 1850...	Brazilian otter ...	Brazil
(2)	„ <i>compactus</i> Cobbold 1859 ...	Indian mongoose ...	India
(3)	„ <i>westermanii</i> Kerbert 1878	Tiger, leopard, man ...	India and Malay
(4)	„ <i>ringeri</i> Cobbold 1880 ...	Man, dog, pig, cat ...	Japan
(5)	„ <i>kellicotti</i> Ward 1908 ...	Dog, cat, pig ...	America and Malay

Paragonimus rudis was found in a Brazilian otter. We know very little about it as the description by Diesing is short and it has never been found since 1850. Its occurrence in the New World however is suggestive that it is identical with *Paragonimus kellicotti*.

Paragonimus compactus, although described so long ago as 1859, was not found again until two years ago when I collected it from the lungs of the Indian mongoose, the type host, at a post-mortem at the Zoological Gardens; since then it has been found in the same host on two other occasions, also at the Zoological Gardens.

Paragonimus westermanii.—The type of the genus was first described in a paper by Kerbert in 1878.

Paragonimus ringeri was described by Cobbold in 1880 from a specimen coughed up by a Portuguese in China and sent to him for diagnosis by the late Sir Patrick Manson. This was the first record of a lung fluke occurring in man.

Leuckart and others considered that the two last flukes were identical and therefore the name *Paragonimus ringeri* was dropped for a time in favour of *Paragonimus westermanii*.

In 1894 a lung fluke was found in North America. At first it was thought to be imported from Asia, but the subsequent discovery of the fluke in the dog and pig in various parts of the States proved that a lung fluke was endemic in the New World. This fluke for some time was considered by Ward and others to be a variety of *Paragonimus westermanii*. However

in 1908 Ward described it as new and named it *Paragonimus kellicotti*, after Kellicott, who first discovered it in the United States.

Up till 1915 all the workers on the genus had relied on the shape, size and position of the internal organs as specific characters. Although these characters may be of great use in dividing up other Trematode genera into species such as the genera *Opisthorchis*, *Heterophyes* and others, there is so much variability in the internal organs of individuals of any one species of *Paragonimus* that one cannot rely on these points to any great extent.

In 1915 Ward and Hirsch in the U.S.A. discovered an easier way of differentiating between the species. They found out that the cuticular spines which are present in all these species differ in shape, size and arrangement in a constant manner.

From the material I obtained at the Zoological Gardens I have been able to show that the cuticular spines of *Paragonimus compactus* differ from the spines of the other members of the genus and also that the arrangement and shape of the spines in this species are constant in a large number of individuals, thus confirming Ward and Hirsch's view that the shape and arrangement of the cuticular spines are the only reliable characters upon which one can split up this difficult genus.

If one takes the sizes of the eggs of various human cases of paragonimiasis given by different authors one finds that they vary in size from $70\ \mu$ by $45\ \mu$ to $100\ \mu$ by $60\ \mu$. This range of size is far beyond that of the variations in the eggs of any one particular species.

Hitherto it has been supposed that man in the East is parasitized by only one species of lung fluke—this has been called *Paragonimus ringeri* by some and *Paragonimus westermanii* by others. A comparison of the eggs of the four best known species brings to light several obvious differences in contour, thickness of shell and size. It is therefore certain that more than one species of *Paragonimus* occurs in man—probably three: *Paragonimus ringeri*, *Paragonimus westermanii*, and *Paragonimus compactus*. In connexion with this it is interesting to note the geographical distribution of the different species.

The object of this demonstration is: (1) To show the value of the cuticular spines in the diagnosis of the different species. (2) To make a comparison of the eggs of the four species showing the chief points of difference. (3) To demonstrate adult flukes of the species *Paragonimus westermanii*, *Paragonimus kellicotti*, *Paragonimus compactus* taken from their various hosts in the Zoological Gardens of London.

Life-history of the Gape-worm.

By R. J. ORTLEPP.

(ABSTRACT.)

THE exhibitor showed a series of preparations illustrating some hitherto undescribed stages in the life-history of *Syngamus trachealis*, the common gape-worm of chickens; these preparations included some sections of the lungs of chickens showing the parasite in development, on successive days after infection, until their appearance in their final habitat, the trachea.

The demonstration was preceded by a short account of the work previously done on this parasite.

Life-History of the Sandfly, *Phlebotomus papatasi*.

By Wing-Commander H. E. WHITTINGHAM, R.A.F.M.S.

(ABSTRACT.)

THE author detailed the life-history of the sandfly, *Phlebotomus papatasi*, and illustrated his remarks by a large number of specimens and drawings. He stated that the Royal Air Force Sandfly Fever Commission has shown that the virus of phlebotomus fever was transmitted from generation to generation of *Phlebotomus papatasi*. This may be effected in two ways, either it is transmitted by heredity or the larvæ infect themselves in the breeding grounds by eating the excreta or the dead bodies of the parent flies. It follows that prophylactic measures against the fever must be directed upon the fly, the habits and habitats of which must be understood.

The life-history of *Phlebotomus papatasi* covers a period of about six weeks, the exact time depending on conditions of temperature and humidity. The ovum, which measures 0.385 mm. in length by 0.12 mm. in breadth, in its development shows certain changes in its surface markings and on the ninth day caudal bristles appear. A few hours later the shell breaks on the dorsum by the action of the egg-tooth. The larva emerges and enters on its first instar, lasting six days and ending with the first moult. In all, the larva passes through four instars of about six days each; and four moults, during which there occur not only increase in size but developmental changes characterizing each period, such as the disappearance of the egg-tooth, the appearance of two and then four caudal bristles, and of one and then two dark pigment bands on the dorsum of the terminal segments of the body. With the conclusion of the fourth instar the larva enters on the pupal stage, lasting about nine days, before the emergence of the imago. The wings of the newly hatched fly are crumpled and moist. Until these dry the young fly can only crawl. The recognition of this stage is most helpful in detecting the breeding spots of the insect. During the night, when the atmospheric humidity is usually great, the wings cannot dry. The process of drying is generally completed within three hours after dawn, and the mouth parts harden to allow of the sucking of blood during the first twenty-four hours of adult life. Copulation can take place within the next twenty-four hours, and eggs to the number of forty are laid six to ten days later. The length of life of the adult female fly in nature is about two weeks, though in the laboratory life may be prolonged for thirty days or more. It should be noted that, in the summer, only a third of the life of the insect is spent in the imago stage.

It has been found possible, as in the specimens shown of the living larvæ and adult flies, to prolong the larval stage considerably by the retarding effect of increased moisture or lowered atmospheric temperature. Hibernation of the insect occurs in the fourth larval stage. Several larvæ have been kept in this stage for six months, and then, by reducing the amount of moisture and incubating at 80° F., pupation has occurred. Fully-formed imagines eventually hatched out of the pupæ.

Case of *Spirochætal Dysentery*.

By W. BROUGHTON-ALCOCK, M.B.

(ABSTRACT.)

THE author demonstrated a series of slides showing intestinal mucus crowded with *Spirochæta eurygyrata*, fæcal matter from another case showing *Spirochæta stenogyrata* and a third specimen showing *Spirochæta eurygyrata* in the mucous portion and *Spirochæta stenogyrata* in the fæcal portion of the same stool. He discussed the question as to whether these are two forms of one and the same organism or two separate species of *Spirochæta*, saying that authors appear divided upon this question. He then continued as follows:—

Miss Hogue, in America, has recently published a very interesting article in the *Journal of Experimental Medicine*, December 1, 1922, describing cultures of *Spirochæta eurygyrata* but the illustrations drawn are, unfortunately, few, and are unlike the *Spirochæta eurygyrata* as I have always seen this organism, and as illustrated by Le Dantec 1903, Fantham and others. Unfortunately, also, there is only complimentary reference to the narrow coil type of Werner seen in the fæcal portion of many normal stools and obviously non-pathogenic, since it is never found in the mucous portion nor characterized by association with a dysenteric, diarrhœic or catarrhal colitis condition. As to the pathogenicity of *Spirochæta eurygyrata*, my experience leads me to believe that it can produce a catarrhal condition with the passage of mucus containing shed degenerated epithelial lining cells, occasionally red blood cells, and, what is extremely rare such a case as was seen in a first attack with typical dysenteric symptoms and passage for three weeks of much blood with non-purulent mucus. The mucus was teeming with *Spirochæta eurygyrata* and no other ætiological cause could be found on repeated microscopical and cultural examinations. There is always the argument that a primary agent has produced a vulnerable surface over the organism and acts symbiotically. This remains unanswered. Observations have been made on more than 20,000 stool examinations and neither my assistants nor myself have found the *Spirochæta eurygyrata* unassociated with mucus in which it is contained. The condition as seen in Britain is a chronic one with a mild degree of general symptoms. The main disability is the persistent or intermittent slight looseness and increased number of mucus-containing stools and the occasionally noted onset of acute symptoms with mild intestinal pain following dietary indiscretion or a so far undetermined cause. The infection is not common nor has an epidemic character been observed. I have observed only one case in England; the endemic areas appear to be in tropical and subtropical countries.

A patient, a case of such infection, has kindly presented himself this evening. He, a Fellow of this Society, is a healthy and robust medical practitioner. He stated that in 1917 he had, when in the Balkans, a moderately severe diarrhœic attack with passage of mucus and accompanying feeling of malaise. No amœba and no bacillus of the enterica or dysenterica groups was found. Occasionally, after dietary imprudence, such as too heavy a dinner, a recurrence of the symptoms lasting three to five days follows. Much mucus is then passed and this I have found packed with the large-coil *Spirochæta eurygyrata*. No other organism to which pathogenicity could be attached was found. He has cleared up the acute attack by taking a small dose of calomel nightly for three or four nights, followed by saline in the morning.

I think there is insufficient evidence in the publication to accept Dr. Fantham's interpretation from his observations cited in the *British Medical*

Journal, June 10, 1916, to which members are referred for an excellent revision of the question and references. It was unfortunate that Dr. and Mrs. Fantham were not in England to record their observations in South Africa. I have not been able to follow any transformation of form, and to my observation the narrow coiled *Spirochæta stenogyrrata* possesses much less motility, less defined border, is narrower, more frequently bowed and bended than the more sturdy active *Spirochæta eurygyrrata*, is found only in the fæcal portion of the stool, and is not infrequently seen in a normal stool. While the possibility of morphological variation according to activity or environment is not denied, evidence of such appears to me as yet insufficient for the *Spirochætæ* to be classified as of one species, as against the original classification of Werner.¹

Attempts to infect mice with the *Spirochæta eurygyrrata* have been unsuccessful. A slide showing *Spirochæta bronchialis* is also exhibited so that the *Spirochætæ* may be compared.

Remarks upon a Photograph of an Endemic Focus of Bilharzia Disease in Portugal; Specimens of the Intermediary Host, *Planorbis dufourii* (Graells).

By J. B. CHRISTOPHERSON, C.B.E., M.D.

IN December, 1921, Dr. Carlos França, of Collares, Lisbon, reported cases of bilharzia disease occurring in the south of Portugal; he found an endemic focus in the Atalaia (St. Luzia) quarter of Tavira—in the province of Algarve. He sent me a photograph of the infected spot and specimens of the intermediate host, both of which I am showing to-night.

It was discovered that at Tavira only washerwomen (with the exception of a boy, aged 13) were infected, and only those washerwomen who washed clothes in a certain tank, formed by water retained in a natural excavation in the limestone (6·3 metres by 7·3 metres); the water being constantly renewed by a thermal spring is always at a temperature of 25° C. At least sixteen out of the sixty-four washerwomen who used this tank were infected with vesical bilharzia.

The thermal tank is inhabited by a snail of the species *Planorbis dufourii* and the snails were found by Dr. França to be infected with the cercaria of bilharzia disease. The women who remain standing washing clothes in the water for many hours a day are infected in the usual way, that is, through the skin. They in turn help to complete the necessary biological cycle by micturating into the water whilst at work and in this way reinfect the snails.

The tank possibly received its original infection either from a washerwoman returned from the North of Africa (Morocco) or from a soldier in the adjacent hospital returned from Portuguese West Africa (Angola) or Portuguese East Africa (Mozambique), in all of which places *Schistosoma hæmatobium* is present.

It might be thought that the infection of the tank occurred in consequence of the temperature of the water (25° C.). This is not so, for in December, 1922, Dr. Carlos França recorded another focus of infection in Portugal, at Alportal, a short distance from Tavira. Here the river was infected, and the temperature of the water when taken was 13° C. Both men and women were infected at Alportal.

¹ *Centralbl. f. Bakt.*, 1909, orig., 1 Abt., lii, pp. 241-243.

With regard to the intermediary host the parasite of Portuguese bilharzia disease is *Schistosoma hæmatobium* and the intermediary host *Planorbis dufourii* (Graells). In Portugal no species of the *Bullinus* genus of molluscs is to be found, and it would appear that *Schistosoma hæmatobium* has adapted itself to a snail belonging to the genus *Planorbis* which it has selected in the absence of *Bullinus*.

Portugal is the only country in Europe definitely known at the present to be endemically infected with bilharzia disease; other foci of infection in Europe will doubtless be found, especially in countries bordering on the Mediterranean.¹ It would appear that possible intermediary hosts for *Schistosoma hæmatobium* are widespread in Europe.

Specimens from a Human Case of Infection with *Dientamæba fragilis*, Jepps and Dobell, 1917.

By ANDREW ROBERTSON, M.B.

(Assistant in Protozoology, London School of Tropical Medicine; Grocers' Research Scholar.)

THE patient, from whom the material was derived, had been resident in London since 1919, but had lived abroad for some years before that date. The only history of previous intestinal disorder was in 1919, just before he returned to this country, when he had a diarrhoeic attack, which cleared up with the evacuation of an *Ascaris lumbricoides*. In spite of the fact that the patient's fæces had been examined repeatedly at irregular intervals no protozoa were noted until the present occasion. Towards the end of January of this year he had an attack of diarrhoea associated with some form of food poisoning or dietetic indiscretion, and in the semifluid stool numerous free *Entamæba coli* were found. This stool was submitted for further study within about ten minutes of being passed, and, in addition to the free *Entamæba coli* already mentioned, there proved to be present a fairly heavy infection of *Dientamæba fragilis*, numerous *Blastocystis hominis* and a very few ova of *Trichuris trichiura*. In two subsequent stools, one of which was almost constipated in character, the *Blastocystis* and *Dientamæbæ* were still present.

In all six specimens are shown. Nos. 1 to 4 are binucleate individuals and show the marked differentiation between the clear, hyaline ectoplasm and the more granular, vacuolated endoplasm in which various bacterial inclusions can be made out. The nuclei are of the characteristic type, being spherical in shape, vesicular and having very thin nuclear membranes. The nuclear chromatin appeared to be arranged in the form of a ring of granules, most frequently five in number, towards the centre of the nucleus.

No. 5 is a binucleate *Dientamæba* at an early stage in the process of degeneration. There is a ring of cytoplasm, thicker towards one pole, and including the two nuclei, surrounding a large vacuole filled with homogeneous material.

No. 6: This specimen is a uninucleate individual, and its characters, except for the fact that it has only one nucleus, are similar to the binucleate forms above described. The proportion of uninucleate to binucleate individuals in this case was about one to four.

No cystic stage of this parasite has been made out.

¹ It is doubtful whether Greece is endemically infected. Cyprus is infected, but it is not in Europe proper.

Section of Tropical Diseases and Parasitology.

President—Sir LEONARD ROGERS, C.I.E., M.D., F.R.S.

The Establishment of an Antirabic Institute in the Tropics

By A. E. HAMERTON, C.M.G., D.S.O., Lieutenant-Colonel
R.A.M.C.

THE fact that Pasteur Institutes are now features of preventive medicine wherever the dread disease of rabies prevails is, perhaps, one of the most noble tributes to the genius of Pasteur.

In the tropics, hitherto it has been customary to establish antirabic institutes in the temperate climate of hill stations often far removed from centres of population in the endemic areas of the disease.

During the expansion of our civilization in the East and in Africa, and under the conditions of military service, it is not unlikely that the treatment of rabies will sometimes have to be undertaken on the plains, where most torrid conditions of climate are present. Such was the case in Mesopotamia.

During the Great War and the subsequent rebellion in Irak considerable loss of service and expense to Government were caused by the lack of local facilities for the treatment of rabies. Between 200 and 300 men had to be sent every year to the Pasteur Institute at Kasauli for antirabic vaccination. In addition, the unavoidable delays in the long journey from up-country stations in Irak resulted in the death from hydrophobia of a number of soldiers who—arriving in India too late for effective treatment—succumbed to this disease *en route* to Kasauli, or soon after arrival there. As long ago as 1910, Colonel Sir David Semple, R.A.M.C. [1], following the lead of Fermi and anticipating the needs of a large field force operating in the East, perfected a simple method of preparing a safe and efficient antirabic vaccine that any competent bacteriologist could carry out for the treatment of rabies locally, at a base or central laboratory.

The vaccine is prepared by emulsifying the brains of rabbits that have died from inoculation with “fixed” rabies virus, in a dilution of carbolic acid of sufficient strength to kill the virus, but insufficient to destroy the antibodies present in the affected nerve tissue or to destroy its immunizing properties. This carbolized vaccine has stood the test of statistical examination and animal experiment by many investigators working independently, and has been used for many years as the standard antirabic treatment in all Pasteur Institutes throughout the Empire [2]. It is proposed to record here a few general remarks on the incidence of rabies in Irak, and to outline the method adopted in working a Pasteur Institute in Bagdad, for the information of those who in future may find the necessity of establishing a similar institute under like conditions of tropical climate.

[May 7, 1923.]

THE OCCURRENCE OF RABIES IN IRAK.

Rabies in dogs and hydrophobia in man were recognized by medical practitioners in Bagdad long before the British occupation. For many years Arab "Hakims" in country districts have been acquainted with "madness" in dogs, and have regarded their bites, when mad, as fatal to mankind. In like manner, they deem a bite from a wolf or jackal to be especially dangerous. Their knowledge of the pathology of rabies is, however, chimerical.

Early in the late war canine rabies was detected by the Military Medical and Veterinary Services in Irak. As the Army, with its following of pet dogs, accumulated, and it became known throughout the Force that a dog-bite, real or spurious, would elicit a passport from the infernal regions of Irak to the delectable mountains of India, so the military establishment became depleted by an increasing stream of personnel passed over to Kasauli for antirabic treatment. There was undoubtedly a good deal of malingering, but since deaths from hydrophobia had actually occurred in the country, no medical officer could undertake the responsibility of denying antirabic treatment to any patient who presented a wound alleged to have been caused by a bite of dog or jackal that might have been rabid.

During 1919 a remarkable series of cases of hydrophobia occurred in a refugee camp at Bacuba, in which forty-six persons were bitten by a rabid jackal which ran amok in the camp. Twenty-eight of the men bitten were sent to Kasauli for antirabic treatment. They arrived fifteen days too late, and five of them died of hydrophobia during, or shortly after, treatment.

Of the eighteen people—some of whom were women and children—who were not sent to Kasauli, eleven of them died of hydrophobia in the Military Hospital at Bacuba. The death-rate amongst the treated cases was 17·9 per cent. The death-rate amongst the untreated was 61·1 per cent. The figures are taken from official records, and I have verified them from correspondence in the Central Laboratory, Bagdad, where the diagnosis of four of the fatal cases at Bacuba was confirmed by histological investigation.

In the year 1920, 50 Europeans and 212 Indians were dispatched to Kasauli for antirabic treatment, and in the first six months of 1921, 58 Europeans and 167 Indians were sent to Kasauli. Of these 487 patients, three British soldiers and three Indians arrived too late for effective treatment and died of hydrophobia.

It is difficult to obtain trustworthy evidence regarding the prevalence of hydrophobia amongst the civil population. The Director of the Civil Hospital, Bagdad, informed me that he had seen four cases in that city during the last three years.

The disastrous effects of a rabid animal running amok in a native village, and the beneficial effects of early vaccine treatment are exemplified by the following well-authenticated case, details of which were kindly given to me by the Director of Health Services, Irak, and by Dr. Corner, the civil surgeon at Kirkuk, who personally investigated the cause of death of the victims.

In November, 1921, an important Sheik and his daughter attended the Central Laboratory, Bagdad, for antirabic treatment. The man was severely bitten on the hand by a wolf, the girl was also dangerously bitten on the face. The civil surgeon at Kirkuk, who sent the patients for treatment, reported that eight other persons in the village had been bitten by the same wolf, but refused to attend for

treatment. I communicated with Dr. Corner, requesting him to keep an eye on all the people who were bitten. Four months afterwards he reported as follows:—"The Sheik of Kifri and his daughter are alive and well. Of the other eight persons bitten, two women and six men developed hydrophobia and died; also three donkeys bitten by the same wolf have died. A cow that was bitten likewise sickened of the disease, but was slaughtered and eaten by the natives."

It is impossible to stamp out rabies in tropical countries, because jackals, wolves, and, in East Africa, hyænas provide a natural reservoir for the virus, and these animals, when rabid, will raid the villages and infect the dogs of the inhabitants and sometimes the people.

In tropical countries the diagnosis of rabies in dogs and animals devolves almost entirely on the laboratory, to which dogs' heads in all stages of decomposition are brought in large numbers whenever rabies is notified.

Negri bodies are found in the brain of 97 per cent. of rabid animals, and their presence in the pyramidal cells or the cells of Purkinje is generally accepted as conclusive proof of rabies; but, unfortunately, one cannot infer the contrary if they are not found.

In making preparations for diagnosis by the microscope I have found that in the tropics the following method of staining gives the best results [3]:—

Tissue for section may be fixed in the usual way. Smears of the hippocampus major should be fixed wet in methyl alcohol. To make up the stain, put 50 c.c. of distilled water in a measure glass—add three drops of saturated aqueous solution of methylene blue, shake up and then add four drops of saturated alcoholic solution of basic fuchsin (if permanent section preparations are required make the stain up in 50 c.c. of a 5 per cent. solution of carbolic acid instead of water); then flood the smear with the stain for five minutes, heating gently until steam arises. Examine under a low power objective and look for pyramidal cells, which will often be found clumped together in one part of the film. They should be stained light blue: if densely stained soak the slide in water and control the decolorizing under $\frac{1}{8}$ in. objective.

Preparations thus obtained have the advantage of presenting various depths of staining, some parts being too heavily stained, other too lightly, whilst in the intervening parts Negri bodies appear under the $\frac{1}{2}$ in. objective as brilliant pink dots, globules or oval bodies, in a light blue mounting formed by the pyramidal cells. Careful focusing will reveal in the depths of the Negri body a few dull bluish points or granules. If the stain is made up with 5 per cent. carbolic solution instead of water, the Negri bodies take a bright ruby red colour in contrast to the cell nuclei which stain deep purple or chestnut.

Negri bodies may be so numerous as to be present in nearly every pyramidal cell, or they may be so scarce as to require prolonged search through many slides before a typical and indisputable specimen can be found. In canine and jackal rabies, however, they are generally found at once in smear preparations, and an immediate diagnosis can be made without the necessity of cutting sections. If the finding in smears is difficult or uncertain, stained sections should always be examined before an opinion is pronounced.

In cases in which the finding of Negri bodies was controlled by biological test, the rabbits died of rabies between the sixteenth and nineteenth day after subdural inoculation with 0.2 c.c. emulsion of the cerebellum of the suspected animal. In one case in which prolonged search failed to reveal Negri

bodies, but in which the clinical evidence was convincing, the biological test proved positive.

I consider it important that in the first case or two that occur in a district the presence of rabies should be biologically proved; for if the diagnosis is unquestionably rabies, then more cases may be expected, and due precautions should be enforced without delay. In cases in which there is a clinical suspicion of rabies, but which prove negative on investigation for Negri bodies, the biological test should be made.

The necessary subdural inoculation of a couple of rabbits or guinea-pigs can easily be done without any special instruments. A cork-borer of about $\frac{3}{8}$ in. calibre does very well instead of a trephine; forceps, a scalpel and a little surgical handicraft are presumably always available.

THE MANUFACTURE OF ANTIRABIC VACCINE.

The vaccine is made, as will be described presently, from the fresh brains of rabbits that have died as a result of subdural inoculation with "fixed rabies" virus. A local strain of fixed virus may be acquired from the original "wild" virus as found in the fresh brain of a naturally infected rabid dog, wolf or jackal, and commonly termed "street" virus. This "street" virus, when inoculated subdurally upon a rabbit's brain, has a variable incubation period of from fourteen to thirty days preceding the onset of symptoms.

If a strain of this virus is carried on from rabbit to rabbit by subdural inoculations of brain substance, the virulence of the poison becomes curiously altered in that the incubation period diminishes—possibly because of more rapid proliferation of the virus by successive "passages" through rabbits—until after some thirty or more of such inoculations we find the period of incubation of the disease has become reduced to a fixed limit of about seven days, followed by death not later than the tenth day. In contrast to the original "street" virus, however, the fixed virus—when injected subcutaneously—appears to be incapable of penetrating to the higher nerve centres and causing symptoms. It would be interesting, however, to have more experimental data on this matter, the explanation of which seems obscure.

Since the preparation of fixed virus is tedious and expensive in animals, it is convenient to obtain the strain of fixed virus from one of the Pasteur Institutes already established. It must be remembered that the virus is extremely delicate, and in hot weather would probably be killed during transmission through the post. Aeroplane transport would solve this difficulty, but in Bagdad it was necessary to get the virus sent over from Kasauli in live rabbits, relays of which were subinoculated as required during the journey.

A virus, killing on the tenth day, is inoculated into a rabbit in the usual way. On the sixth or seventh day after the operation it will be observed, on disturbing the rabbit, that it has lost the power of judging the distance and the muscular effort required in jumping from one side to the other of its cage. The animal takes too forcible a spring and strikes its nose against the wall towards which it jumps. This very early and, I believe, characteristic symptom of fixed virus infection should be noted. Within twenty-four hours of this first symptom appearing, a very fine tremor of the ears and head will be observed, and the next day, if taken out of its cage and allowed to run about, the creature will be seen to have a reeling gait, as though intoxicated. On the ninth or tenth day it will be dead or dying. If it dies on the ninth day, its brain can be used for the preparation of vaccine

only. If it survives until the tenth day, and is then obviously moribund, it can be used for subinoculation or "passage," and for the preparation of vaccine also. If, however, the rabbit has died before the ninth day, its death has been caused by some intercurrent malady, and the animal should be discarded. During a period of intense heat in Bagdad, the virus seemed to lose its strength and did not kill until the twelfth or thirteenth day after inoculation. One passage of the virus through a guinea-pig, however, was followed by the restoration of its original lethal effect on rabbits, in which it remained stable, killing on the tenth day until last summer, when, I am informed, it had to be "restored" again, this time by passage through a monkey. The stock virus should be maintained by passage from rabbits dying only on the tenth day. Occasionally a rabbit escapes and does not die. Such an event is probably due to a defect in technique, and will be of very rare occurrence after a little practice and careful attention to detail, which I have described in the *Journal of the Royal Army Medical Corps* for December, 1922. Assuming that the inoculated rabbit is dead or dying on the tenth day, it should be disinfected by immersion in a pail of cresol for a couple of minutes. The floor of the room and the operating table should be swabbed with cresol; the dead rabbit laid out with its head resting on the edge of the table and its legs tied out behind. The top of the rabbit's skull is then exposed by reflecting the skin from each side of a median incision extending from the nape of the neck to the muzzle, care being taken not to open the orbital or aural cavities. The skull is then swabbed with 1 in 20 carbolic and the site of the trephine hole seared with a hot iron. A culture is taken from the brain by passing a platinum needle through the trephine hole and inoculating tubes of broth and agar. The skull is then chipped away and the brain exposed. It should be normal in appearance and the surrounding tissue should be free from any inflammatory reaction. A portion of the cerebellum is snipped off and preserved in equal parts of glycerine and distilled water, not saline solution, which would weaken the virus. This is the reserve supply of the virus, which should be sealed up in the tube and kept in ice to be used in the event of any mishap to the rabbit to be inoculated with another small piece of the fresh cerebellum emulsified in sterile distilled water.

Having thus provided for continuation of the strain of fixed virus, the whole brain is then removed under strict aseptic conditions, weighed and thoroughly triturated in a mortar. A solution containing 1 per cent. of phenol in 0·8 per cent. salt solution is slowly mixed with it until a suspension of 1 in 50 of brain substance has been obtained. This is placed in an incubator at 37° C. for twenty-four hours, a treatment which is sufficient to kill the virus. The suspension is then diluted with an equal part of 0·85 per cent. salt solution and samples are taken which are tested for sterility, aërobically and anaërobically in broth and agar. The suspension of the virus is stored at 0° C. between blocks of ice in an ice-chest until the sterility tests have been completed, and subsequently until required for use. Under these conditions the carbolized vaccine retains its full potency for at least three months. It should not be used beyond three months from date of manufacture.

Before inoculation the suspension—which should be stored in hermetically-sealed rubber-capped bottles holding 25 c.c.—is withdrawn by means of a syringe sterilized in hot oil (140° C.) and again diluted with an equal part of 0·85 per cent. salt solution; so that the vaccine finally contains 0·5 per cent. of brain substance, 0·25 per cent. of phenol, and 0·85 per cent. of salt.

Each patient, however severely bitten, received daily doses of 5 c.c. of this

suspension for a period of fourteen days: $2\frac{1}{2}$ c.c. are inoculated on either side of the middle line of the abdomen by inserting the point of the needle at an acute angle between the superficial and deep layers of the skin and pressing out the vaccine between the epidermis and dermis, i.e., intracutaneously rather than subcutaneously. The total amount of brain substance injected is about 0.25 grm. for each patient. Roux syringes holding 5 c.c. are most suitable for injecting the vaccine. The needle should be sterilized by dipping it in hot oil between inoculation of each patient. The inoculation causes no pain, no general reaction and no local reaction beyond a little redness and itching of the skin. Patients should be advised against taking alcohol or indulging in any unnecessary physical exertion during, and for ten days after completion of, treatment. At the conclusion of the course of inoculations each patient is given a stamped addressed post-card and requested to inform the Director of the Institute of the state of his health three months after treatment.

Hydrophobia usually develops before the eighth week after infection in those who arrive too late for treatment. Patients reported to be alive three months after completion of treatment are recorded as having been successfully vaccinated.

RESULTS OF ANTIRABIC TREATMENT IN BAGDAD.

During the last six months of 1921, 137 patients attended the Central Laboratory, Bagdad, for antirabic treatment by carbolized vaccine. Up to the time I left Irak in May, 1922, no case of hydrophobia nor any unpleasant after-effects of the treatment had occurred.

The work of the Bagdad Pasteur Institute has since been continued by the Civil Medical Administration, and up to within a few weeks ago only one death from hydrophobia had occurred in a patient who had undergone treatment, and that was in the case of a man who refused to allow the treatment to be completed and so received only four doses of vaccine instead of fourteen.

Analysis of the records shows that 16 per cent. of the patients were bitten by animals proved by the laboratory investigation (i.e., by biological test or by the finding of Negri bodies) to have been rabid at the time of biting, and 10.9 per cent. were bitten by dogs not examined in the laboratory, but certified by veterinary medical officers to have been rabid. In 26.9 per cent. of the cases treated there was evidence of rabies in the biting animals, though evidence on clinical grounds only cannot always be regarded as conclusive.

Of the several methods of prophylactic treatment for rabies, I believe that, under conditions of tropical climate, the carbolized vaccine of Semple is the best. The standardized glycerinated virus of Phillips [4], as used in the United States, seems ideal for treatment in non-tropical countries; but, apart from the difficulty of obtaining sterile glycerine in the dusty atmosphere of a tropical town, we found that during the hot weather in Mesopotamia, contact with glycerine rapidly killed the virus and would, presumably, affect the potency of the vaccine.

LIMITATIONS OF ANTIRABIC TREATMENT.

The object aimed at in antirabic treatment is to confer an active immunity against rabies before the virus in the saliva of the animal which inflicted the bite reaches the nerve centres; when this has been accomplished the failures rarely exceed 0.8 per cent. On the other hand, should the virus have already

reached the nerve centres by the time the patient has arrived for treatment, there will be no symptoms to show that this has taken place, but hydrophobia will set in fourteen days or so afterwards, irrespective of whether the patient receives treatment or not. Suppose for instance the virus reaches the brain of a bitten person one day before the course of treatment is completed, the patient will develop hydrophobia fourteen days afterwards and during this interval he will not have a single symptom to show that the object of treatment was defeated one day before completion. The explanation of such a case is clear, since we know that "street" virus planted direct on the brain of a rabbit has an incubation period of at least fourteen days, and that no treatment subsequent to direct inoculation of the virus on the nerve centres will prevent the onset of rabies. Now antirabic treatment extends over a period of fourteen days, and the time occupied by the virus in growing up the nerve centres is variable and mainly dependent on the proximity of the bite to the brain. The importance of early treatment is evident, for we have to set going a race between the growth of a disease and the progress of immunity, in which immunity is handicapped by disease having a considerable start. If the disease wins and reaches the vital nerve centres before their defence is organized, the patient will be in the same hopeless condition as a rabbit would be had it been inoculated directly upon the brain with street virus.

COST OF ANTIRABIC TREATMENT.

Provided that there is a well-equipped laboratory already established for general bacteriological work, the additional outlay required is trivial. The cost is roughly estimated as follows:—

INITIAL EXPENDITURE.						Rs.
Outlay for instruments and apparatus	620
Cost of rabbits imported from Kasauli	208
Cost of rabbit hutches, &c.	360
						1,188
RECURRING EXPENDITURE.						Per month. Rs.
Salary of 1 assistant surgeon, I.M.D., at, say	450
„ 1 laboratory attendant	200
„ 1 extra sweeper	14
Charge allowance for the responsible medical officer	250
						914

The successful breeding of rabbits is an essential item in the maintenance of a Pasteur Institute. Cut grass and crushed oats or grain must be supplied daily, and it must be remembered that these animals will not breed during the hot season in Mesopotamia. The whole year's supply must, therefore, be raised in the cool months.

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**Specimens of (1) *Schistosoma bovis* and of its Snail Carrier ;
(2) the Intermediate Hosts of *Schistosoma mansoni* Brazil.**

Exhibited by J. B. CHRISTOPHERSON, C.B.E., M.D.

I AM showing to-night specimens of: (1) *Schistosoma bovis* from the Egyptian Sudan sent by Major L. Danels, of the Veterinary Department, Sudan, also some specimens of the fresh-water snail which has been found by Cawston to be a carrier of this trematode in Natal—the vector in the Sudan has not yet been identified. (2) Also specimens of the intermediary hosts of *Schistosoma mansoni* of Brazil, sent by Professor Lutz.

Schistosoma bovis is interesting in view of the fact that Cawston has recently reported the case of a boy infected by *Schistosoma bovis*, a conclusion which F. Milton supports from Cawston's "typical" drawings of the ova (I have put out Cawston's drawings for exhibition). The ova have terminal spines and are longer and narrower than those of *Schistosoma hæmatobium*.

Milton suggests that cases of rectal infection with terminal spined ova without vesical symptoms, such as Low described in Uganda in 1907, and Chesterman has lately reported from the Belgian Congo, and Clapier from the French Congo, are cases of infection with *Schistosoma bovis* and not with *Schistosoma hæmatobium*. In support of this suggestion it has been pointed out that in Egypt, when *Schistosoma hæmatobium* invades the rectum, it rarely does so without involving the bladder to a greater extent. *Schistosoma bovis* inhabits the rectal veins in bovines and equines, and the ova are found in the fæces and sometimes in the urine of infected animals.

As will be seen from the specimens, *Schistosoma bovis* is considerably larger than *Schistosoma hæmatobium*.

With regard to the intermediary host of bovine schistosomes Dr. F. G. Cawston has incriminated *Physopsis africana* in Natal (specimens shown.) Dr. Cawston writes regarding a photograph thrown on the screen: "This site of the Magalies Tobacco Factory is well known as a source of bilharzia infection in the Transvaal. In 1917 I found *Physopsis africana* on rushes at the side of the river infested with schistosomes, and *Limnæa natalensis* infested with other cercaria."

The intermediary hosts of the Brazilian *Schistosoma mansoni* sent by Professor Lutz are *Planorbis olivaceus* and *Planorbis centimetralis*; he has given the following information:—

"*Schistosoma hæmatobium* does not as far as we know exist on the American continent, no doubt due to the absence of a suitable intermediary host. *Schistosoma mansoni* is the species of Bilharzia disease found in South America and in the West Indies; Cuba is not known to be infected endemically. The intermediary host of *Schistosoma mansoni* in the West Indian islands is *Planorbis guadelupensis*, which is also the intermediary host of *Schistosoma mansoni* in Venezuela. This snail is sparsely found in northern Brazil, the intermediary hosts of *Schistosoma mansoni* in Brazil are: (1) *Planorbis olivaceus*, and in north Brazil (2) *Planorbis centimetralis*. *Schistosoma mansoni* is found not only in Brazil but also in Dutch Guiana, Venezuela, Colombia and Peru."

PROCEEDINGS
OF THE
ROYAL SOCIETY OF MEDICINE

EDITED BY
SIR JOHN Y. W. MACALISTER
UNDER THE DIRECTION OF
THE EDITORIAL COMMITTEE

VOLUME THE SIXTEENTH

SESSION 1922-23

SECTION OF UROLOGY



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1923

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SECTION OF UROLOGY.

CONTENTS.

October 26, 1922.

Sir JOHN THOMSON-WALKER, M.B., F.R.C.S.	PAGE
Relation of Calcified Abdominal Glands to Urinary Surgery	1

November 30, 1922.

W. LANGDON BROWN, M.D., F.R.C.P.	
The Factors in Uremia	19

January 25, 1923.

Sir JOHN THOMSON-WALKER, F.R.C.S.	
(1) Case of Cyst of Prostate	31
(2) Case of Myosarcoma of Epididymis	31
(3) Case of Aberrant Prostatic Nodule	32
Sir JOHN THOMSON-WALKER, F.R.C.S., and F. J. F. BARRINGTON, M.S.	
Case of Malakoplakia	32
W. GIRLING BALL, F.R.C.S.	
(1) Necrosis of Kidney following Ligature of Abnormal Renal Vessels ...	34
(2) Specimen showing Transitional-celled Growth of the Kidney ...	35
(3) Absent Right Kidney ; Deformity of Left Ureter	35
W. G. SUTCLIFFE, F.R.C.S. (Shown by CYRIL NITCH, M.S.).	
Two Large Calculi removed from the Perinaeum of a Male, aged 62, in Margate Cottage Hospital	36
J. McALPINE, M.B.	
Specimens of New Growth of the Pelvis and Kidney	37
Shown by R. OGIER WARD, F.R.C.S.	
A Large Renal Calculus	38
J. SWIFT JOLY, F.R.C.S.	
Two Cases of Glandular Epispadias	39
R. H. JOCELYN SWAN, O.B.E., M.S.	
(1) Multiple Cystic Formation in Lower Pole of Kidney	41
(2) Pyonephrosis due to the Kinking of the Ureter by Aberrant Renal Vessels	41
(3) Prostate removed by Prostatectomy ; Weight, 12 oz., or 340 gm. ...	42

Shown by JOHN EVERIDGE, O.B.E., F.R.C.S.	PAGE
(1) Specimen showing Interior of the Bladder Six Months after Extensive Resection for Carcinoma, with Transplantation of the Right Ureter	43
(2) Specimen of Diverticulum of the Bladder	43
A. CLIFFORD MORSON, O.B.E., F.R.C.S.	
Case of Ectopia Testis	43
KENNETH M. WALKER, F.R.C.S.	
Serous Cyst of the Kidney	45
February 22, 1923.	
A. RALPH THOMPSON, Ch.M., F.R.C.S.	
The Propriety of attempting to secure Primary Union after Operations upon the Bladder and Prostate	47
R. OGIER WARD, F.R.C.S.	
Case of Air Embolism occurring during Urethroscopy... ..	54
March 22, 1923.	
J. SWIFT JOLY, F.R.C.S.	
The Operative Treatment of Vesical Diverticula	55
May 31, 1923.	
R. H. JOCELYN SWAN, O.B.E., M.S.	
The Incidence of Malignant Disease in the apparently Benign Enlargement of the Prostate	71
June 28, 1923.	
CHARLES GREENE CUMSTON, M.D.	
Two Points in connexion with Chronic Nephritis	81
Specimen shown by FRANK KIDD, M.Ch.	
Candle removed from the Bladder of a Male... ..	84
W. GIRLING BALL, F.R.C.S.	
Pyelogram illustrating the Breaking of Two Shadows into Multiple Shadows as the Result of Injection of Sodium Bromide	85
Sir JOHN THOMSON-WALKER, F.R.C.S.	
(1) Case of Malignant Growth of the Renal Pelvis, with Calculi	85
(2) Case of Vesico-urethral Calculus	87

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Section of Urology.

President—Sir JOHN THOMSON-WALKER, F.R.C.S.

Relation of Calcified Abdominal Glands to Urinary Surgery:

PRESIDENT'S ADDRESS.

By Sir JOHN THOMSON-WALKER, M.B., F.R.C.S.

IN the last few years I have examined forty-two cases of urinary disease, or of supposed urinary disease, where calcified abdominal glands were found and in which questions of ætiology, of diagnosis and of treatment arose. It has occurred to me that a discussion of these questions might be of interest to most of the members of the Section. There are doubtless many angles from which the subject of calcified abdominal glands may be viewed, but a rapid review of the literature of the last twenty years does not reveal any widespread interest in the subject.

We have here the final stage of tuberculous glands in the mesentery, *tabes mesenterica*, a disease common in children, and the literature refers almost exclusively to the active stage of the disease as it occurs in early life. In children, Still [1] says, the condition is very common but the clinical diagnosis of *tabes mesenterica* is much less frequent. In 254 necropsies on tuberculous children at the Children's Hospital, Great Ormond Street, tuberculous mesenteric glands were present in 151, or 59 per cent., while clinically only forty-six cases were diagnosed in 6,000 or 7,000 patients at the Evelina Hospital. Corner [2] and Branson [3] in 1905, and Carson [4] in 1918, have published important articles on the clinical aspect of *tabes mesenterica*. Fordyce [5] refers to it in a more general article, and Lund [6] has drawn attention to it in America. Sims Woodhead [7], Coleman [8], and MacFadyen and MacConkey [9] have published post-mortem statistics.

From these authors I gather (1) that tuberculous infection of the mesenteric glands is common in children; (2) that it gives rise to symptoms which are seldom sufficiently characteristic to permit of a diagnosis unless a palpable mass is present in the abdomen, or unless, as happens in the late stage, one or more of the glands become caseous and throw a shadow with the X-rays; (3) that at this period of life, tuberculosis of these glands, next to that of the bronchial glands, is the most frequent cause of disseminated tuberculosis; (4) that some observers advocate operation upon these glands, but that owing to the difficulties of diagnosis, operation is only performed if a palpable swelling is present; (5) that the results of operation at this stage have been good in the recorded cases.

We will now pass over a period of twenty years or more—the average age in my forty-two cases was 34 years—and come to the subject I have set out to discuss, namely, calcified glands in relation to urinary surgery.

Here we are dealing with the adult, or at least the cases that I have to bring before you were with a few exceptions those of adults.

REMARKS ON ANATOMY AND PATHOLOGY.

Before passing to discuss the clinical side of calcified abdominal glands from the urological standpoint, I should like to refer briefly to some points in the anatomical and pathological aspect of the subject. Clinical investigation showed that one of two groups of glands was affected in the majority of the cases under my care. One, the more common, lying at the lower part of the abdomen on the right side (twenty-five in forty-two cases) and the other less common, towards the upper part of the abdomen on the left side (ten in forty-two). There were five cases where the shadows lay in the mid-line over the sacrum, and experience shows that these belong to the same

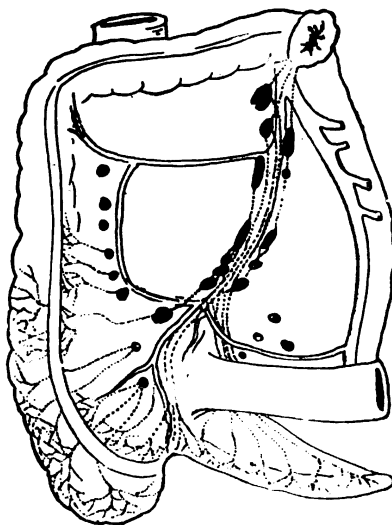


FIG. 1.—Diagram of superior mesenteric artery with ileocolic groups of lymphatic glands (after Jamieson and Dobson).

group as the shadows lay down on the right side, bringing the total in this group up to thirty-two in forty-two cases. There was one case where shadows were present on both right and left sides.

We must refer to the anatomy of the abdominal lymphatics for information in regard to these groups of glands. Jamieson and Dobson [10] in their valuable research on the abdominal lymphatics show that the lymphatic vessels of the intestine from the duodenum to the splenic flexure of the colon, accompany the branches of the superior mesenteric artery and terminate in the large glands which lie around the artery at the root of the mesentery. The lymphatics may be divided into three groups :—

(1) Mesenteric, lying between the layers of the mesentery draining the small intestine.

(2) Ileocolic, lying behind the parietal peritoneum in the space bounded by the attachment of the mesentery below and to the left, the transverse mesocolon above and the ascending colon to the right (fig. 1).

(3) Mesocolic, lying between the layers of the transverse mesocolon.

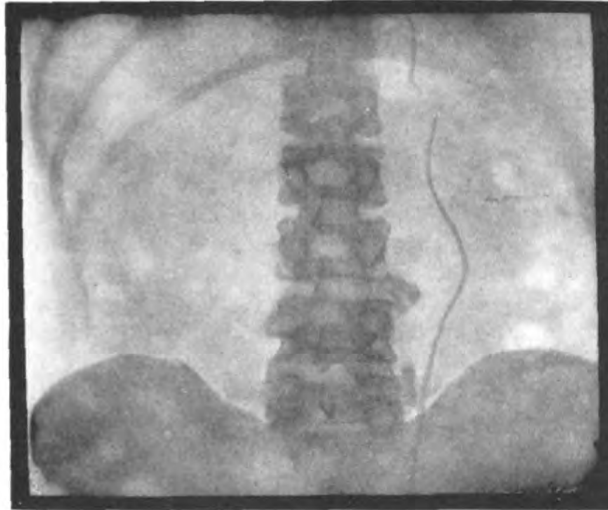


FIG. 2.—Calcified abdominal glands with opaque catheter in ureter.

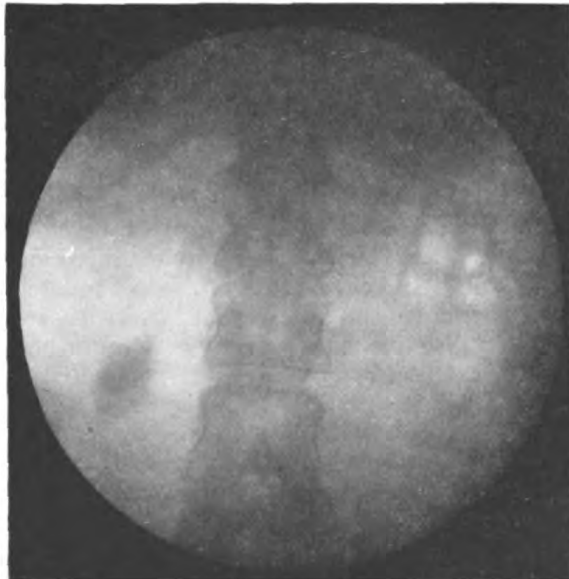


FIG. 3.—Calcified mesenteric gland.

Following the branching of the superior mesenteric artery, five subgroups belonging to the ileocolic group are found, namely: (1) anterior and (2) posterior ileocolic, (3) appendicular, (4) ileal, (5) right colic.

In my cases the calcified glands lying in the right lower abdomen and those over the upper part of the sacrum belonged to the anterior and posterior ileocolic subgroups, while those on the left side of the abdomen belonged to the mesenteric group. These ileocolic subgroups receive lymphatic vessels from the terminal portion of the ileum, from the appendix and from the cæcum, while the mesenteric group drain the small intestine. In the majority of my cases (72·5 per cent.) the ileocolic subgroups were affected (fig. 2).

These glands, which lay to the left of the spine and high up (23·8 per cent.), were assumed to belong to the mesenteric group of glands (fig. 3). In the operated cases none of the calcified glands either here or in the ileocolic angle lay near the bowel margin of the mesentery. One or even 2 in. separated the gland from the bowel, and this was especially noted in those on the left of the spine, which in some cases lay close to the root of the mesentery, if not actually in it. It is possible therefore that some of these left-sided glands may have been glands belonging to the upper ileocolic group near to the origin of the superior mesenteric artery. These glands, we know from the researches of Jamieson and Dobson, may receive lymphatics directly from the ileocolic area, which do not pass through the ileocolic subgroups.

In the cases on which I operated, it was invariably noted that there was no change in the adjacent bowel, which formed the lymphatic field. In six cases the appendix had been removed—twice during childhood and three times in adult life. In two of the adult cases the appendix was certainly normal, and the operation had been performed for pain, on a mistaken diagnosis. In the remaining case I removed the appendix myself, at the operation on the calcified glands. In this case the appendix was fibrous, and the lumen had been obliterated, a condition noted also by Corner in a similar case. In no case was there any adhesion of the peritoneum in the neighbourhood of the gland or elsewhere, and there was no sign of tubercle on the peritoneal surface. Except in three cases, clinical examination in these cases showed no focus of tubercle elsewhere in the body.

These facts are in accord with the observations of others on *tubercles mesenterica* in children and with experimental evidence stated by Sidney Martin [11], which goes to prove that this form of tuberculous infection of the mesenteric glands is usually an isolated tuberculous infection, and results from infection from the bowel. In such cases no trace of tuberculous ulceration is discovered in the bowel, and the bacilli appear to pass through the wall without producing any gross change. The fibrosis of the appendix already noted indicates bygone disease, probably of a septic nature. Tuberculous peritonitis has no direct connexion with this form of glandular infection.

There were three cases in which tuberculosis was present elsewhere in the body. In these three cases there was tubercle of the kidney and bladder. Two were boys, aged 13 and 14 years respectively, and one an adult male of 41 years. In the adult the tuberculous infection was widespread, for he had also suffered from tuberculosis of the lungs and larynx, and there was tubercular disease of one elbow.

These cases raise the important point of the relation between tuberculous abdominal glands and renal tuberculosis. Many years ago Brongersma [12] pointed out that tuberculous mediastinal glands were the chief source of infection in the so-called primary renal tuberculosis. There can be little

doubt that *tabes mesenterica* may, in some cases, be the focus of tubercle from which the kidney is infected, for the condition is an acknowledged source of disseminated tuberculosis. This is more likely to be the case in the *tabes mesenterica* of children than in the calcified glands of the adult. I do not think, however, that even in children this can be a very frequent source of renal infection. Tuberculous infection of the mesenteric glands is a very common condition at this age, but tuberculous disease of the urinary tract does not occur so frequently in children as in adults. It may be noted that the only adult in forty cases in which the calcareous mesenteric glands and urinary tubercle co-existed suffered from tuberculosis of the lungs also. So that presumably tuberculous mediastinal glands were likewise present.

In the calcified state, the tuberculous process is obsolete, and in the cases on which I operated I did not find any tuberculous glands apart from the calcareous glands which I removed. There does not, therefore, seem to me to be any danger of infection of other parts of the body by tubercle bacilli.

I shall describe a case of calcified abdominal glands and then discuss the symptoms :—

Miss O. B., aged 22, had suffered from pain in the left side of the abdomen about five years ago, the pain coming on when she was tired. After a few months it disappeared and commenced again suddenly three years ago. As a child she had been subject to what were called "bilious attacks." Ten years ago she had abdominal pain and the appendix was removed. During the last three years she had been subject to attacks of severe pain at intervals of about a week, and during the last two months the pain had been constant, with attacks of more severe pain once or twice a day. The attack of pain commenced suddenly, usually in the morning, and lasted about three hours. The pain was very severe and was situated in the left side of the abdomen and left loin. It ceased suddenly and left no aching or tenderness. The pain sometimes came on after stooping but often without ascertainable cause. The condition of the bowel did not affect it. It was relieved by heat and lying still. On two occasions only had there been vomiting. There had never been any change in the quantity of the urine and on examination it had always been normal. Menstruation had never been fully established.

On examination the patient was seen to be well nourished. There was no tenderness or anything abnormal on palpation of the abdomen. The urine was normal.

X-ray examination showed, in the antero-posterior view, a heavy, somewhat irregular shadow in the region of the upper pole of the kidney, or suprarenal capsule, alongside the body of the first lumbar vertebra. In the lateral view the same heavy shadow was found in front of the bodies of the third and fourth lumbar vertebrae. A bismuth meal showed delay in the large intestine.

I removed two calcified glands through a paramedian incision to the left of the umbilicus. They lay in the mesentery near the upper end of its attachment and about two inches from the bowel margin. The uterus and ovaries were small and undeveloped. The pain disappeared after the operation and has not re-appeared.

Calcified abdominal glands come into relation with urinary surgery in diagnosis and in treatment, and I propose to discuss them under these two headings.

CLINICAL SYMPTOMS AND DIAGNOSIS.

It is a noticeable feature that in only one adult case could a clear history of childish illness affecting the abdomen be obtained. This patient, aged 44, suffered from indigestion and remembered that as a child he had suffered from abdominal pain and constipation. This absence of a definite history of abdominal trouble during childhood is not surprising when we remember

the slight symptoms and the difficulties in diagnosis in the great majority of cases of *tabes mesenterica* in children. The cardinal symptom of calcified abdominal glands is pain, but there are certain cases which lead me to believe that hæmaturia may also be a symptom.

PAIN.

All surgeons will admit that the diagnosis of the cause of obscure abdominal pain is a matter of some difficulty and the result of the investigation is sometimes disappointing. I do not propose to range over the whole subject of obscure abdominal pain but must confine my remarks to those cases where pain resembles in some measure that of disease of the urinary tract. The distribution of pain due to conditions affecting the kidney and ureter is well known. On the right side of the abdomen two other common pain areas may give rise to difficulty in diagnosis, namely, gall-bladder and appendix pain. The position of these also is familiar to all of you.

In a number of the forty-two cases other pathological conditions were present in addition to the calcified abdominal glands. Thus in seven cases there was stone in the kidney or ureter, in two there was pyelitis, in three urinary tuberculosis, and one patient was pregnant. These conditions all tended to mask the symptoms that might have been due to the glands and thus confuse the diagnosis. But there were twenty-eight cases in which no other disease beside the calcified abdominal glands could be detected and the following notes in regard to symptoms are based upon these cases.

Pain is the chief symptom in calcified abdominal glands and in my cases it was the prominent feature in twenty-five of the twenty-eight cases. It was a dull ache in four, very acute pain amounting to abdominal colic in fourteen, and moderately acute in seven.

In the majority of these pure cases, the pain was a severe colic (fourteen in twenty-five). The duration might vary from a few minutes to several hours. The pain commenced suddenly and usually ceased suddenly. In severity it was comparable to the two chief abdominal colics, namely, renal and biliary colic, and was much more severe than that of appendicitis. In distribution it resembled that of moderate renal pain or of renal and ureteral colic in seventeen cases, of appendicitis in four, and the pain resembled biliary colic in one. In five the pain area was not defined and in one case there was no pain. There were certain negative points that helped to distinguish the pain due to calcified glands from that of renal colic. Movement had practically no effect in initiating or in increasing the pain. Vomiting, so common in renal and also in biliary colic, was absent in these cases. There was no retraction of the testicle and no pain referred to other parts of the body. There was very rarely any disturbance in the action of the bowels such as might be expected in a case of appendicitis of long standing.

A tender spot was present in the abdomen in four cases in which the calcified gland was the only disease present. The tender area lay directly over the calcified gland and, in all of the cases, lay within the area of tenderness present in cases of appendicitis, so that this sign tended rather to confuse than to clear the diagnosis. In one case the calcified gland could be felt as a nodule and rolled beneath the finger at a point in the line of the ureter above the brim of the pelvis.

The proportion of cases in which the pain resembled that of renal pain, or colic, may have been unduly large from the fact that this class of case was the

most likely to come under my care. The explanation of the pain is, I think, to be found in the proximity of the calcified glands to the ureter. The drag or presence of such a calcareous mass would very easily cause ureteric spasm. The aching pain in the appendix region in four cases was due to the position of the calcified gland in this area.

HÆMATURIA.

Blood was present in the urine in microscopic or in naked-eye amount in six cases in which no disease except the calcified gland could be detected. One case had severe intermittent hæmaturia for eight years as the only symptom. This case may be described in detail as it is of exceptional interest:—

Miss A., aged 25, the daughter of a doctor, first noticed blood in the urine in 1912 at the age of 17. Since that time there had been intermittent hæmaturia, the attacks being brought on by walking or other exertion. For six months she remained recumbent and saw no blood. At the end of that time she walked and the blood recurred. She was examined by a number of physicians and surgeons during the eight years of her illness. The diagnosis of chronic nephritis was first made but this was abandoned. Bacilluria was found on two or more occasions. On other examinations the urine was free from organisms. Guinea-pig inoculations were negative. There were occasional granular casts and some blood casts had been found.

I examined her in December, 1920. The bladder and ureteric orifices were normal. A specimen of urine drawn from each kidney was normal. Examination with the X-rays showed a group of irregular shadows on the right side of the spine opposite the fifth lumbar vertebra just above the iliac crest. There were no other abnormal shadows. There was no tenderness at any part of the abdomen and no enlargement of either kidney. There were no signs of chronic nephritis in the urine or in the general condition of the patient.

I operated in January, 1921, and removed a chain of calcified glands extending from the ileo-cæcal junction for some distance upwards. The appendix was adherent and fibrous, and was removed. Careful palpation of the kidneys, renal pelvis and ureters revealed nothing abnormal. The report on the appendix showed that it was the seat of chronic obliterative inflammation.

In January, 1922, the patient had almost resumed her normal life. She was getting about and had been taking dancing lessons. There had been no recurrence of the hæmorrhage.

The laboratory report on the urine showed some red blood cells, some epithelial squames and a few granular casts.

Any statement in regard to the relation of hæmaturia to calcified abdominal glands must be purely speculative. In the cases in which this symptom has been present, and no other cause has been ascertained, removal of the calcareous masses has been followed by disappearance of the hæmaturia. One may, therefore, I think, be justified in suggesting some relation of cause and effect between the glands and this symptom. The close anatomical relation of the calcareous glands to the ureter is undoubted. In several cases of calcareous glands I have found that the passage of a catheter up the ureter has been arrested at the level of the glands, and although other more rigid or smaller catheters pass on, I have gained the impression that some pressure or drag existed at this part. Looking back on cases of obscure hæmaturia, I can recall, and I think most urologists must have seen, cases in which the only cause of the hæmaturia was some condition outside the ureter, such as an appendix abscess. One cannot avoid the conclusion, therefore, that calcareous glands may, by pressure or by dragging on the ureter, be the cause of hæmaturia.

X-RAY DIAGNOSIS.

With the complete X-ray examination of obscure abdominal cases that is now customary, the discovery of calcified glands is becoming much more common. But, as a result, new difficulties of diagnosis have arisen in the interpretation of radiograms of the abdominal areas. The shadow thrown by calcified glands are most likely to be confused with those of stone in the renal pelvis or ureter, or with gall-stones. There are other less common shadows, but it is not difficult to differentiate them. A calcified abdominal gland throws a shadow of varying density and irregular shape that may be situated over the kidney or gall-bladder areas, or in some abdominal area which a displaced kidney or gall-bladder may acquire. A kidney stone throws a uniformly dense shadow in the position of the calices, the renal pelvis or the upper ureter. A gall-stone throws a shadow in the area of the gall-bladder, or bile ducts, which is not uniformly opaque. There are many exceptions to these statements, and, moreover, the kidney and gall-bladder areas overlap. It follows that difficulties arise in recognizing the nature and position of these shadows.

(1) *Position of a Shadow in the Renal and Gall-bladder Areas.*—In an antero-posterior radiogram, the kidney lies in the upper part of the space bounded by the twelfth rib above, the outer border of the psoas muscle internally, and the crest of the ilium below. The long axis is obliquely set, so that the inner border of the upper and lower poles closely approximate the oblique line of the outer border of the psoas muscle. The renal pelvis lies at the outer border of the psoas muscle at the level of the transverse process of the second lumbar vertebra. From this the ureter passes downwards on to the transverse processes of the third, fourth and fifth lumbar vertebrae. This position is modified by respiration and position. With full expiration the upper pole ascends behind the twelfth rib, and with full inspiration the kidney descends about half the breadth of a vertebral body. In the vertical position there is a similar descent of the kidney. The shadow of a normal kidney can be recognized in a radiogram of first quality. The lower pole and inner border are most evident, and the upper pole less defined. The normal gall-bladder has been demonstrated on several occasions. In most of the pathological conditions of the gall-bladder the wall is thickened, and the outline of the gall-bladder can be shown in a radiographic plate of proper quality. There is greater variation in the position of the gall-bladder than in that of the kidney in normal individuals. It very frequently occupies the space between the twelfth rib and the outer border of the psoas muscle that is occupied by the kidney. The shadow is an elongated pear shape with the apex above (Knox). It lies nearer to the twelfth rib than does the kidney shadow, and its long axis is not parallel with the outer border of the psoas shadow, as in that of the kidney shadow, but bisects the angle between the shadow of the twelfth rib and the outer border of the pyriform gall-bladder, and the outer border of the psoas shadow is much greater than that below the kidney and the psoas. Very considerable variation is found in the relation of the gall-bladder to the bony landmarks, and this is sometimes due to a long and oblique twelfth rib narrowing the costo-vertebral angle. The gall-bladder shadow is then behind the twelfth rib and last intercostal space (fig. 4). The long axis may be more vertical or more transverse. Knox [13] points out that when the gall-bladder is greatly distended it loses its pear shape and becomes more rounded, so that the rounded end of the gall-bladder may resemble the lower pole of the kidney,

but the shape of the whole organ never resembles the shape of the kidney. It may occupy practically the same area as the kidney, and if only the lower half of the gall-bladder is seen, it might easily be mistaken for the lower pole of the kidney. If the outline of the kidney or gall-bladder is shown on a skiagram, a shadow thrown by a stone in either organ may be localized, or a shadow thrown by calcareous glands may lie outside these areas, and the diagnosis can then be made. The stereoscopic method will give valuable information in regard to the depth of the shadow from the surface, but this is not sufficiently accurate for the localization of a doubtful shadow in the kidney or gall-bladder. A calculus in the gall-bladder may lie, as Knox has shown, at any depth from 3 cm. to 13 cm. from the anterior surface of the body, this depending on what part of the gall-bladder or ducts it occupies. ^{10 x 50}

(2) *Size and Shape*.—Very large shadows may be thrown by kidney-stones or gall-stones, but rarely by calcified glands. The larger shadows thrown by

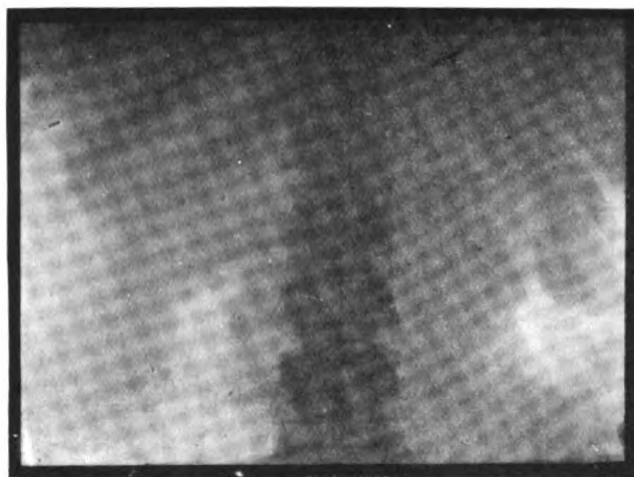


FIG. 4.—Outline of gall-bladder containing gall-stone.

calcified glands are usually groups of shadows and not a single shadow. A large shadow, if thrown by a kidney-stone, will usually be accompanied by a large kidney, that is readily palpated in the loin, for in such cases the kidney is usually dilated with urine or pus. It may be stated, therefore, that when a large oval or round shadow appears in the loin without urinary symptoms and the kidney is not palpable, the shadow is more likely to be thrown by a gall-stone than by a kidney-stone. There are, however, some remarkable exceptions to this generalization, where a very large kidney-stone surrounded by a thin layer of kidney tissue was not palpable in the loin. A stone free in a cavity, such as the pelvis of the kidney or the gall-bladder, will have a round or oval shape, which is rarely, if ever, seen in a calcified gland, where the deposit of salts is not on the surface of a free body but in the irregular necrotic areas of a diseased tissue, and the outline of which is irregular. Thus, a wedge-shaped shadow is characteristic of a stone in the renal pelvis, and a branching shadow is certainly renal. A shadow with irregular outline may be renal or glandular, but is not biliary.

(3) *Density and Uniformity of the Shadow.*—A calcified gland or group of glands has, in the average case, a density midway between that of a urinary calculus and that of a gall-stone. It is not so dense as a urinary calculus of the same size, and it is denser than a gall-stone. There are, however, some exceptional cases, when a gland may throw a dense shadow comparable in opacity to a kidney-stone, and occasionally a gall-stone may contain a large proportion of salts which cast a heavy shadow. Calcified glands throw a shadow which is irregular in density. When a large shadow is thrown, it has a mottled appearance that distinguishes it from the shadow of a urinary calculus or a gall-stone. When a group of shadows are demonstrated, there is a varying density in different parts of the same shadow and between the different shadows that distinguish them from urinary calculi. The shadow of a large calcified gland is not heavier than that thrown by a small gland. The shadow of a urinary calculus is homogeneous, and the density varies with the size of the calculus. There is one exception, however, when a flat calculus



FIG. 5.—Gall-stone and pyelography.

is photographed in face. This will give a faint shadow when the outline is that of a large calculus. In gall-stones the density of the shadow does not correspond to the size of the calculus. The most characteristic form of gall-stone shadow has a central nucleus, a faint, somewhat irregular density in the body and a dense ring at the periphery. The details of structure and the regularity of the shadow are almost characteristic. Rarely, as Knox points out, a renal calculus may throw a shadow which in detail and arrangement has the character of a gall-stone.

(4) *Grouping of Shadows.*—When a number of shadows are present, the arrangement of the shadows will conform to some extent to their surroundings. If calculi lie in a cavity, they will assume more or less completely the shape of the cavity. Large branching calculi in the kidney throw a shadow which resembles a cast of an enlarged renal pelvis and branching calices. Shadows of multiple calculi in the kidney which do not form this perfect cast may nevertheless with careful study be recognized as occupying the pelvis and

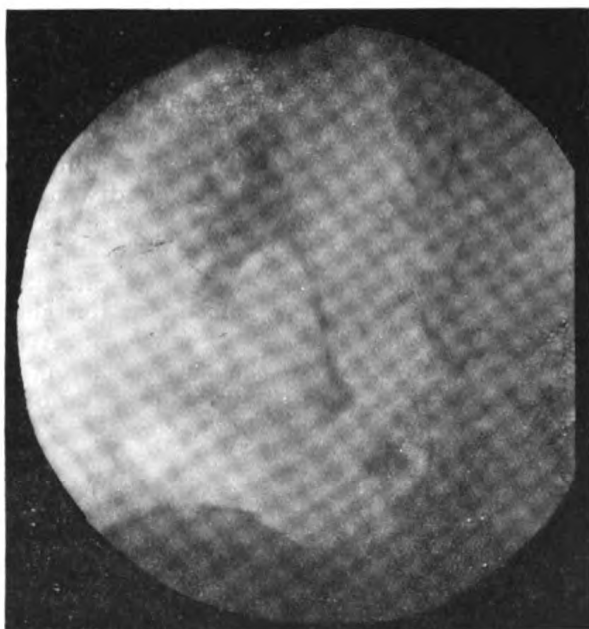


FIG. 6.—Calcified ileocolic glands with pyelography.

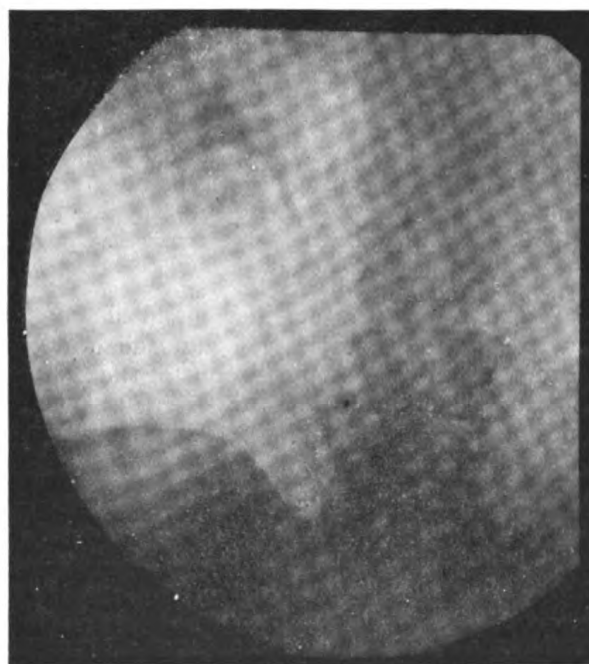


FIG. 7.—Mass of calcified abdominal glands on bodies of fourth and fifth lumbar vertebrae. Pyelography.

calices. If a wedge-shaped shadow of a pelvic calculus is present, outlying shadows which occupy calices will radiate outwards from the base of the wedge. In gall-stones, where all the stones throw a shadow, a peculiar mosaic or honeycomb pattern is produced, which is unmistakable. Where only some of the gall-stones are opaque, or where calculi lying in the ducts are opaque, a line of opacities may be recognized. This line will radiate downwards and outwards from the costo-vertebral angle, and would cross the lines of radiation of renal calculi. Calcified gland shadows never assume this radiating or string-like appearance.

(5) *The Effect of Respiration and Change of Position.*—A stone in an adherent kidney and one in the gall-ducts is fixed and the shadows will move little, if at all, with respiration, or change of posture. A calcified gland is never fixed. The range of movement of the shadow of a stone in the kidney free from adhesions, of a stone free in the gall-bladder and of a calcified gland, differ from each other. Of the three the kidney shadow moves least, the gall-

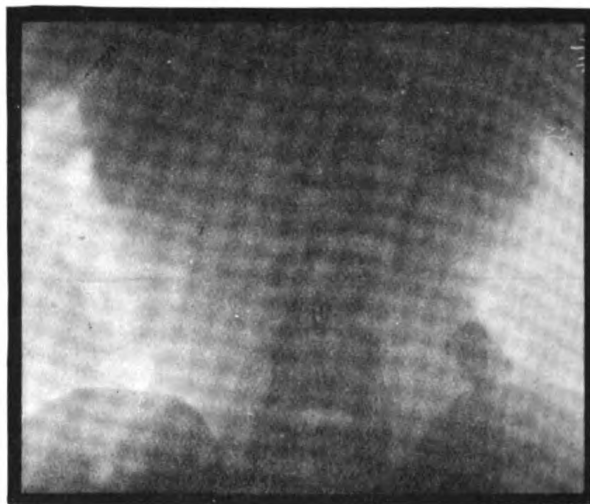


FIG. 8.—Calcified abdominal gland with pyelography.

stone shadow next in extent, and the calcified gland shadow has the greatest range of movement. Not infrequently the gland shadow will, on a second exposure, be found to have moved completely out of the field.

The direction of movement is also of importance. Knox has shown, by making double exposures on the same plate, one in full inspiration and one in full expiration, that the kidney stone shadow moves downwards and outwards, while the shadow of a gall-stone free in the gall-bladder moves almost directly downwards. The shadow of a stone in the common or cystic duct has a less pronounced displacement in deep inspiration and its line of movement will approximate that of a kidney stone. The wide excursion of a calcified gland shadow may be vertical or lateral. Extreme lateral movement is characteristic of the calcified gland shadow. In one plate the shadows may lie on the spine, in another over the iliac bone.

(6) *Pyelography.*—By pyelography the doubtful shadow which may lie within the kidney area and actually within the limits of the kidney shadow,

may be proved to lie outside the renal pelvis and calices. The shadow of a kidney-stone will be engulfed in, or at least be continuous with, the shadow of the renal pelvis or one of the calices. The shadow which lies apart from the pelvis or calices is not a renal calculus, and the diagnosis will lie between a gall-stone (fig. 5) and a calcified gland (figs. 6, 7, 8). Fallacies exist however, even in this accurate method, and a calcified gland may throw a shadow over the kidney (fig. 9) and even over a pyelographic shadow of the renal pelvis. A super-imposed gall-stone may also come within the area of the pelvis and calices and cause confusion (fig. 10). There are many cases therefore in which antero-posterior radiography and antero-posterior pyelography will fail to decide definitely the position of a doubtful shadow. In such cases lateral radiography and pyelography are invaluable.

(7) *Lateral Radiography and Pyelography*.—Dr. Knox and the writer have discussed fully the technique of this method elsewhere and it is unnecessary to

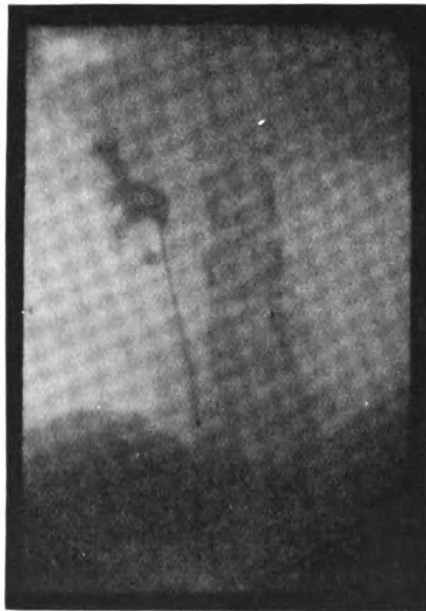


FIG. 9.—Calcified gland in kidney area. Pyelography. (Mr. Clifford Morson's case.)

revert to these details. In lateral radiography the kidney lies on the sides of the bodies of the first, second and upper part of the third lumbar vertebræ. Its shadow cannot be defined, even in a radiogram of the first quality. In a pyelogram with an opaque catheter in the ureter, the pelvis and abdominal segment of the ureter is shown (fig. 11). The pelvis throws an elongated oval shadow tapering at its lower extremity and lying in the shadow of the body of the second lumbar vertebra. From this the shadows of the calices project to right and left, that is anteriorly and posteriorly. The upper calices project above the shadow of the pelvis like horns and the lower calices downwards below the pelvis. The ureter passes downwards and forwards, crossing the body of the third lumbar vertebra and reaching the line of the anterior border at the lower border of the third and the upper border of the fourth lumbar vertebra.



FIG. 10.—Gall-stone superimposed on large branching renal calculus.

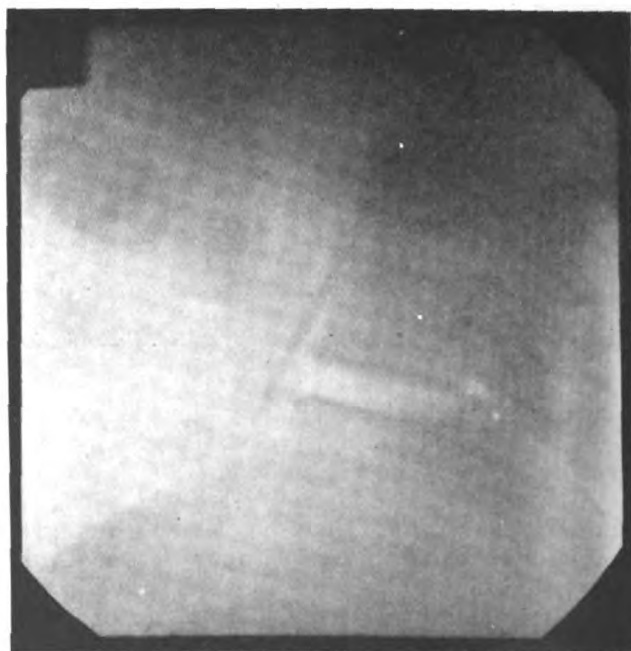


FIG. 11.—Lateral pyelography. Renal pelvis on body of second lumbar vertebra, opaque catheter in front of vertebrae, calcified gland anterior to catheter above iliac crest.

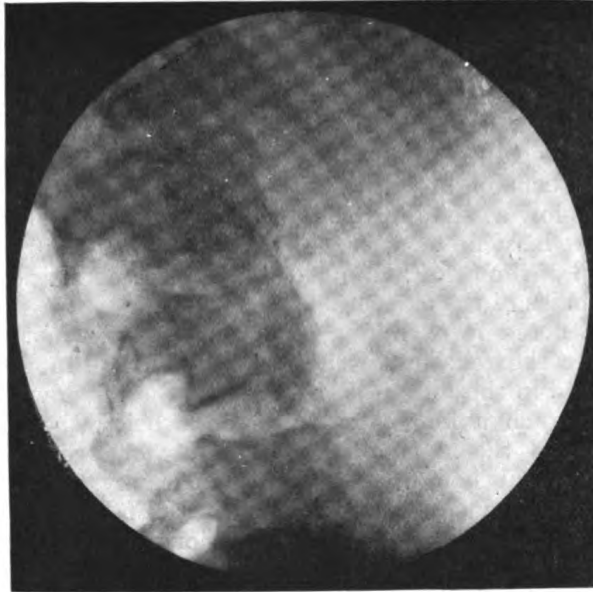


FIG. 12.—Lateral radiography. Renal calculi on bodies of second and third lumbar vertebrae.

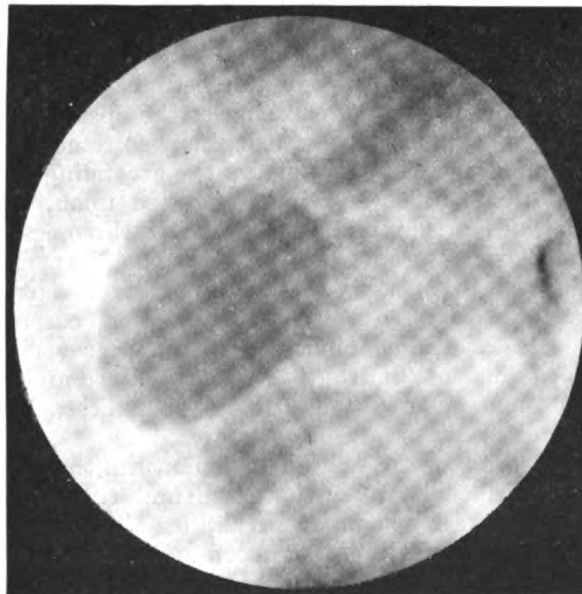


FIG. 13.—Lateral radiography. Large renal calculi projecting in front of lumbar vertebrae.

In lateral radiography, with the kidney in its normal position and where no great enlargement of the organ is present, a kidney-stone will throw a shadow on that of the body of the second lumbar vertebra (fig. 12). A stone occupying the extreme limit of a calyx may throw a shadow which appears behind the body of the vertebra, but this is a rare finding and will not give rise to confusion with any other shadows. The two conditions which may cause confusion with the shadow of a kidney-stone, are gall-stones and calcified abdominal glands. The gall-stone shadow lies well in front of the shadow of the vertebral bodies, usually at the level of the upper three lumbar vertebræ, but sometimes as low as the fourth lumbar vertebra. The shadows of calcified abdominal glands may lie as high as the kidney but they are usually lower down at the level of the third and fourth lumbar vertebræ and they are always in front of the bodies of the vertebræ.

It should be noted in examining doubtful shadows in the abdomen that disease may alter the relation of the organs. Thus, a movable kidney has a wide excursion and in the antero-posterior view the shadow which is thrown by a calculus in such a kidney, may be found below the crest of the ilium. Further, mobility of the kidney may completely change the relation of the organ to the vertebral bodies in lateral radiography. If a patient with a movable kidney is turned on the side the organ falls forward and a stone shadow may be thrown in front of the anterior margin of the bodies of the vertebræ. To avoid this displacement the lateral view must be taken with the patient lying on his back. Lateral pyelography with an opaque catheter in the ureter, will in any case, settle the relation of the doubtful shadow to the ureter and renal pelvis.

When the kidney is greatly enlarged it will project in front of the spine and lateral radiography will show a stone shadow in front of the vertebral bodies (fig. 13).

TREATMENT.

In eleven of the forty-two cases of calcified abdominal glands, I operated and removed the glands. In ten of these cases the operation was planned for the removal of the glands and in one a calculus was removed from the kidney and the gland was uncovered in stripping up the ascending colon and removed at the same time. One other case was operated upon, on my advice, by Mr. G. H. Percival, of Northampton, who removed three calcareous glands.

The result of the operation in these cases was the disappearance of the pain, whether it had the form of constant aching or recurrent attacks of colic. In one case—that of a very stout woman with a large caseous gland near the upper end of the mesenteric attachment—there were several attacks of acute pain soon after the operation, but these have now ceased and there has been no further attack for eighteen months. All the other cases have remained free from pain since the operation.

In the case of severe recurrent painless hæmaturia that I have described in detail, the attacks of hæmaturia have ceased and the patient has returned to practically normal life after being an invalid for about eight years.

These results have justified the operation and although the number is not large, it is sufficient, I think, to lift the operation out of the purely experimental or speculative stage. The question as to whether the operation should be recommended in all cases in which calcified mesenteric glands are discovered is, however, open to discussion.

The evil result of the glands in my cases was practically confined to the causation of pain. At the end-stage of the disease there is no longer any danger of general dissemination of the tubercle bacillus, nor is there in my opinion, any probability of infection of the urinary tract. Tuberculous peritonitis is not a concomitant or a sequel of this condition and adhesions which might interfere with the action of the bowel or form obstructing bands do not take place. From the point of view of the possible development of these complications, therefore, operation need not be considered.

The question is, I think, largely one of the degree of severity of the symptoms and this will frequently be decided by the patient himself. In sixteen out of the forty-two cases, operation was suggested and was either refused or indefinitely postponed by the patient, either owing to the operation being considered too severe a method of treatment for the symptoms or owing to a temporary lull in the symptoms.

In children this view is subject to modification. Here there is some probability of other more recently infected glands being present alongside the calcareous glands which throw a shadow. Against any general rule of operation in these cases in children, there is, however, the knowledge that recovery without operation must take place in the great majority of these cases without further trouble. The fact that a large number of cases of calcified abdominal glands are now discovered during routine examination in adults itself lends support to this conclusion.

Carson, Corner and Branson all advocate operation in children, the two former authorities from the point of view of local abdominal conditions and the latter from the fear of general complications. Corner lays it down very definitely that operation should only be performed in those cases in which a palpable swelling in the abdomen is detected. This narrows the operation field to quite a small minority of cases in children.

My view in regard to adults, where the calcareous end-stage of *tabes mesenterica* has been reached, is that operation is only justifiable in those cases in which symptoms are severe and are proved to be directly due to the calcified glands. These cases can be selected only after investigation by thorough modern methods of examination.

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Section of Urology.

President—Sir JOHN THOMSON-WALKER, F.R.C.S.

The Factors in Uræmia.

By W. LANGDON BROWN, M.D., F.R.C.P.

THE conventional view of uræmia may, I think, be fairly expressed as follows: In nephritis, some toxin or toxins may, by acting upon the central nervous system, lead to one or more of the following groups of symptoms: (1) Cerebral, (2) respiratory, (3) gastro-intestinal, (4) cutaneous, (5) muscular.

In my opinion it is possible to simplify the problem by eliminating certain of these groups. Thus, the gastro-intestinal symptoms of nausea, vomiting and diarrhœa, may be due to an attempt to find alternative channels of excretion. Eight grm. of nitrogen can be excreted by the bowel daily, whereas the skin cannot get rid of more than 3 grm. (Von Noorden). No doubt this is irritating to the bowel, and in certain cases it is clear that organic lesions develop such as ulceration and catarrhal or "diphtheritic" colitis, which in themselves would tend to produce diarrhœa. It is true that toxic irritation of the medulla can of itself produce vomiting, but the presence of urea in the vomit in greater concentration than in the blood, is suggestive of a vicarious method of excretion. Thus Canti has found urea in the blood to be 0·3 per cent., and in the vomit 0·6 per cent. in a severe case. Again, rashes and intense pruritus are very probably symptomatic of an attempt to excrete toxins by the skin. In a case of uræmia with an intense erythema, under my care, the blood, in spite of continuous vomiting, contained 0·4 per cent. of urea. Again, the immediate cause of pruritus is the setting up of osmotic currents in the lymph between the prickle cells; and excess of sodium chloride there might be expected to excite it. The contractions, local pareses and myoclonic movements have also been referred to a local intoxication of the muscles and not to a central cause [1].

Such a view would restrict the term uræmic syndrome to the cerebral and respiratory manifestations. We may next inquire into the evidence of a toxic cause for these manifestations. Roger [2] describes eleven different toxins in normal urine, several of which produce uræmic symptoms, such as coma and convulsions. Canti [3] has shown that urea retention occurs in uræmia, and has been able to correlate the degree of urea retention with the prognosis. But it is difficult to refer uræmia to the retention of a normal constituent of urine since Ascoli showed that total suppression, such as follows complete

obstruction to the ureters, does not lead to the characteristic features of uræmia. Just as hydronephrosis follows an incomplete or intermittent obstruction to the ureter, while atrophy of the kidney is more likely to follow complete obstruction, so uræmia is more apt to follow incomplete failure of excretion, while complete failure produces the syndrome termed "urinæmia." According to Ascoli, severe urinæmia in man is chiefly manifested by bodily weakness and languor which are often apparent before any other symptoms, but it generally leads to progressive mental weakness and exhaustion, often terminating with great suddenness. In urinæmia, however, most of the prominent symptoms of uræmia are lacking, especially the severe and acute mental disturbances, the sudden amaurosis, and the epileptic phenomena in general. Only in occasional cases do the symptoms resemble uræmia. Nor has any better success attended the attempts to explain uræmia as due to the decomposition products or the antecedents of such constituents. If we are going to attribute all the manifestations of uræmia to a toxic cause, its association with partial—rather than total—failure of secretion suggests (1) that the process of its development is a gradual one, and that total suppression is too rapidly fatal for this to occur: (2) that incomplete excretion may lead to altered metabolism so that abnormal toxic products are formed. On this view uræmia would be a disease of metabolism resulting from damaged kidneys. In the same way, diabetic coma is not the direct result of glycosuria but of the altered metabolism produced by diabetes. Golla has suggested that such altered metabolism leads to the formation of trimethylamine, which he has found to be increased ten-fold in the blood in uræmia, and he is inclined to attribute the whole of the syndrome to this substance. It is true that the cerebro-spinal fluid in uræmia will develop a smell of trimethylamine, but for reasons which will appear later I find difficulty in accepting this view *in toto*.

That toxins have a selective effect on nervous tissues is well illustrated in diphtheritic paralysis and plumbism, among exogenous poisons, and that endogenous poisons may have a similar action is suggested by the combination of cirrhosis of the liver with progressive degeneration of the lenticular nucleus (Wilson's disease) where the affected nervous structures become bile-stained. One poison may, by its selective action on different tissues, produce very different symptoms: thus alcoholism may be associated with delirium tremens, multiple peripheral neuritis, cirrhosis of the liver and cardio-vascular degeneration. It may be argued that other conditions determine which of these manifestations occurs; thus delirium tremens follows too rapid reduction of the dose of alcohol when it has become an acquired need, while cirrhosis only develops when alcohol has set up a preceding gastritis, in which other factors such as pyorrhœa may play a part. Similarly it is quite possible to explain uræmia as due to one poison acting on different parts of the central nervous system according to varying conditions. And yet it is a striking fact that the different manifestations appear under very different conditions and have a widely different significance.

Let us consider next the conditions under which they arise.

Is nephritis a necessary precursor to uræmia? Using the term nephritis in the strict sense, the answer is, "no"—for a toxæmic kidney may, at any rate, give rise to convulsions and amaurosis, although it apparently never causes paroxysmal dyspnœa. By a toxæmic kidney we mean a kidney the tubules of which are suffering from degenerative but not inflammatory changes, the result of some chemical poison. Although severely damaged for a time, such a kidney is, nevertheless, capable of complete recovery. The commonest cause

of toxæmic kidney is bacterial toxæmia. Febrile albuminuria associated with cloudy swelling of the kidney should be referred to this group. But this is a mild form of the condition. "More potent are certain exogenous and endogenous poisons. Mercurial salts, arsenic and cantharides are important causes clinically, while uranium and bichromate salts are frequently used in the experimental production of this condition. Jaundice and diabetes mellitus are not uncommon causes, but the outstanding example of the endogenous group is the toxæmia of pregnancy" (Geoffrey Evans). It involves no cardiac hypertrophy or gross arterial disease. There is cloudy swelling, vacuolation and desquamation of the epithelium of the convoluted tubules, there may be fatty degeneration, and in more severe cases focal or diffuse necrosis. The glomeruli, the loops of Henle, and the collecting tubules show little damage. There is œdema of the interstitial tissue but no small cell infiltration except as a secondary reaction to necrosis. Similar changes may be found in other organs, especially the liver. The condition may give rise to œdema and intense proteinuria, like acute nephritis, but distinguishable, during life, by the following tests. The urine rarely contains polymorph leucocytes, as is usual in acute nephritis. The protein in the urine of acute nephritis shows an albumin to globulin ratio of 6 to 1, but in toxæmic kidney, of 2 to 1. The output of diastase in the urine is low in nephritis but high in toxæmic kidney. The urea in the blood is raised in many forms of nephritis but not in toxæmic kidney, while the blood sugar tends to rise in the former but not in the latter, for in nephritis the glomeruli are less permeable, and in toxæmic kidney they are too permeable (Mackenzie Wallis). The failure to recognize toxæmic kidney has led to some confusion, which has affected prognosis. The outlook in toxæmic kidney is better than in nephritis of apparently equal severity, even when accompanied by uræmic convulsions. Again, the macroscopic appearances of a toxæmic kidney, post mortem, have been mistaken for those of chronic parenchymatous nephritis. But the distinction between the two conditions can readily be made by the above biochemical tests. Apparently, then, uræmia may occur with a temporary and non-inflammatory lesion of the kidney, which is not characterized by a retention of toxins, but by an undue permeability of the kidney to anything which the blood presents to it.

Next, as to acute nephritis, interest in which was much stimulated by the widespread epidemic which occurred in the war [4] [5]. This is not the place to discuss its causation, but it may safely be asserted that the condition was a true nephritis, and that some of the apparent points of difference between it and the acute nephritis of civil practice were due to previous observations on the latter having been inaccurate or defective. Thus polymorph leucocytes which were found in the urine of war nephritis have since been noted in ordinary acute nephritis. Dunn found the glomerular tufts were obviously empty of blood in war nephritis, and Andrewes has since found this to be true of ordinary acute nephritis. Blood may be extravasated between the layers of Bowman's capsule, but the glomerular vessels are empty. The frequent onset with bronchitis and dyspnœa at first seemed a point of distinction, but Dunn found that these were due to fibrinous exudate into the bronchi, desquamation of the endothelium and minute thrombi in the pulmonary capillaries. He regarded the lungs as the primary site of invasion, the nephritis being secondary. In the same way, the sore throat preceding scarlatinal nephritis cannot be regarded as differentiating this from other types of acute nephritis. Renal permeability tests in the war cases pointed to true nephritis in such features as diminished diastase output, chloride retention, deficient urea con-

centration in the urine and increase of the urea in the blood, though not all these points were present in every case investigated. The interest of this epidemic for our present discussion is that here we have a true acute nephritis, with a low mortality-rate, while only about 7 per cent. of the cases passed into a stage of chronic nephritis (Tremolières and Caussade), so that we have an opportunity of studying uræmic manifestations in a benign type of the disease. In a series of 166 cases investigated by me the only uræmic manifestation was that of convulsions in seven cases, i.e., 4 per cent., the same proportion as that found by other observers. All these seven cases recovered after venesection. Apathy, slight drowsiness, subnormal temperature with headache and nausea were the most usual uræmic symptoms according to Bradford. Vomiting occurred but was not usually so severe as in civil practice. Bradford noted transitory acute mania after a convulsion in one case, and amaurosis in another, but he states that uræmic asthma did not occur. Certainly I never saw it in my series. Canti did not find the urea in the cerebro-spinal fluid so much increased as in ordinary cases of uræmia. Pains in the limbs were emphasized by several observers. It will be noted that apart from convulsions the manifestations of uræmia in war nephritis were slight, and that the convulsions were not of evil prognosis.

Another form of acute nephritis is that known as focal embolic nephritis, as seen in infective endocarditis. The starting point of this lesion is not merely a chemical toxin, but the deposit of actual infective material in the kidney by embolism. The septic element thus predominating, it is not surprising that ordinary uræmia is comparatively uncommon in this form. Thus, I have never personally seen uræmic convulsions in this type, while life is seldom prolonged sufficiently for the uræmic manifestations associated with more chronic forms of nephritis to develop.

In chronic parenchymatous nephritis, whether in the form of large white kidney or the secondarily contracted form, all types of uræmic symptoms are comparatively common. But perhaps this is the condition under which paroxysmal dyspnoea is most apt to occur. When the interstitial tissue is largely affected, and the blood-pressure becomes higher, the cerebral manifestations other than convulsions are most likely to declare themselves. When the kidney lesion is due to an ascending infection of the ureters, or to obstruction of the outflow of urine, or to both of these conditions combined, the symptoms are those of urinæmia rather than those of uræmia. Sepsis may, however, play a part in inducing typical uræmia by throwing sufficient kidney substance out of action. Thus in a recent case of glycosuria with persistent *Bacillus coli* infection, in a woman aged 60, uræmic convulsions developed. Soon after this the right kidney became swollen and tender, and there was a copious discharge of pus in the urine, after which there were no more convulsions. Some three weeks later the patient became comatose and died. The occurrence of an acute pyelitis in a chronically infected kidney appeared to determine the onset of uræmia. In another case, an elderly man who had recovered from a severe prostatectomy some four months previously, rapidly became uræmic with hæmorrhagic diarrhoea, when he developed an abscess in his epididymis. Another factor which may precipitate uræmia in chronic interstitial nephritis is inability to absorb sufficient water to keep up an adequate blood volume when the kidney is incapable of excreting a concentrated urine. This I have seen occur in new growth of the œsophagus and stomach.

In contrast to these various conditions of the kidney in which uræmia or urinæmia may occur, I should like briefly to refer to one in which no

such symptoms are to be expected. This is "leaky kidney," by which I mean a kidney that has suffered from an earlier but non-progressive lesion. It allows proteins to escape from the blood into the urine, but it does not retain material which it ought to excrete, nor is it associated with cardio-vascular changes. The ratio of albumin to globulin may be 2 to 1, as in toxæmic kidney, and unlike nephritis, where it is, as I have said, 6 to 1. The reactions to renal permeability tests are all normal, and the principal chemical difference between leaky and toxæmic kidney is that although the diastase output is normal in leaky kidney, i.e., 10 to 33 units, it is never raised above normal, as in toxæmic kidney (Mackenzie Wallis). The clinical distinction is that toxæmic kidney is an acute, while leaky kidney is a chronic condition. The recognition of leaky kidney is important, as it is compatible with normal health and runs a favourable course, despite the large amount of protein in the urine. It is not the material which escapes, but the material which is retained, that leads up to uræmia.

Having thus briefly considered the conditions which do or do not produce uræmia, I will proceed to analyse the significance of the principal factors which I should include in the syndrome, and state my view on their causation.

(1) *Convulsions and Amaurosis*.—I have already said that the most dramatic manifestation of uræmia, namely convulsions, may develop in quite recent cases, and complete recovery may occur, not only from the uræmia but from the nephritis. Convulsions may be the first sign of a kidney lesion, as in the case of a man I saw who more than ten years ago had severe convulsions when apparently in normal health, except for a septic gunshot wound of one eye. His urine was found to be loaded with albumin, and he had many retinal hæmorrhages in the other eye. Yet the whole condition cleared up after excision of the wounded eye. His retina is normal, he has no albuminuria, no cardiac hypertrophy, and no rise of blood-pressure. There was some reduction in his urinary diastase after other signs had cleared up, but even this is now normal. This has not been my experience with other uræmic manifestations, and such cases suggest that the toxin causing convulsions is not the same as those producing other uræmic symptoms. It may be compared to that causing the clinical condition called eclampsia, which, again, may come on very suddenly, and from which recovery may be complete. Now eclampsia has been referred to the effect of a toxin acting on the liver; possibly uræmic convulsions are also thus produced. Amaurosis may occur under the same conditions as convulsions, such as eclampsia, toxæmic kidney, and acute nephritis. Complete recovery is possible in cases showing both these symptoms, and I am inclined to attribute them, therefore, to the same cause. The important point to note is that these symptoms may occur under conditions in which the kidney is not retaining toxins.

(2) *Paroxysmal Dyspnœa*.—It has been established by several observers that acidosis may occur in nephritis from the failure of the kidney to excrete acid sodium phosphate. Such acidosis may reach a higher level in nephritis than in any other condition [6]. Now, when acid is retained in this way, the body will attempt to compensate for it by excreting more CO_2 through the lungs. The normal changes in the alveolar air during digestion show this very well [7]; during the time that the acid gastric juice is being secreted into the stomach, the fall of hydrogen-ion concentration in the blood leads to diminished stimulation of the respiratory centre—consequently CO_2 tends to accumulate in the pulmonary alveoli. During the secretion of the alkaline pancreatic juice,

the hydrogen-ion concentration of the blood rises again, stimulating the respiratory centre to renewed effort, so that CO_2 is washed out from the lungs, and the alveolar air is found to contain less. In health the kidney and the lung can insure between them a fairly constant hydrogen-ion concentration in the blood—any change is soon compensated for. But when the kidney fails to excrete acid sodium phosphate the hydrogen-ion concentration of the blood must rise, and the respiratory centre will be stimulated to compensate for this. The increased respiratory effort in time becomes manifest, and there is obvious dyspnœa. Yet even the marked dyspnœa which constitutes uræmic asthma may prove inadequate to compensate for the acidosis. This unchecked acidosis poisons the heart muscle, cardiac dilatation ensues and pulmonary œdema follows. For it is a familiar clinical observation that uræmic asthma may terminate in pulmonary œdema. It is true that heart failure often excites dyspnœa in nephritis without failure to excrete acid sodium phosphate. But I would restrict the term "uræmic asthma" to paroxysmal dyspnœa occurring in nephritis before signs of heart failure develop.¹

Increased ammonia formation may be looked upon as simply an attempt to combat this acidosis. It has been held responsible for uræmic convulsions, but this is unlikely, as ammonia formation may reach a high level in other conditions without causing convulsions. The three compensatory mechanisms for acidosis are hyperpnœa, protective ammonia formation, and increased output of acid in the urine; in nephritis the last method is interfered with and the other two become inadequate. It might be suggested that drowsiness and coma could be attributed to acidosis, on the analogy of diabetic coma, but Hurler and Trevan have shown that these symptoms in diabetes are due to the enolic group $\begin{array}{c} \text{COH} \\ | \\ \text{CO} \end{array}$ in diacetic acid, and not to acidosis at all.

They are produced as readily by sodium diacetate as by diacetic acid. It would appear, therefore, that paroxysmal dyspnœa stands apart from the other symptoms in the syndrome; in being directly due to acidosis from failure to excrete acid sodium phosphate. That it occurs in paroxysms is presumably due to the temporary diminution of the acidosis which the increased respiratory effort can achieve by removing more CO_2 from the blood. The way in which this paroxysmal dyspnœa may be replaced by Cheyne-Stokes respiration also suggests this.

(3) *Headache, drowsiness, coma, hemiplegia, insomnia and acute mental changes, such as mania or delusional insanity*, which usually occur in chronic lesions of the kidney, although they sometimes manifest themselves with great rapidity. This third group of "uræmic" manifestations appears to be chiefly associated with kidney lesions when vascular changes are paramount. Geoffrey Evans [8] has shown by his histological studies that the essential glomerular lesion in chronic interstitial nephritis is inflammatory in origin. His general conception of nephritis may be summarized thus: The kidney may be affected by pathogenic agents of varying kind and intensity, all of which, however, are capable of inducing active inflammatory reaction in the tissues they attack, whether epithelial or vascular. When acute, the epithelial reaction dominates the picture; when chronic, both epithelial and vascular changes co-exist; in the very chronic, only the vessels suffer, the pathogenic agent being too feeble to evoke reaction in the epithelial tissues, or these tissues being too feeble

Postscript.—In the discussion the importance of heart failure without acidosis, in the causation of this dyspnœa, was emphasized.—W. L. B.

to react, they simply atrophy or degenerate. From this point of view neither vascular nor renal disease is to be regarded as the cause of the other, but both are due to the action of the same toxic agent. According to this author, diffuse hyperplastic sclerosis is more common in the kidney than in any other organ; it is not found in other organs unless present in the kidney or spleen; it is always most marked in these organs, and it is not found in its typical development in the heart, lung, or skeletal muscles. In this connexion it may be noted that the recognized association between chronic interstitial nephritis and cerebral hæmorrhage has been attributed, and I think rightly, to the simultaneous occurrence of similar changes in the vessels of the kidney and the brain. But I think we can now see that this association between vascular changes in these two organs is not limited to cerebral hæmorrhage. We have more recently recognized the extraordinary impermeability of the normal endothelium of the cerebral vessels to toxins. In his study of a case of carbon monoxide poisoning, Mott was able to show that the later cerebral effects were due to fatty degeneration in the capillary endothelium of the brain, following its deprivation of oxygen. In consequence of this vascular change multiple extravasations of blood occurred. Without postulating the constant presence of such a gross lesion as this, we can see how the active inflammatory changes in the cerebral vessels in chronic interstitial nephritis must seriously diminish their impermeability to toxins, and it is to this that the poisoning of the brain must be referred. Often, however, actual vascular lesions such as punctiform hæmorrhages are found in the brain, post mortem, when clinically the case appeared to be one of uræmia.

I should agree with Evans that the difference between diffuse hyperplastic sclerosis and senile arteriosclerosis is one of physiological age response, as expressed both in the kidney and in the brain. With the first are associated cerebral hæmorrhage and uræmia, with the other cerebral thrombosis and a "contraction of the sphere of mental and bodily activity" (Allbutt).

Canti has arrived at somewhat similar conclusions by a different route, and I am indebted to him for kindly putting at my disposal his, as yet unpublished, recent work. Since 1913 he has carried out about 300 observations on urea retention by the hypobromite method. He found the distribution of urea in the body fluids, e.g., the blood, cerebro-spinal fluid, pleural effusion, pericardial fluid, ascitic fluid and œdema fluid, obtained in Southey's tubes, to be always approximately uniform in any one case. The rate at which equilibrium takes place was demonstrated by an experiment carried out in conjunction with Hurtle and Trevan. A dog was anæsthetized and cannulas were inserted into the sub-cerebellar cisterna and into an artery. A large quantity of urea was then injected intravenously and samples of blood and cerebro-spinal fluid were collected every thirty seconds. At first urea was found in abundance in the arterial blood while that in the cerebro-spinal fluid was normal, but in the course of a very few minutes the urea was found in equal concentration in both. Observations on the cerebro-spinal fluid may be therefore taken as indicating the general urea concentration in the tissue fluids. Normally this is 0.02 to 0.05 per cent., though when death from any cause is approaching this figure may be somewhat raised. To eliminate this factor he only accepted percentages above 0.2 per cent. as showing definite urea retention. Taking ninety-six cases in which uræmia was positively diagnosed clinically, sixty showed urea retention and thirty-six did not. He seldom found serious urea retention in cases which recovered, and all cases with a urea content of

0·3 per cent. or more in the cerebro-spinal fluid were fatal. The highest figure he obtained was 0·88 per cent. Of cases diagnosed as uræmia in chronic nephritis, two classes could be distinguished. The first and larger class showed urea retention—all of these died. The second showed no retention; some of these recovered, at any rate for the time being, while all of the fatal cases in this class showed cerebral lesions such as hæmorrhages, embolism or thrombosis. Cases diagnosed as uræmia with the arteriosclerotic type of kidney also fell into two classes. Here the first class, showing urea retention, was the smaller; the retention was not extreme, rarely above 0·3 per cent., but the cases were all fatal. The second class, without urea retention, was the larger; some of these survived for the time being, the rest died. He considers that many of these cases without urea retention showed signs of cardiac failure, and that treatment directed towards combating this was the only thing which helped them at all. If the uræmic syndrome is to be regarded as essentially toxic, it follows that such cases must be excluded.

One criticism I made to him on these observations was that the hypobromite method evolves nitrogen from other nitrogenous bodies besides urea, so that I would prefer to speak of retention of non-protein nitrogen. Although urea would be the most abundant constituent, it need not be the toxic one, nor, indeed, is it likely to be. He then compared the yield to the hypobromite test with that given by the urease test, which would show the amount of urea only, without the other forms of non-protein nitrogen. The figures he obtained in one case of mine were very striking. The blood urea by the urease test was 0·4 per cent. Analysis of the cerebro-spinal fluid gave 0·69 per cent. by the hypobromite method, and 0·45 per cent. by the urease method. The amount of "unknown nitrogen" was, therefore, greatly raised, both proportionately and absolutely, being 0·24 per cent. Expressed proportionately this was nearly 35 per cent. of the total instead of the usual figure of about 8 per cent. And this is typical of other cases. I would suggest that it is this development of non-protein nitrogen other than urea which is the real toxic element in these cases. For urea is not a toxic substance.

I consider that Canti has shown the existence of two definite groups of cases which have not been discriminated hitherto: one in which a chemical factor can be detected—azotæmia, in the widest sense; and another in which cardio-vascular conditions, particularly cerebral-vascular lesions, are responsible. I think that grouping together a large number of symptoms under the term of "uræmic syndrome," and trying to find one toxin responsible for them all have tended to obscure the issue and to discourage attempts at detailed correlation of symptoms with their actual causes.

I would express my position somewhat as follows: (1) The intense action of a toxin may produce a profound degenerative or inflammatory change in the renal epithelium, which may be associated with convulsions. As cerebral and renal vessels are liable to similar lesions it may well be that these convulsions are due to exudation, even of blood, from the cerebral vessels. In the more chronic cases punctiform hæmorrhages have been found in the brain in patients dying with convulsions. From the existence of similar changes in the liver and from the analogy of eclampsia, it is at least possible that the liver is as much responsible for this symptom as the kidney. The work of the kidney is elimination, and in toxæmic kidney this at least is not impaired. Part of the work of the liver, on the other hand, is detoxication, which would be seriously impaired by damage to the hepatic cells. Convulsions and amaurosis are the only "uræmic" manifestations from which complete recovery is possible.

(2) The retention of acid salts by a damaged kidney can produce such a high grade of acidosis that marked dyspnœa, generally paroxysmal, may result. This is most likely to occur in more chronic lesions of the tubules.

(3) In the most chronic inflammatory vascular lesions of the kidneys similar changes take place in the cerebral vessels, which allow a soaking into the brain of toxins, probably amines other than urea, which produce headache, drowsiness, coma and various less common cerebral symptoms. Grosser vascular lesions such as hæmorrhages or thromboses may also result. And Nordman [9] has found the characteristic lesions of chronic meningitis in chronic uræmia.

(4) Severe obstruction to the outflow, on the other hand, is more likely to lead to symptoms of urinæmia, as defined by Ascoli. The clear-cut features of the different types are often blurred, however, by the co-existence of several lesions. Thus, the urinæmia of prostatic origin is very likely to be complicated by vascular changes as well, considering the age at which prostatic troubles occur, while the septic complications to which nephritics are so prone are apt to alter the manifestations of uræmia at any age.

(5) Some cases of apparent uræmia are really cases of cardiac failure and only respond to treatment for this. It would therefore appear that even using the term in a restricted sense, the uræmic syndrome may be due to at least two different chemical substances, one nitrogenous and the other non-nitrogenous, in addition to vascular lesions and septic complications.

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DISCUSSION.

Dr. J. R. MARRACK said he considered that paroxysmal dyspnœa in nephritis was due not to acidosis, but to vascular disturbances. In the most severe cases of dyspnœa he had seen, the plasma bicarbonate had been normal and the plasma reaction normal or on the alkaline side; the blood-pressure of these patients had been very high, and in some the renal efficiency had been only moderately reduced. On the other hand, hyperpnœa might not be striking, even in cases of extreme acidosis, with plasma bicarbonate of 20 or under, and when it occurred it was a deep and undistressed breathing, very different from the violent efforts of dyspnœa. Other disturbances usually classed as uræmia occurred in patients with high blood-pressure and fair renal efficiency, with no evidence of retention; among such disturbances were mental changes, such as delusions of persecution, transient unconsciousness, and paralyses, headache and vomiting. Apart, however, from these cases, which appeared to be explained by vascular changes, the majority of cases of uræmia had high blood urea, and the obvious explanation of the condition was that it was caused by retention of some poisonous products of metabolism. However in occasional cases, which could be classed in the vascular group, coma and convulsions occurred although the blood urea was normal and the renal function only moderately impaired. In contrast with these there were others with extreme renal impairment, blood urea up to 0.6 per cent., high inorganic phosphates, uncompensated acidosis, altered distribution of Cl between plasma and tissue

fluids, great change in plasma proteins, and yet no evidence of uræmia—or, at most, drowsiness and headache. It was difficult to reconcile such cases with an explanation of uræmia as due to retention, or to any of these changes. It was possible that some of the changes were compensatory, and that uræmia in the cases with no evidence of retention, was due to slight retention or change of distribution, without the compensatory change. In some cases uræmia, especially muscular twitchings, appeared with reduction of plasma Ca, but the plasma Ca was not reduced in all cases of uræmia.

Dr. G. A. HARRISON said that acidæmia could not be an important factor in the causation of renal dyspnœa, because renal dyspnœa might occur in the absence of acidæmia, and vice versa. Professor Fraser¹ and his collaborators had recently published a paper demonstrating this point. They had determined the hydrogen-ion concentration of the arterial blood by the colorimetric method of Dale and Evans. A case of "Lewis's dyspnœa," under Sir Charlton Briscoe, had been similarly investigated at King's College Hospital by Professor Lovatt Evans, and was interesting in this connexion. This patient had marked hyperpnœa with Cheyne-Stokes breathing and no cyanosis. When first examined he was on large doses of alkali and his blood was alkaline (PH 7·81). Later the alkali was discontinued. His dyspnœa very definitely improved, although his blood was now slightly more acid than normal (PH 7·50). His systolic blood-pressure varied between 148 and 194 mm. Hg. He had slight albuminuria, and very occasionally granular casts were found. The Wassermann reaction was strongly positive. He finally died in typical uræmia. Post mortem, the kidneys were granular and contracted, weighing 2 and 2½ oz. respectively. Dr. P. P. Laidlaw and Dr. E. ff. Creed had kindly examined the sections which they had regarded as characteristic of arteriosclerotic kidneys. There was also slight fibrosis of the arterioles of the medulla oblongata. This case therefore provided some evidence in favour of the view that renal dyspnœa was due to disease of the vessels supplying the respiratory centre.

Dr. F. PARKES WEBER alluded to the apparently mechanical factor in the causation of certain cases of uræmic convulsions. Young adult patients were occasionally met with in whom universal renal dropsy, commencing acutely, lasted for months and months, until they were regarded as almost hopeless cases of "large white kidney." They required repeated tapping of ascites, hydrothorax and subcutaneous œdema of the lower extremities. At last the dropsy might completely disappear, almost spontaneously, within a week or so, owing to a regular "diuretic crisis." The patient could get up and resume active life, though more or less albuminuria persisted. Some of these cases, *but not all*,² were connected with early syphilis³ (possibly even before or without the occurrence of secondary syphilitic cutaneous manifestations). Vomiting and headache were sometimes present, but one of the greatest dangers was the onset of uræmic convulsions, which in this class of case was likely to be followed by death. It seemed as if the uræmic convulsions in such cases, when they occurred, were due to a kind of overflow of the dropsical effusions from the subcutaneous tissues, peritoneum, and pleuræ, into the cerebro-spinal system, thus giving rise to excess of cerebro-spinal fluid (and possibly to œdema of the meninges and brain itself) and cerebral anæmia by compression of the brain. This view was certainly supported by the modern teaching of F. Volhard, in his excellent book on "Die doppelseitigen hämatogenen Nierenerkrankungen"; obvious preventive therapeutic measures were suggested in that work.

Mr. R. H. PARAMORE asked whether the changes in the kidneys and liver which led to uræmia were always due to a toxin: he was of the opinion that some toxæmias

¹ F. R. Fraser, J. P. Ross, and N. B. Dreyer, "The Reaction of the Blood in Relation to Dyspnœa," *Quart. Journ. Med.*, 1922, xv, p. 195.

² F. Parkes Weber and H. Schmidt, *International Clinics*, 1917, ser. 27, iii, p. 151 (case I).

³ F. Parkes Weber and H. Schmidt, *ibid.*, 1916, ser. 26, i, p. 89.

ending in acute uræmia were caused by pressure—notably that occasionally occurring in pregnant and parturient women and spoken of as eclampsia. In 1909, Dr. Parkes Weber described a case of anuria with necrosis of the convoluted tubules occurring in a man of 69 who died from carcinoma of the prostate.¹ The case was significant; he (Mr. Paramore) believed the explanation was to be found in mechanical aberrations induced by the disease. He asked whether the lesions of the renal convoluted tubules, caused by poisons (taken by the mouth) affected these tubules uniformly; and pointed out that in eclampsia, the cortical necrosis was more or less limited to the outer two-thirds of the cortex.² If the necrosis were due to a toxin, he thought the tubules would be uniformly affected: the fact that the parts nearest the medulla escaped indicated that the lesion was the result not of a toxin but of another cause. He agreed with those who said that not only the kidneys but the liver also were affected in patients exhibiting uræmia. The question was: What caused the lesions? Considerable stress had been laid on gastro-intestinal disturbances preceding the visceral changes associated with uræmia. Whilst he readily agreed that this was so in chronic forms of the disease, he was unable to allow that an acute uræmic toxæmia was caused in this way.³ The Dublin School of Obstetricians attributed eclampsia to poisons arising in the intestinal tract; but this he thought untenable in view of the relatively short period of gestation. Prostatic disease threw considerable light on the matter. The vomiting of altered blood by patients during the first day or two after prostatectomy, noted in a case of his own in which it was associated with great restlessness, and also referred to recently by Sir John Thomson-Walker (President of the Section) at the Harveian Society,⁴ indicated that the liver circulation was disturbed. In the absence of gastric and other intestinal ulcerations, and rupture of œsophageal veins, the vomiting of altered blood could only be due to the bursting of gastric capillaries, which a clamping of the liver capillaries—determined by the movements, the restlessness, the cries and the strains of the patient—explained. Mention had been made of hæmorrhagic diarrhœa occurring with an epididymitis sometime after prostatectomy (and hæmorrhagic intestinal extravasations were not uncommon in eclampsia): that also must be attributed to hepatic circulatory difficulties. Such changes were to be imputed to the manner of living of the patient *during his disease*—to his movements, &c. That the kidneys were affected in the same way by the same cause, he thought was plain. Recognizing in his own case that the hæmorrhagic vomiting was due to the increased intra-abdominal pressure determined by the condition of the patient (a well-built rather stout and strong man), and that this pressure was less in the lateral than in the supine position, and much diminished by quieting the patient; he (Mr. Paramore) turned his patient on to the left side and induced sleep, with the happiest of results. His surprise was complete and his interest great when he found the President of this Section in his recent paper,⁵ already mentioned, also advised that stout patients should be made to lie on their sides. It was interesting to notice that the Dublin school of obstetricians insisted on the lateral position in the treatment of eclampsia. They thought that this prevented the patient drowning from the accumulation of mucus: but the more subtle change in the pressure conditions to which he had alluded had not yet been considered. The suppression of urine occurring after the rapid emptying of a urinary bladder greatly distended (as in prostatic disease) supported his view that inactivity of the kidneys, and so uræmia, were at times occasioned by physical causes. He cited the opinion that in the pre-eclamptic state, Cæsarean section was occasionally followed by fits; and that when this operation was done for eclampsia itself, the patient occasionally was apparently made worse.⁶ A more remarkable case was that published

¹ *Lancet*, 1909, i, p. 601.

² Jardine and Kennedy, *Lancet*, 1920, ii, p. 120.

³ This wants defining—but for the moment can stand. Perhaps we are on the track here of acute yellow atrophy.

⁴ As reported in the *Lancet* of November 25, 1922, p. 1121.

⁵ *Lancet*, November 25, 1922, p. 1121.

⁶ From Dublin (private communication).

in the *Bulletin* of the Paris Obstetrical and Gynæcological Society, in 1920,¹ of a rachitic primigravida operated on at term. She was 24 years old, and was first seen when seven months pregnant—Cæsarean section at full time being advised. From that time until term, the urine was examined twice every fortnight, no albumin being discovered. On the morning of the operation (done at 10 a.m.), the urine contained no albumin and no toxæmic symptom was present. At 4 p.m. the same day, an eclamptic fit occurred followed by coma, and the catheterized urine was found to be slightly albuminous. Two further attacks occurred, and then recovery. He (Mr. Paramore) thought this case and the suppression of urine following the rapid emptying of the greatly distended urinary bladder in prostatic disease were in line; and that just as a mechanical factor explained the one, so it also explained the other. What were the lesions found in the kidneys in these patients who died as a result of suppression of urine? Closer attention to such details was urgently called for, and it was necessary to break away from the tradition that all visceral diseases were due to toxic agencies. Man was very largely a mechanical being, and if the mechanism went wrong some chemical interaction or other was bound to get out of order.

¹ *Bull. de la Soc. d'Obst. et de Gyn. de Paris*, 1920, xix, p. 444.

Section of Urology.

President—Sir JOHN THOMSON-WALKER, F.R.C.S.

Case of Cyst of Prostate.

By Sir JOHN THOMSON-WALKER, F.R.C.S.

J. N., MALE, aged 43, examined February 9, 1920. For four weeks had complained of a poor stream and straining to pass water and frequent micturition. Three days before I saw him a catheter was passed. This was repeated twice daily, and from 14 to 30 oz. of residual urine removed. Apart from use of catheter there was frequent desire, but very little urine was passed. He was a strong healthy man, and there were no senile changes or signs of nervous disease. The prostate, as felt from the rectum, was elastic and movable, and very slightly larger than normal.

On cystoscopy there was a rounded bulging on the left of the internal meatus, the outline of which was otherwise normal. The bladder was slightly trabeculated, and the ureteric orifices normal. On opening the bladder a large soft cyst the size of a walnut was found arising from the prostate at the anterior and left edge of the internal meatus. The prostate was not enlarged. I shelled it out with some difficulty, and it carried the cyst with it.

Recovery was uneventful, and there was no residual urine.

The specimen shows a prostate, little if at all enlarged, and on the upper surface there is a thin-walled cyst the size of a walnut.

Case of Myosarcoma of Epididymis.

By Sir JOHN THOMSON-WALKER, F.R.C.S.

M. M., MALE, aged 32, examined June, 1922. Patient noticed a lump attached to the testicle about twelve months ago. This had gradually increased in size, especially during the week previous to examination, but there had not been any pain. There had been no injury, and the patient denied venereal disease. In the globus major of the right epididymis there was a very hard round nodule, the size of a marble. The surface was smooth, and was not adherent to secondary structures. The testicle, remaining portion of epididymis and vas deferens were normal, and nothing abnormal was found elsewhere in the genital organs. The diagnosis lay between an unusual form of tuberculous epididymis and growth.

Epididymectomy was performed, and on section the nodule was found to be a myosarcoma.

Fifteen months later there was no recurrence. He was lost sight of after that time.

Case of Aberrant Prostatic Nodule.

By Sir JOHN THOMSON-WALKER, F.R.C.S.

C. A., MALE, aged 61, examined July, 1922. Complained of an inflamed pile, and on examination of the rectum a greatly enlarged prostate was discovered. He had never had any difficulty in or urgency of micturition. There was slight increase in the frequency of micturition to six or seven times during the day and once during the night, and the stream was rather weak and finished off slowly. He was a thin, very active man, who had lived in India and China for twenty-two years, and had returned to England eighteen years ago. The abdomen was flat. There was a prolapsed thrombosed pile. There was no distension of the bladder or enlarged groin glands. The urine was normal. The prostate was small, elastic and movable. Above and to the right of the prostate was a large firm elastic mass, which was rounded somewhat, and nodular. On one part of the surface a cyst could be felt. The mass was separated from the prostate by a groove and occupied the position of a greatly enlarged seminal vesicle. There was no shadow shown by the X-rays.

On August 16, 1922, I opened the bladder suprapubically. There was some bulging at the right base behind the trigone. On palpation, the greater part of the mass was in this situation, but it extended over to the left as far as the level of the left ureteric orifice. The trigone and ureteric orifices were normal, and there was no change at the internal meatus. A median incision was made from just behind the posterior lip of the internal meatus through the trigone muscle and base of the bladder. A rounded, elastic, nodular mass was exposed, and was enucleated by blunt dissection. It was surrounded by a smooth capsule which was slightly adherent, anteriorly and laterally, but there was no pedicle, and no connexion could be traced with the prostate or seminal vesicles. The latter were not seen during the dissection. The resulting cavity was drained, and the floor of the bladder united.

The specimen was a firm, elastic, round body, the size of a tangerine orange. The surface was smooth, and at parts shows bosses due to small cysts.

Section showed firm, opaque white tissue, in which were embedded numerous cysts filled with milky fluid. Microscopic section showed a fibro-muscular stroma with glandular alveoli similar to the gland tissue of the prostate. The alveoli were dilated with cysts of varying size, and the whole structure was that of senile enlargement of the prostate. There was no sign of malignant changes at any part.

Post-operative examination confirmed the observation that the prostate was normal in size and consistency.

Case of Malakoplakia.By Sir JOHN THOMSON-WALKER, F.R.C.S., and
F. J. F. BARRINGTON, M.S.

MRS. L., aged 43, examined October, 1914. Four months previously noticed pain at the end of micturition which gradually increased. Terminal hæmaturia appeared about the same time and increased until the hæmorrhage was severe for about fourteen days, when she was admitted to hospital. The pain and hæmorrhage were increased by movement. The frequency of micturition was increased to about six times during the day and twice at

night, and some days the interval was reduced to a few minutes. She had resided and travelled abroad. Two years ago, and again four years ago, she had visited Burma, and since 1897 had lived in India, with intervals of some months in England, and had passed through Ceylon and Egypt.

There was no family history of tubercle. Ten years ago she had pleurisy, and as a child had malaria and pleurisy.

On cystoscopy the bladder held 12 oz. without pain, and there was no difficulty in getting a clear medium. The whole of the mucous membrane of the bladder, with the exception of the trigone and a small area behind this, was strewn with yellow plaques, which varied in size from a very small point to an area the size of a threepenny piece. Viewed from a distance the colour was yellowish pink, but on closer inspection it was pale yellow. On the posterior and lateral walls of the bladder the plaques were discrete and distribution was very irregular. Behind and to the outside of the left ureteric orifice there were a number of large plaques, and to the right of the trigone there was an area closely covered with plaques which had become small nodules. A plaque was flat or nearly so on the surface, it had a rolled-over edge and stood up sharply from the mucous membrane. The central part was frequently a little depressed, somewhat like the nodules of molluscum contagiosum. They were rounded or oval. Closely examined, they had the appearance of a caseous nodule covered with a thin layer of epithelium. Occasionally the base was a little contracted so that the edge rolled over. Around the edge of the plaque there was a halo of moderate inflammation in a few plaques, but in most of the plaques there was no sign of reaction in the mucous membrane around, and there was no ulceration. The mucous membrane apart from the plaques was healthy. The ureteric orifices were normal with a slight reddening of the lips.

On November 2, 1915, the bladder was opened suprapubically. The area at the apex of the bladder was found thickly studded with plaques, and they extended down the posterior wall. The plaques could be distinguished with the finger as raised areas in the mucous membrane. In parts they felt leathery and the isolated plaques were hard to the touch and felt like a plaque of xanthelasma, or a hard chancre on the foreskin. A number of the plaques were excised and the remaining areas treated with solid nitrate of silver. The plaque was entirely confined to the mucous membrane and did not penetrate, and was not adherent to, the muscular coat.

Histological Examination.—A section of plaque stained with hæmatoxylin and eosin shows that it consists of a mass of cells situated immediately deep to the position of the epithelium. The epithelium lies directly on the cell mass at the sides but over the central part it is absent. On the deep surface a layer of the submucous areolar tissue separates the cell mass from the muscular coat of the bladder. Interstitial tissue in the cell mass is very scanty and is represented by a few blood-vessels and fine strands of areolar tissue which have a general direction vertical to the surface. The majority of the cells of the mass are large with a finely spongy eosin-staining cytoplasm and a small deeply-staining eccentrically situated nucleus. In the deeper parts of the plaque these cells are polygonal from mutual pressure, but near the surface, where they are arranged more loosely, they tend to be rounded and slightly larger. Michaelis-Gutmann bodies are present in many of the large cells, sometimes singly and sometimes as many as five in one cell. These bodies are more or less round, and vary in size up to about twice that of the cell's nucleus: they stain much more faintly with hæmatoxylin than do the

nuclei, some appear homogeneous, while others have a definitely concentric structure. At the edges and base of the plaque there are numerous small round cells; most of these are mononuclear, and at the base of the plaque they show a marked tendency to be arranged in closely packed foci. A few scattered large cells of the same kind as those forming the plaque are present in the areolar tissue deep to its base. Some of these large cells contain Michaelis-Gutmann bodies. A few Michaelis-Gutmann bodies of unusually large size are found in the zone, outside the cells. No bacteria are present in the section.

Necrosis of Kidney following Ligature of Abnormal Renal Vessels.

By W. GIRLING BALL, F.R.C.S.

THIS specimen was obtained from a woman, aged 35, who was admitted to St. Bartholomew's Hospital on February 25, 1922. It shows the kidneys, pelves and ureters, together with the blood-vessels attached thereto. The main point of interest is the necrosis of the lower pole of the left kidney which followed the ligature of an abnormal renal artery and vein, which had caused kinking of the ureter and a consequent large hydronephrosis. The line of suture, after excision of part of the wall of the sac, can be seen and it demonstrates the straightening of the ureter which followed this procedure. The ligatured vessel has been dissected out, and is seen to come off as a separate vessel directly from the aorta. An abnormal artery is present on the opposite side and shows a kinking of the ureter of that kidney. There is another large vessel below the main artery which is possibly the remnant of a previously ligatured vessel.

The microscope slide shows the necrosis of the renal tissue at the lower pole of the kidney.

The history of the case was that of renal colic associated with a swelling on the left side of the abdomen and polyuria extending over a period of fourteen months. The patient related that she had had a similar condition on the right side, giving symptoms for twenty-one years; for this she was operated on and had an abnormal renal artery tied two years previously. This is interesting seeing that the specimen still demonstrates the presence of such a vessel; she had not had any symptoms relating to the right side since the operation. The diagnosis of kinking of the left ureter was made by pyelography, the X-rays showing a typical picture.

She was operated upon on March 13, 1922. A considerable hydronephrosis was found, resulting from the kinking of the ureter at a very acute angle over two vessels entering the lower pole of the kidney. These vessels were large in size, but the main renal vessels appeared to be normal. The accessory vessels were ligatured and divided. A V-shaped portion of the posterior wall of the hydronephrosis was excised and sewn up again with a double layer of sutures; in this way the kink in the ureter was straightened. After this the kidney at first appeared engorged at its lower pole, but subsequently its normal colour returned. The wound was drained.

On the day following the operation the patient's abdomen became distended, but relief was obtained by the use of enemata; the distension, however, increased despite the passage of flatus. There was no leakage of urine from the wound or evidence of perirenal infection. She died suddenly on the third day.

Post-mortem.—There was cedema and a certain degree of suppuration about the lower pole of the kidney and the upper end of the ureter. In front of the pole of the kidney was a small suppurating focus, attached to and infecting the peritoneum to the inner side of the colon. The peritoneum was intact. Here there was a small focus of suppurative peritonitis, with a spreading general peritonitis and great distention of the small intestine. The sutured pelvis did not show any evidence of leakage.

Specimen showing Transitional-celled Growth of the Kidney.

By W. GIRLING BALL, F.R.C.S.

THE specimen shows a kidney, the upper pole of which is occupied by an oval tumour $1\frac{1}{2}$ in. in diameter. The tumour is surrounded by a capsule composed mainly of a condensation of the surrounding kidney. It is of fairly uniform appearance, semi-translucent, and of a pale yellow colour; the clear-cut surface presents a somewhat granular structure. Projecting into the pelvis of the kidney and extending down the ureter for about an inch is an extension of the growth of similar appearance, except that there is no surrounding capsule, and the tissue is superficially somewhat necrotic. This extension of the main mass has produced some blockage of the ureteric orifice, which has resulted in the production of a condition of hydronephrosis in the portion of kidney free from the tumour. The renal tissue here is almost completely absent.

Microscopic Appearances.—The microscopic section of the growth shows that the tumour is composed of transitional cells of the type met with in the lining of the pelvis, ureter and bladder. The cells are arranged as an epithelial membrane, but much thicker than normal, which is thrown into a large number of folds upon a fine supporting stroma containing blood-vessels. This is the fundamental arrangement, but here and there the structure is more complicated and cell proliferation is more active and of a less regular type. The section does not show actual invasion of the kidney by this growth, which originated in the pelvis. It is a transitional-celled papilloma, which exhibits some appearance of malignancy.

The kidney was removed from a female patient, aged 38, in September, 1922, who had had almost persistent hæmaturia for a period of ten months. This was associated with pain and swelling in the left loin. A curious feature in the bleeding was the fact that it only occurred in the morning or after lying down, disappearing completely during the day. The pain was no doubt caused by the hydronephrosis, and the intermittent character of the hæmaturia by the temporary blockage of the ureter by the tongue of growth. There was no evidence of metastasis. The patient made an excellent recovery and has remained well up to the present.

Absent Right Kidney ; Deformity of Left Ureter.

By W. GIRLING BALL, F.R.C.S.

THE cystoscope pictures illustrate the following case : A boy, aged 9, was suddenly taken ill on August 8, 1922, with abdominal pain and vomiting. His doctor discovered a sausage-shaped swelling on the left side of the abdomen extending from under the ribs to the level of the brim of the pelvis ; it could be

36 Ball: *Absent Right Kidney*; Sutcliffe: *Two Large Calculi*

felt in the pelvis in front of the rectum. There were no urinary symptoms. This swelling proved on exploration to be an enormously distended left ureter, which was opened extraperitoneally and found to be connected with a distinctly hydronephrotic kidney; the cause of the dilatation could not be discovered. The hole in the ureter was sewn up.

The boy remained well for a fortnight, when the ureter again filled up, but on this occasion it emptied itself in the bladder on the application of hot fomentations to the abdomen. A week or so later it again filled and this time discharged itself through the incision in the loin. Occasionally after this the boy passed his water by the bladder and then the fistula closed, but when there was leakage through the fistulous track he did not pass any water from the bladder at all. It thus seemed probable that there was no right kidney present. Efforts were then made to prove this, and also to find out the cause of the obstruction to the left side.

Cystoscopic Appearance.—(1) Marked twisting of the ureteric orifice on the left side, and the absence of an orifice on the right. There was no obvious prolapse of the left orifice. (2) Repeated X-ray examination failed to demonstrate the presence of a left ureteric calculus or the presence of either kidney. (3) The injection of dyes failed to demonstrate the presence of a right ureteric orifice. (4) As the boy now had a fistula from what appeared to be his only kidney, something had to be done.

Operation: The peritoneal cavity was first opened. There was no evidence of a right kidney, rudiment of such, or of a ureter. The left kidney was very large and cystic. The peritoneum was closed and the incision extended downwards to the suprapubic region. The bladder was opened and the ureteric orifice examined. There was no prolapse. The bladder was then drawn out of the pelvis and the peritoneum pushed up, and the distended ureter traced down to the point of its insertion into the bladder; the last inch of the ureter was normal in calibre, and from that point upwards the dilatation commenced without obvious cause. The normal portion of the ureter was divided between ligatures. The upper portion was freed and divided above the point of narrowing and implanted into the bladder at a point higher up on its wall. The bladder was drained suprapubically. The wound rapidly healed and the boy has remained well since.

Appearance seen in November, two months after the operation: The orifice stands open, but contracts down considerably as if at the termination of a peristaltic wave from the ureter; it does not completely close. The boy remains quite well.

Two Large Calculi removed from the Perinæum of a Male, aged 62, in Margate Cottage Hospital.

By W. G. SUTCLIFFE, F.R.C.S.

(Shown by CYRIL NITCH, M.S.)

THE patient had suffered from a perineal urinary fistula complicated by frequent perineal abscesses following an operation forty-two years ago for a ruptured urethra in University College Hospital. During this period he had never consulted a doctor. Some urine was passed by way of the urethra, but the greater part escaped through the perineal fistula. When seen by Mr. Sutcliffe in April, 1922, two hard, rounded masses were felt occupying the region between

the anus and the peno-scrotal angle. A sound passed by way of the urethra grated against a stone. At the operation, by Mr. Sutcliffe, the stones were found to occupy a large sac which communicated with the urethra. The large stone weighed $13\frac{1}{2}$ oz. and the small one $1\frac{1}{2}$ oz. An attempt was made to close the sac by suture, but the fistula soon re-formed. Both stones were found to consist of calcium phosphate.

Specimens of New Growth of the Pelvis and Kidney.

By J. McALPINE, M.B.

(I) SIMPLE PAPILLOMA OF THE RENAL PELVIS.

THE specimen was removed from a man, aged 45, who presented himself as the result of a copious hæmaturia of sudden onset. This was accompanied by clot colic. In other respects the history of the case was painless. It is worthy of notice that the organ was palpable, and was diagnosed as being increased in size. I find from a glance at the literature, that it is uncommon for papillomata of the kidney to give rise to an increase in size of the organ, unless this increase be dependent on the development of a hydronephrosis as the result of blocking of the ureter by the growth; there is no hydronephrosis in my specimen. I should say that the papilloma is one of fairly considerable size, though as I have not myself seen a great many of these specimens, I am unable to compare it with others. It presents all the characteristics of a papilloma as seen either in the bladder or elsewhere. There are numerous secondary splashes on those parts of the pelvis which are not primarily affected. There was one point of interest in the post-operative history; the patient suffered from clot colic shortly after coming round from the anæsthetic, and for a period of thirty-six hours the urine was stained with blood. At the operation I had removed just as much of the ureter as I could conveniently by the loin incision, the danger resulting from secondary implantation in the ureter having temporarily escaped me. On the bleeding occurring I did not, however, submit the patient forthwith to a complete ureterectomy, and as events have turned out, it is probable that the colic and hæmorrhage were due to blood which had been squeezed out of the papilloma by the manipulations at operation. It is now six years since I removed this kidney and I have kept the patient under observation at frequent intervals, and have cystoscoped his bladder periodically; there is no suspicion of recurrence. The possibility, even probability, of secondary implantation in the ureter should, I think, make one resort to nephro-ureterectomy rather than nephrectomy in all such cases.

(II) MALIGNANT PAPILLOMA OF THE RENAL PELVIS.

This is perhaps more interesting as a specimen. It was removed from a male patient, aged 56, whose only symptom was hæmaturia. The diagnosis of the site of origin of hæmaturia was complicated by the fact that he also presented a medium prostatic bar, which tended to bleed on the introduction of the cystoscope. The hæmorrhage was, however, traced to the left ureteral orifice and the opposite kidney being diagnosed as of adequate capacity, the present specimen was obtained. This organ was not palpable in the loin, and the patient had never suffered any pain or discomfort attributable to the gland.

The specimen is that of a kidney of normal size, and there is no lesion of the capsule or parenchyma apart from the tumours calling for comment. In the pelvis may be seen a number of small sessile, papillomatous tumours none of which is larger than a millet seed, springing from the mucosa. One of these is to be observed occupying a position high up in a renal calyx, and in close proximity to the two tumours of the parenchyma.

One of these two latter tumours is about $\frac{3}{4}$ in. in diameter, and occupies the mid-zone of the kidney, being situated more in the medulla than in the cortex; it is soft and encephaloid in consistency, maroon-coloured, and gives the impression of being partially encapsuled.

The second parenchymal tumour has a different appearance, it is scirrhus in type, white and glistening, and is evidently infiltrating the kidney substance. I submitted this specimen to Professor Dean, who was then Professor of Pathology at the Manchester University, and he cut for me the three slides which I have exhibited this evening. He is of the opinion that the growth in the pelvis of the kidney is primary and that each of the two growths in the parenchyma is secondary, one having taken on the encephaloid type and the other the scirrhus type.

This nephrectomy was performed about fifteen months ago and I was called to see the patient within the last month. I found he had done well until within a fortnight of my seeing him, when he began to show signs of cachexia. On examination I found a considerable mass occupying the left upper quadrant of his abdomen, which was evidently a neoplasm secondary to the kidney lesion.

(III) MASSIVE CALCULUS FORMATION IN THE KIDNEY.

The last specimen is only interesting as a specimen, but it illustrates also the silence of patients suffering from calculus of this kind. The patient was a man, aged 29, who had never suffered from any symptoms indicative of stone formation in his kidney, with the exception of an occasional slight ache in the loin. The symptom which brought him to me for treatment was again hæmaturia; the absence of pain with such enormous masses of stone in the kidney is indeed remarkable.

A Large Renal Calculus.

Shown by R. OGIER WARD, F.R.C.S.

A LARGE renal calculus, composed throughout of ammonium magnesium phosphate with a little calcium oxalate and on its surface some calcium carbonate: weight, 555 grm. (1 lb. 2½ oz.). Also a ureteric calculus of the same composition: weight, 2·8 grm. These, with the kidney and ureter, also shown, were removed from a man aged 58. For twenty-three years the patient's urine had been intermittently blood-stained; sometimes there had been sufficient hæmorrhage to cause retention of urine in the bladder from clot formation. He had never had any renal or vesical pain. For five years recently his urine had smelt offensively. For two weeks only he had had marked frequency of micturition, fever and some rigors.

On examination the left kidney could be felt to be greatly enlarged and unusually firm. The urine was heavily infected with a coliform bacillus, and with cocci. Cystoscopy showed acute cystitis, a swollen left ureteric orifice, from which there were frequent purulent effluxes. The prostate was enlarged.

X-rays showed the ureteric calculus and a dense shadow in the left renal region. Nephrectomy was performed in the usual manner, 8 in. of greatly thickened ureter and the ureteric calculus being also removed.

The renal calculus occupied the greatly dilated and thickened renal pelvis, much of the kidney remained in a fairly healthy condition, but showed on microscopical examination a condition of chronic interstitial nephritis.

Two Cases of Glandular Epispadias.

By J. SWIFT JOLY, F.R.C.S.

GLANDULAR or balanitic epispadias is the most uncommon form of this condition. In 1904 Katzenstein¹ could only collect five cases from the literature, and I do not know of any others since reported. As this condition is so rare, I thought it might be of interest if I described two cases which have recently come under my care.

Case I.—Well-developed boy, aged 12. At first sight the external genitalia appeared normal, except that the penis was slightly stunted. The prepuce was long, and in its normal position entirely hid the defect. When it was fully

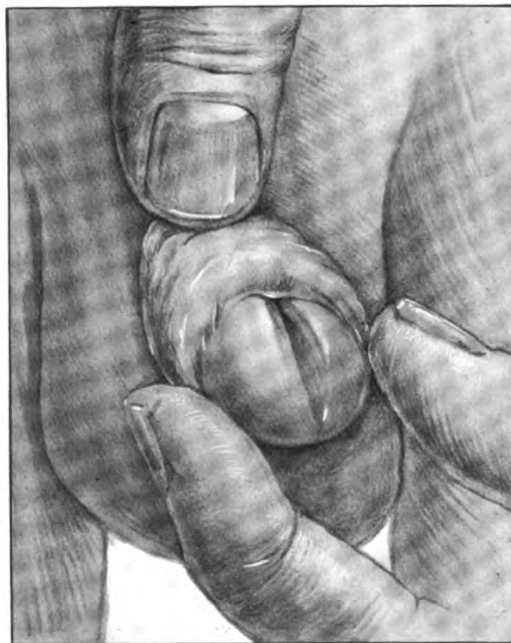


FIG. 1.—Case I.

retracted, it was seen that the urethra opened on the dorsal surface of the penis, immediately behind the corona glandis. A deep groove extended from this point to the usual position of the external meatus. It involved the whole thickness of the corona, but in front of this point it only extended about half-way through the glans (fig. 1). Its greatest depth was 6 mm. The groove was

¹ Quoted by Nové-Jusserand and Gayet, "Encyclopédie française d'Urologie," v, p. 900.

lined with mucous membrane, which was of a much deeper pink than the epithelial covering of the glans. There was no stricture. A No. 12 F gum-elastic bougie was passed, and the urethra palpated when it was in place. The corpora cavernosa were separated from each other, and the urethra could be felt with equal ease from the dorsal and from the ventral aspects of the penis. On perineal and rectal examination the course of the urethra appeared to be normal. The testicles were well descended, but rather small considering the age of the boy. The pelvic girdle appeared to be normal, but no X-ray examination was made. As the boy was in excellent health, and suffered no inconvenience from this defect, no operation was advised.

Case II.—Well developed boy, aged 7. At birth the penis was attached to the abdominal wall by a thin web involving only the skin. This was

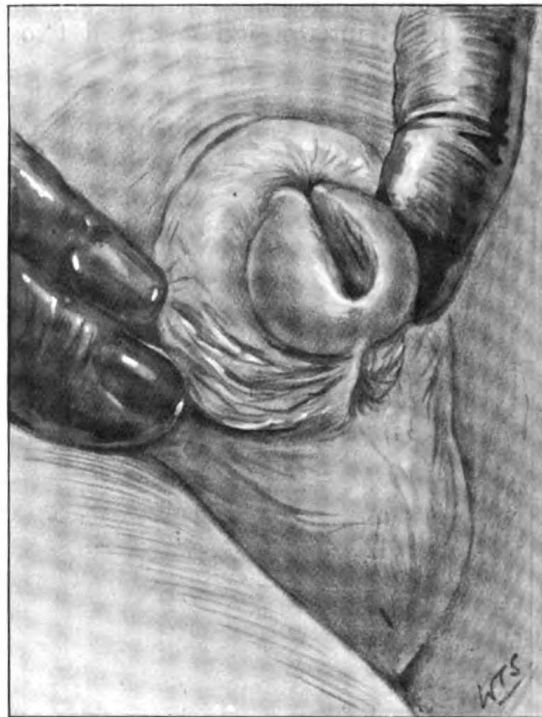


FIG. 2.—Case II.

divided when he was 11 months old. Four years later he began to suffer from diurnal incontinence. This steadily got worse, and when I saw him the urine was constantly dribbling away. On examination, the penis was found to be small and stunted, the prepuce long and voluminous. A vertical oval scar, about $1\frac{1}{2}$ by $\frac{1}{2}$ cm. was seen immediately in front of the pubis, and another of almost equal size was visible on the dorsal aspect of the prepuce. When the prepuce was fully retracted, a rather stenosed urethral opening was seen in the middle line, immediately behind the corona. From this a very deep groove extended forwards. It involved almost the whole thickness of the glans penis (fig. 2). Its greatest depth was 1 cm. The whole groove was lined with pink mucous membrane, which formed

a distinct ridge on each lateral wall. These were well marked in the posterior half of the groove, but when traced forwards, gradually diminished in size, and disappeared entirely about 3 or 4 mm. from the tip of the penis. This patient had a very tight stricture of the penile urethra, about 1 cm. behind the abnormal orifice, which was fully dilated under general anæsthesia. The penile portion of the urethra was dorsal to the corpora cavernosa, which were united to each other. The deep urethra was normal in position. The testicles were only partially descended, and lay a short distance below the corresponding external rings. There was a small hernia on the right side. The pelvic girdle was normal. The incontinence was due to the stricture, and disappeared as soon as it was dilated.

Multiple Cystic Formation in Lower Pole of Kidney.

By R. H. JOCELYN SWAN, O.B.E., M.S.

W. R. C., MALE, aged 36, seen in 1916, gave a history of an attack of acute pain in loin four years previously, but could not remember on which side it occurred. Patient awoke one night three weeks before with urgent desire to micturate and found urine deeply stained with blood. Had to get up frequently during night and urine remained blood-stained for two days. Similar attacks have occurred on two subsequent occasions, each lasting two to three days. During the attacks of hæmorrhage there is frequent micturition and the urine varies considerably in colour, sometimes being clear on one occasion and blood-stained the next. No pain on micturition and no pain nor aching in loins.

On examination: Strong, healthy looking man. No pain of any sort. Urine deeply stained with red blood, acid, specific gravity 1016. No casts and no pus found. Left kidney distinctly palpable and enlarged. Not tender on pressure. Right kidney not felt. Testes normal. Per rectum: Prostate and vesicles normal; ureters not felt. Cystoscopy in 12 oz. distension. Bladder normal. Blood-stained urine in feeble but frequent efflux from left orifice, which was normal in appearance. Clear urine from right orifice. Catheter passed into right ureter and urine collected found to contain 2'2 per cent. urea. Skiagraph: No shadow of calculus.

Operation: Left kidney exposed and lower pole found to be occupied by numerous cystic cavities containing blood-stained fluid under tension. Area was firm and with suspicion of growth; kidney was removed.

Patient has been seen recently and is in perfect health. Urine is clear; free from albumin. Specific gravity 1020.

Pyonephrosis due to the Kinking of the Ureter by Aberrant Renal Vessels.

By R. H. JOCELYN SWAN, O.B.E., M.S.

A. D., FEMALE, aged 47. Had had attacks of dull, aching pain in left loin for twenty years, with occasional severe attacks of acute pain accompanied by vomiting and collapse. Pain localized in loin with increased desire to micturate during severe attacks. During the last three months the pain had been much more severe and more frequent. Patient states that after an attack of acute

pain the urine is thick and deposits a white sediment. Hæmaturia has not been seen.

On examination: A rounded tumour was easily palpable in the left loin, presenting the features of a renal swelling. Right kidney not palpable. Urine acid, specific gravity 1012, albumin present. Deposit contained pus and a few blood cells. Sugar absent. Cystoscopy: Bladder normal. Ureteric orifices normal, efflux from left orifice seen to be purulent. Chromo-cystoscopy: Blue coloration of the efflux from right orifice five minutes after an intravenous injection of indigo-carmin. No coloration from left orifice after fifteen minutes. Blood urea: 23 mg. in 100 c.c. of blood. Urea concentration: 1·8 per cent. in 2½ oz. in first hour, 3·4 per cent. in 1½ oz. in second hour; 2·8 per cent. in 1½ oz. in third hour.

Operation: Kidney found with a very dilated pelvis together with renal dilatation from the acute flexion of the ureter at the junction with the pelvis over a tense band containing large vessels running to the lower pole of the kidney. The main renal vessels entered the kidney at the upper part. Nephrectomy was followed by uninterrupted recovery.

Prostate removed by Prostatectomy; Weight, 12 oz., or 340 gm.

By R. H. JOCELYN SWAN, O.B.E., M.S.

E. C., AGED 81. Had had trouble with fréquent micturition for many years and seven years ago was advised to pass a catheter night and morning. For the last two years he has been entirely dependent upon the catheter, but has found difficulty in passing it, bleeding and pain often being caused. Catheter passed about six times a day. No backache, no increased thirst, and takes food well.

On examination: Good colour; does not look his age. Pulse 70, arteries thickened; blood-pressure 160 mm. Tongue slightly coated, moist. Urine: neutral, specific gravity 1010, small deposit of pus, without odour; albumin present. Bladder not distended. Neither kidney felt. Catheter had been passed half an hour before patient was seen. Per rectum: Very large soft, elastic prostate felt. Was under observation for several days during which the daily amount of urine averaged 70 oz. with daily excretion of 370 grains (average). Urea concentration: Before test, urea 1·7 per cent.; after one hour, urea 2·1 per cent. in 3 oz.; after two hours, urea 2·2 per cent. in 6 oz.

Operation: Suprapubic prostatectomy, September 18, 1922. Very large intravesical projection and considerable difficulty in enucleation owing to the size of gland. Each half removed separately. Condition good at end of operation.

September 19: Was cyanosed; pulse 120 and respirations 40; only 11 oz. drained in twenty-four hours.

September 20: Rapid collapse, and died at 8 a.m. Only 2 oz. drained in previous twelve hours.

**Specimen showing Interior of the Bladder Six Months after
Extensive Resection for Carcinoma, with Transplantation
of the Right Ureter.**

Shown by JOHN EVERIDGE, O.B.E., F.R.C.S.

FROM a patient, aged 43, from whom in April, 1922, I resected practically the whole of the right half of the bladder for carcinoma. The right ureter lay in the centre of the growth and was resected and implanted into the wound. At the completion of the operation the lumen of the reconstructed bladder was cylindrical—about the size of the middle finger—being built up around a medium sized drainage tube. The suprapubic wound healed in three weeks, and two months later urine could be held for four or five hours.

In November, 1922, hearing that a friend had died of cancer, he shot out his brains, from the fear that he might be afflicted by the same disease.

The chief interest in the specimen lies in the healthy and uncontracted new orifice of the implanted ureter, as well as in the striking absence of scar tissue.

Specimen of Diverticulum of the Bladder.

Shown by JOHN EVERIDGE, O.B.E., F.R.C.S.

THIS specimen shows a sac as large as the bladder, and communicating by a narrow stoma with the main cavity. It springs from the right lateral wall of the bladder above and behind the right ureteric orifice. Both ureters open into the main bladder, and neither presents any obvious dilatation. The kidneys also appear normal to the naked eye, no hydronephrotic changes having occurred. Microscopically the kidneys show marked engorgement of the vessels, and early fibrosis around the glomeruli. The wall of the bladder is distinctly trabeculated, but there are no other diverticula. The wall of the sac presents muscle fibres.

Clinical History.—From a male, aged 50, admitted to a medical ward on account of uræmia. The patient gave a history of stricture, but no hindrance was found to the passage of a catheter, and no tangible source of urinary obstruction could be found. No lesion of the central nervous system was discovered. Death from uræmia took place four days after admission to hospital.

Case of Ectopia Testis.

By A. CLIFFORD MORSON, O.B.E., F.R.C.S.

AMONGST numerous examples of misplaced organs which appear from time to time in medical journals, there are, perhaps, none more interesting than those relating to the sexual apparatus. The development of those structures which determine sex is dependent upon factors of which little is known, and therefore the *raison d'être* of their appearance in positions sufficiently unusual as to cause their owners considerable inconvenience is a matter of conjecture.

A youth, whose abnormality had remained unknown to him for seventeen years, presented himself in August, 1920, at my Out-Patient Department of the Hampstead General Hospital with the complaint of aching in the right groin when bicycling or running fast. On examination I noted that the lad was well built, of good muscular development, and, for his age, had a plentiful growth of pubic hair. The penis and left testicle were normal. The right side of the scrotum was undeveloped, and the right testicle could be seen as a definite swelling on the inner side of the thigh about 3 in. from the anal margin (*see figure*). Pressure upon the gland produced the characteristic sensation associated with the normal testicle, and its size was approximately the same as the left. There did not appear to be any other anatomical peculiarities.



Having obtained consent to operate, I decided to replace the right testicle in the scrotum. An incision was made over the right testicle, and the spermatic cord isolated as it emerged from under the free edge of the external oblique aponeurosis. To pull the testicle out from beneath the skin was a simple matter, there being no process of fibrous tissue anchoring it down. A passage was next made into the right side of the scrotum with sinus forceps, considerable stretching of the tissue being necessary in order to fashion a tube large enough to admit the testicle into the scrotum. The misplaced organ when exposed was anatomically perfect. When the operation had been com-

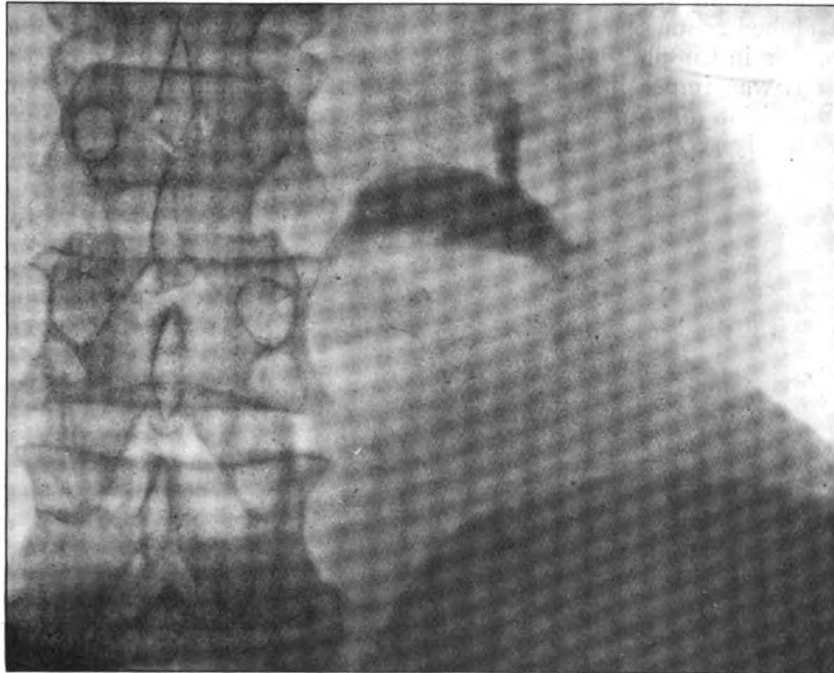
pleted the skin of the right side of the scrotum was tense, and entirely free from rugæ. Within seven days this half of the scrotum had acquired the normal rugose appearance, and showed no evidence of lack of development. The patient was discharged from hospital ten days after operation with no outward manifestation of disability apart from the scar in the thigh.

Two and a half years have now elapsed since this operation, and the position of the right testicle in the scrotum remains normal. However, the development of this organ seems to have been arrested, for while the left testicle is of adult size, the right is no larger than it was in August, 1920. Sensation to pressure is also dulled, and I am of opinion that ultimately complete atrophy will take place.

Serous Cyst of the Kidney.

By KENNETH M. WALKER, F.R.C.S.

THE patient was a woman, aged 65, admitted to the Royal Northern Hospital, under my colleague, Dr. Malcolm Donaldson, for a large cystic swelling the size of a coco-nut on the left side of the abdomen. The diagnosis lay between an ovarian cyst and a cystic kidney, and I was asked to examine



her from the point of view of differential diagnosis. There were no symptoms of any sort, and the patient suffered no disability from the presence of the cyst. The vaginal examination showed that the cyst was entirely independent of the uterus and its adnexæ. In palpating the abdomen I thought I could feel the left kidney lying above the level of the cyst, and was inclined to believe that the cystic swelling was not renal. However, to clear up matters

a pyelogram was made. Cystoscopy showed a normal bladder, with normal effluxes from both orifices. On the left side the ureteric catheter became fixed some 12 cm. up the ureter. The pyelogram was of great interest (*see figure, p. 45*). It showed that the left kidney was low in position, its hilum being at the level of the lower border of the second lumbar vertebra. The pelvis was distinctly distorted, forming a semilunar shaped cavity stretched over the upper border of the large cyst, the edge of which could be plainly seen in the skiagram. The result of this was to show that the cystic swelling was undoubtedly connected with the left kidney. On lumbar exploration, Dr. Donaldson found a large serous cyst bulging from the lower pole of the kidney, and a nephrectomy was performed. Microscopic sections of the kidney showed some chronic interstitial nephritis with areas of round cell infiltration.

The case owes its interest to the fact that single serous cysts of the kidney are extremely rare, and that as far as I know a pyelogram of such a condition has never before been made. Guinsbourg in his "*Contribution à l'étude des grand kystes du rein*" (Dissert., Paris, 1903), was only able to collect up to the time of his report thirty-nine cases of single serous cyst. Their ætiology is still obscure, some authors regarding them as retention cysts and others as comparable to polycystic kidneys, but neither explanation is entirely satisfactory. In some cases the kidney itself is quite healthy, and conservative operations have been performed, the first one by Tuffier in 1897. The interesting features of the pyelogram are the manner in which the pelvis has been stretched round the cyst and the clearness with which the outline of the cyst appears in the photograph. Owing to the fact that the cyst burst during removal it was impossible to collect any fluid for examination, but whatever the composition of that fluid may have been, it was such as to cast an extraordinarily good shadow.

Section of Urology.

President—Sir JOHN THOMSON-WALKER, F.R.C.S.

The Propriety of attempting to secure Primary Union after Operations upon the Bladder and Prostate.

By A. RALPH THOMPSON, Ch.M., F.R.C.S.

THE object of this paper is to raise a discussion upon the advisability of suturing the bladder in certain cases of suprapubic cystotomy.

Without doubt, in the present state of our knowledge, no effort should be made to close the bladder after operations performed for certain conditions, particularly in those cases in which it is desirable to establish free drainage, either for bladder or kidney conditions.

I have on record from my wards at Guy's Hospital or from private practice thirty-one cases in which the bladder and the abdominal wall were deliberately sutured after suprapubic cystotomy. In a few cases a gauze drain was inserted superficially; and in all such cases the gauze drain was not retained for more than two days, and in some cases only for twenty-four hours, but in the larger number of cases no superficial drainage was provided.

These cases of attempted primary union after operations upon the bladder or prostate by the suprapubic route may be arranged under seven heads, according to the nature of the condition which gave rise to the operation. These heads are as follows: (I) Adenoma of prostate; (II) malignant disease of prostate; (III) adenoma of prostate accompanied by the presence of vesical calculi; (IV) growth of the bladder; (V) vesical calculi; (VI) retrograde catheterization; (VII) exploration of bladder (*see* Table, p. 48).

(I) ADENOMA OF THE PROSTATE.—FOURTEEN CASES.

Suprapubic prostatectomy was performed in all these cases. The average age of the patients was 64, varying downwards from 85 to 54. The average stay in the hospital after operation was twenty-nine days.

[I have chosen ten cases of suprapubic cystotomy from my records in which the bladder was not sewn up, but allowed to heal of itself. I find the average stay after operation was in these cases forty-five days.]

Of these cases, fourteen in number, one male, aged 63, died of uræmia after operation. I ought not to have closed the bladder, and in fact I cut the stitches within six hours of the operation and placed a tube in the bladder. The patient, however, died on the fourth day after operation. Three cases had a leakage of urine through the wound for a very short time; two of these recovered perfectly, and in one the wound broke down slightly, necessitating readmission to the hospital for ten days, during which time the patient got quite well and is now in very good condition. When the term "leakage" is used it is meant only that the dressings were very slightly damp.

48 Thompson: *Operations upon the Bladder and Prostate*

TABLE SHOWING CASES OF PRIMARY UNION AFTER SUPRAPUBIC CYSTOTOMY.

(I) <i>Simple Adenoma of Prostate.</i>			
Leakage	No leakage	Complications	Death
(1) Adlam ... 58	(8) Bignell ... 64		
(2) Coles ... 65	(9) Butcher ... 65		(28) Collins ... 63
	(10) Cox ... 60		
	(11) Hill ... 70		
	(12) Howell ... 56		
	(13) Johns ... 54		
	(14) Lane ... 66		
	(15) Metcalf ... 85		
	(16) Nightingale 54	Bacilluria	
(3) Wellings ... 71	(17) Rogers ... 64		
Average age, 64; average stay in hospital after operation, twenty-nine days.			
(II) <i>Malignant Disease of Prostate.</i>			
(18) Lambert... 69			
III) <i>Simple Adenoma of Prostate + Calculi.</i>			
(19) Cooper ... 65			
(20) Jourd ... 60			
(29) Sieveyer ... 68			
(IV) <i>Growths of the Bladder.</i>			
(4) Duncan ... 43 (female)	(21) Collins ... 48 (female)	Slight recurrence	(30) Burton ... 38 (female)
(5) Phillips ... 19	(22) Jones ... 44 (female)		
(6) Roper ... 63	(23) Monser ... 69		
	(24) Patmore ... 37		
	(25) Ward ... 29		(31) Ritchie ... 56
Average age, 44½; average stay in hospital after operation, twenty-five days.			
(V) <i>Vesical Calculi.</i>			
(26) Mylam ... 50			
(VI) <i>Retrograde Catheterization.</i>			
(27) Elbourne 30			
(VII) <i>Exploration.</i>			
(7) Collins			
Average stay in hospital of ten non-selected cases (apart from fistula), wound left open, 45½ days.			

(II) MALIGNANT DISEASE OF PROSTATE.

The only case I have to offer of this condition is an important one.

B. L., aged 69, suprapubic cystotomy performed on June 24, 1919. The prostate was found at the operation to be malignant. This opinion was confirmed by Dr. G. W. Nicholson, who found it to be carcinomatous. As I was fortunate enough to get the growth out cleanly and as no obvious disease was left behind—I decided to close the bladder. A little leakage occurred, but only a very little, and the patient was discharged with the wound healed soundly on July 12, 1919, or eighteen days after the operation. He died in June, 1922, three years after operation, with secondary deposits. Between operation and death he never had any local recurrence and lived a happy life free from pain and urinary trouble.

(III) ADENOMA OF PROSTATE COMPLICATED BY THE PRESENCE OF VESICAL CALCULI.—THREE CASES.

These cases, three in number, are recorded under a separate heading, as I take it the presence of vesical calculi is indicative of damage to the bladder mucous membrane.

Of these three cases two, aged 65 and 60 years respectively, did perfectly well and lived some years after the operation with no recurrence of local symptoms or disease of the urinary tract. One patient, aged 69, however, died shortly after the operation from hæmorrhage and shock, chiefly associated with the prostatectomy. Doubtless the attempt at primary union was wrong, and its occurrence prevented the officers on duty at the hospital from doing what they might otherwise have done in the way of controlling the hæmorrhage. The main calculus removed from this patient was a very "spiky" one.

(IV) GROWTH OF THE BLADDER.

I do not include under this heading any case of such a malignant condition as a true carcinoma or sarcoma, but I do include not only simple papillomata but also those papillomata, which, clinically, tend to spread or multiply, and, pathologically, are found to be infiltrating.

I have records of ten such cases, the average age of whom was 44½, varying from 69 downwards to 19—four females are included under this heading. The average stay in hospital after operation was twenty-two and a half days, just half the average time. Five cases healed perfectly; four of the patients are now well, and one patient has a slight papillomatous fringe at the neck of the urethra; and this condition is manifestly improving with diathermy. In three cases there was a trifling amount of leaking for one or two days, but healing was otherwise perfect and the patients have done well since the operation. In two cases the patients died. Of these two cases, one of the deaths, that of a man aged 56, took place one week after operation from bronchitis following a cold spell of weather in February, 1916. *Post mortem*: bladder and wound perfectly healthy. The other death was, I have no doubt, due to the suturing of the bladder. The patient was a woman, aged 38, who was under the care of my colleague, Mr. R. Davies Colley, in Guy's Hospital. I am indebted to him for permission to use the notes of the case.

She had passed a stone about the size of a nut some two years before admission. She continued to pass stones at frequent intervals, and in all passed about nine stones, which were white and brittle. She had had bladder irrigation. She was admitted with hæmaturia on November 25, 1921. I examined her cystoscopically, and found, as I thought, a papilloma in the region of the left uretreic orifice of the bladder. I operated upon the bladder on December 16, 1921, and removed the growth—the bladder came easily into the wound, so I excised the mucous membrane round the growth. Two or three sutures were put through the mucous membrane and the edges brought together. I sutured the bladder and abdominal wall. She appeared to do well, but on December 30 she suddenly became worse, and sank, and died on January 1, 1922.

Post mortem.—Pyelitis in the right kidney. A small hole in the base of the bladder—cellulitis round the base. Pus tracking up to the left kidney, which was infected with septic changes.

(V) VESICAL CALCULI.

I have only one case to record of this condition:

Patient, aged 49, had had his right kidney removed for tubercle by Mr. C. H. Fagge, in 1918, who, in addition to his own, was kindly doing my work at Guy's Hospital. This operation was quite successful. Patient in 1919 complained of pain in the bladder. The cystoscope revealed the presence of multiple calculi. I performed suprapubic cystotomy on March 6, 1919, and removed the calculi. I sewed up the wound as the patient was a nervous "jumpy" man, and I feared that, if the wound were left open, any tuberculous focus in the bladder, of which there was no naked-eye evidence, might

50 Thompson: *Operations upon the Bladder and Prostate*

become associated with sepsis. The wound healed well without leaking and patient was discharged on March 30, 1919, twenty-four days after the operation. He did remarkably well, but now has pyuria and hæmaturia with no X-ray evidence of vesical calculus, and, I fear, tubercle of the remaining kidney.

(VI) RETROGRADE CATHETERIZATION.

I have one case to record of this operation.

The patient, aged 80, had an impassable stricture of the urethra. I failed with internal urethrotomy and at once opened the bladder. I performed retrograde catheterization, and tied in a catheter and sutured the bladder and abdominal wall. Patient's recovery was complete, and he was discharged twelve days after operation and has done well since the operation.

(VII) EXPLORATION OF THE BLADDER.

Patient in this one case was a male, aged 65, in whom I suspected enlarged prostate. I cut down and found nothing to warrant removal. The wound was sewn up and a catheter inserted. I cystoscoped him on the fourteenth day and found the edges of mucosa in the bladder separated from each other by a yellow coloured slough. The wound, which had been median, longitudinal and straight in the distended bladder was, in the bladder when holding 6 oz. of fluid, curved forwards, downwards and to the right. The patient was discharged apparently well on the sixteenth day after operation. In one week's time, he returned under my care with a small fistula in the suprapubic region which healed up in ten days, with suitable catheterization. I traced him for a few months and he did well. I think his medical attendant would have reported to me, if anything untoward had happened.

The cases of attempted primary union, after suprapubic cystotomy has been performed for various reasons, have now been dealt with in detail. The deductions to be drawn from these records may now be discussed. The method of operating and the treatment after operation will be considered later.

In the cases previously cited it may be stated at once that the stay in my beds at Guy's Hospital of primary union cases is much shorter after operation than that of cases left to heal up by themselves. I have suggested forty-five days as the average stay of cases of open operation. Primary suture after prostatectomy reduces the stay to twenty-nine days. Primary suture after removal of a growth reduces the average stay to twenty-two and a half days, or exactly half the open operation average. As in these cases of primary union the amount of dressing used is very small compared with that of the open wounds, the advantages accruing to the finances of a hospital are sufficiently obvious.

There is not a single case on record in my notes of a fistula persisting more than ten days, and that only in two cases.

There is only one case of bronchial complications. That indeed was a fatal case, but I have given reason for supposing that the bronchitis was not due to the primary suture. I have frequently heard patients complain that either they are afraid or they are unable to cough with the open wound. With the closed wound I have not had any complaints of this difficulty. The ability to cough up phlegm is of inestimable service to an old man.

The wound has never really caused me any anxiety as regards local sepsis. There has never been undue tension upon the suture or any cutaneous septic lesion. In fact, there can be very little doubt that the patients have benefited greatly by the primary union operation.

There is one case of slight recurrence of a growth of the bladder at its neck, but surely this is much better than a spread of the growth to the apex of the

bladder and into the surrounding abdominal wall: I have seen this five times in cases admitted into Guy's Hospital, having been operated upon previously in other hospitals. Surely the wound in such cases, if complete removal can be reasonably ensured, should be sewn up.

I have given an account of the advantages of primary union after bladder operations. The rules for not attempting primary union are, in my opinion, the following:—

(1) Any wound of the base of the bladder, apart from that involved in prostatectomy, may lead, even when sutured, to a cellulitis of the base of the bladder and I shall be very cautious in future about sewing up a bladder when the base has been involved in the operation, apart from the prostatic region.

(2) I should not advise suture of the bladder in cases of enlarged prostate associated with the presence of a rough or sharp pointed calculus. I have four specimens of these "spiky" calculi coming under Class III and, I take it, they are, therefore, not uncommonly associated with enlarged prostate.

(3) If I did not think that the patient's pulse was good enough to warrant my saying that there would be no reactionary hæmorrhage within twelve hours of the operation, I would not recommend suture.

(4) If I did not think that the bladder was left in a nice post-operative state—e.g., absence of hæmorrhage, with a smooth, clean and contracting cavity after prostatectomy, I would not recommend suture of the bladder. With advanced cystitis and uræmic symptoms, it need hardly be said I should not recommend suture of the bladder.

As regards malignant disease, with the reservation that my first rule must be kept in mind, I think the case I have related of B. L., aged 69 (p. 48), shows that after removal of a malignant prostate by means of suprapubic cystotomy, the question of primary union should be very seriously considered.

SUPRAPUBIC OPERATION.

The steps of the suprapubic operation may now be considered very briefly. The patient is prepared in the usual way, and an incision is made through the abdominal wall in the usual suprapubic position. The cave of Retzius is exposed and the peritoneum retracted from the front and sides of the bladder. The edges of the wound are very carefully packed round with gauze in the undermined part lying superficial to the bladder. This packing is done as carefully as possible. A transverse incision is made in the bladder wall and the fluid evacuated. If it is thought that the intravesical condition demands complete exposure of the bladder cavity the sides and upper and back part of the bladder are gently dissected free with a finger or a blunt instrument, or with an occasional touch of the knife, but care is taken not to tear the tissues. The space then exposed is thoroughly packed with gauze.

After the bladder incision is made, if it be a large prostate which is to be operated upon, this is removed in the usual way, and hæmorrhage stopped by massage of the cavity and the application of hot liquor hamamelidis at 115° F. It is surprising how far the prostatic region may be brought up into the wound.

During the actual enucleation of the prostate the patient is under deep chloroform anæsthesia. I have been greatly assisted in this respect by Mr. Eric Scott and Mr. A. D. Marston, and we find that under these circumstances there is much less shock to the patient during enucleation than when

other methods of anæsthetization are adopted. After all hæmorrhage has ceased, care being taken that the pulse is of sufficient volume to indicate a reasonable blood-pressure, the wound in the bladder is sutured with catgut sutures, five or six in number, applied Lembert-wise to the wall of the bladder, care being taken to avoid the mucous membrane. The sutures are tied off and cut close; if necessary, reinforcing Lembert sutures are applied.

Great care is taken to ensure that there is no deficiency in the union of the bladder wall. The bladder is dropped back into place, the gauze packing is removed, and it is surprising how the peritoneum follows it and completely covers the site of incision in the bladder-wall. Catgut sutures are then applied to the rectal sheath, which lies in front of the rectus muscle. The suturing of the rectus muscle itself is avoided as far as possible. Salmon-gut sutures are used for sewing up the skin. A catheter is passed and retained for twenty-four hours. My former house-surgeon, Mr. D. J. P. O'Meara, who took a great deal of trouble over my cases, found that patients, both male and female, pass their urine more frequently than might be expected when it is removed.

One or two questions occur to me with regard to particular points in the steps of the operation, the main details of which have just been given.

First, as regards distension of the bladder preliminary to operation. It is very important in order to ensure primary union in suprapubic wounds to avoid shock, which, occurring just after the operation, occurs at that particular time when the wound is most susceptible to infection. At one time I used to distend the bladder with air, preliminary to operation, in order to avoid contamination of the tissues around the bladder by fluid escaping from it after incision. But my house-surgeons did not like this plan, and certainly there seems, for some reason, to be more shock caused to the patient by distension with air than with warm fluid. I have ceased to inject air, and seek to avoid contamination of the perivesical tissue, when the fluid is evacuated, by careful packing with gauze.

It would be interesting to know if others have had similar experience with air distension of the bladder.

Secondly, the question arises as regards the direction of the incision in the bladder.

My reasons for adopting the transverse incision in suprapubic cystotomy are as follows:—

(1) There is less likelihood of the peritoneum being damaged by a transverse incision than by a longitudinal incision, especially if it becomes necessary to prolong the incision. Even with a bladder which is very much distended with fluid I believe that the peritoneum is constantly to be seen and avoided.

(2) In my experience, there is less bleeding from the bladder-wall after a transverse incision than after a longitudinal incision—the large perivesical veins being cut right across rather than notched, as they may be with a longitudinal incision.

(3) Complete and satisfactory suture of the bladder wound is more easily secured after a transverse than after a longitudinal incision. I find it a difficult and rather lengthy proceeding to suture the wall of the empty bladder in the lower part of a longitudinal wound—much more difficult than the suture of the ends of a transverse incision.

There are special points to be considered in connexion with an operation

for intravesical growths which do not, I think, occur in connexion with the removal of an enlargement of the prostate gland.

Here I shall touch upon very debatable questions which I shall, nevertheless, venture to put forward. These questions centre round the use of a retractor in bladder surgery. I find that, in females practically always, and without much perivesical dissection, and, in males less frequently, and with more perivesical dissection, it is possible to bring the base of the bladder into the wound and remove growths which are situated in the base of the bladder. I avoid the use of a retractor as far as possible. First, because I feel that its unwise and careless use may damage the bladder-wall in the region of the incision. Secondly, because a retractor commits one to a deep and possibly therefore a lengthy operation. I like the growth to come to me rather than that I should go to meet the growth in the depths of a bladder rendered deeper by the retractor.

My plan is this, that instead of using a retractor, I place curved Lane's or Moynihan's forceps upon the edges of the wound. These are applied round the wound as symmetrically as possible, six to eight in number, and traction is made upon them as a whole, and not upon one or two. If traction is thus applied to the edges of the wound it is, at any rate to me, surprising that there is no tearing of the edges of the wound. Care, however, must be taken that undue shock is not caused by this traction upon the bladder.

I consider, also, that in those cases, in which a surgeon, especially at a teaching hospital, can bring a growth up into the wound, he is much more likely to remove the growth entire, and for the better instruction of students, than if he is operating at the bottom of a deep and obscure cavity.

An advantage of primary union after an operation for vesical growth is that there is, I think, less tendency for it to spread either locally or into the bladder-wall along the urachus and post-part of the rectal sheath. If it does recur it remains in the bladder and is separated from the abdominal wall by healthy tissue.

Summary of the reasons for suprapubic cystotomy wounds being sewn up may now be given.

REASONS FOR PRIMARY UNION.

- (1) The comfort of the patient.
- (2) The decreased trouble of after-treatment of the case, and diminished amount of dressings.
- (3) The shorter convalescence.
- (4) The greater strength of the wound resulting from the operation.
- (5) The better restoration of the normal anatomy of the parts after operation; including particularly that of the peritoneum, and the better post-operative functioning power of the bladder muscle.
- (6) The prevention of late complications, which may be associated with an open suprapubic wound.
- (7) Last, but not least, the greater care which would be exercised in a suprapubic operation by an operator who proposed to suture the bladder after the operation.

Case of Air Embolism occurring during Urethroscopy.

By R. OGIER WARD, F.R.C.S.

PATIENT, a man, aged 68, who had suffered from gonorrhœa forty years previously, and had passed urine with some difficulty and in a narrowed stream for twenty years. An unsuccessful attempt was made to pass an instrument through a stricture in the bulb of his urethra. A week later, aëro-urethroscopy was carried out, a tightly fitting urethroscope tube was employed, and during the introduction of this some hæmorrhage occurred from the urethral mucous membrane. After swabbing this out the examination was continued. The urethra did not easily dilate and, in order to see the stricture, more air pressure was employed than in ordinary cases. The patient became dyspnœic, cyanosed, convulsed, and then died. After unsuccessful artificial respiration for about ten minutes, the abdomen was opened, the diaphragm incised and the pericardium entered. Cardiac massage was employed for another ten minutes without avail. Air could be felt in the right ventricle: finally the wall of this was incised and frothy blood escaped.

On post-mortem examination a stricture was found in the bulb of the urethra with small lacerations of the mucosa anterior to it. One kidney was hydronephrotic, the other showed interstitial nephritis. All the cavities of the heart contained frothy blood. There was no patent foramen ovale. Bubbles of gas were also found in the aorta, common iliac arteries and the circle of Willis; also in the pulmonary veins, inferior vena cava, common iliac, spermatic and cerebral veins. No froth was found in the pulmonary artery, probably owing to the fact that the right ventricle had been incised and partly drained. No air was found in the bladder.

The case is noteworthy because such occurrences are rare, though minor degrees of embolism during urethroscopy are perhaps not very uncommon. The distribution of the gas throughout the heart and throughout the circulation is interesting. This experience emphasizes strongly the well known danger of employing aëro-urethroscopy when there is injury of the urethral mucous membrane. The danger is certainly greater if a tight stricture prevents the easy escape of gas into the bladder, especially if the urethroscope tube fits closely. Finally, it may be suggested that oxygen may be usefully employed, since there will be a far greater margin of safety should embolism occur.

Section of Urology.

President—Sir JOHN THOMSON-WALKER, F.R.C.S.

The Operative Treatment of Vesical Diverticula.

By J. SWIFT JOLY, F.R.C.S.

THE treatment of large vesical diverticula is one of the most difficult problems with which the urinary surgeon has to grapple. Complete excision of the sac is, at present, the accepted method of dealing with it, but as so many widely divergent views have been expressed on this subject, it may be of interest to give a brief description of fourteen cases upon which I have operated, and the conclusions I have drawn as to the relative values of the different methods employed.

Case I.—Male, aged 51, admitted to St. Peter's Hospital January 11, 1910.

History: He always had difficulty in micturition, and took much longer to complete the act than other people. Was catheterized in 1901. His bladder was then infected. Since then he has suffered from frequent attacks of pain in the left groin, accompanied by headache, backache, night sweats, marked frequency of micturition, and occasionally by hæmaturia. Frequency of micturition from one to two hours both by day and by night. The urinary stream was slow and weak, but he never had retention of urine. Pain during micturition felt along the urethra.

On examination: No stricture. Residual urine 13 oz. Prostate normal; above it the bladder bulged into the rectum, but was not indurated. The bladder could be felt, when it was apparently empty, rising just above the pubis, under the right rectus; when it was distended the displacement to the right was more marked.

Urine: Specific gravity 1018, acid, albumin present, no sugar, urea 1 per cent., deposit of pus and blood.

Cystoscopy: It was only after a long course of daily irrigation that it was possible to wash the bladder clear enough to obtain a satisfactory view. Distention 24 oz. The right ureter was displaced to the right, and the left almost to the middle line. The opening of a large diverticulum was seen above and to the left of the left ureter. It was round, and appeared to be about the size of a lead pencil. Surrounding this orifice, except at its lower part, was a polypoid mass of œdematous mucous membrane. The left ureter was dilated.

Operation, January 19, 1910: Trendelenburg position. Median incision from pubis to umbilicus, and a transverse incision across the left rectus $1\frac{1}{2}$ in. above the pubis. The operation was entirely extra-peritoneal. The diverticulum was about half as large again as the bladder. It completely filled the left half of the pelvis, and extended a short distance to the right of the middle line, pushing the bladder before it. The diverticulum was separated from the surrounding tissues until the spine of the ischium was reached. A large vein was torn here, and as it could not be picked up and tied, this part of the wound was plugged with gauze. The bladder was then opened, and the orifice of the diverticulum identified. It just admitted the first finger, and was about

an inch above and to the left of the left ureter. With one finger in the opening, the wall of the diverticulum was divided close to the bladder. This incision was carried round the orifice, and the diverticulum was separated from the bladder. This brought the left ureter into view. It was about as thick as the finger, and curved forwards, immediately below the diverticular orifice to its entrance into the bladder. In this portion of its course it was compressed between the diverticulum and the bladder. The diverticulum was now rapidly freed from the remaining adhesions to the rectum and prostate, and removed. The bleeding points were picked up and ligatured, the wound partially closed, and the bladder and pelvis drained.

The convalescence was slow, and a suprapubic fistula, which tracked down to the diverticular opening, persisted for almost three months. It then closed, but after a short time broke down again. On June 8, he was again admitted to hospital, and the sinus excised, at the same time the prostate was enucleated, though it did not appear to be enlarged, as it was thought to have been the cause of the persistent sinus. After this operation the fistula remained closed. In 1918 he was again admitted to hospital, and a suprapubic hernia was cured by the fligree method.

This patient eventually died in December, 1920, almost eleven years after his operation, of chronic pyelonephritis, which persisted in spite of treatment. During this interval he had practically no frequency or difficulty in micturition, but there was always about three ounces of residual urine.

The diverticulum, after it was hardened, was about the size of a large orange. Its walls were $\frac{1}{4}$ in. thick, and were composed of three distinct layers of muscular tissue, lined with transitional epithelium, which was much altered by inflammation.

Case II.—Male, aged 58, admitted to St. Peter's Hospital March 19, 1912.

History: Gonorrhœa thirty years before. Developed a stricture, which was easily dilated. Passed a bougie on himself twice a year, but the second time he did so he infected his bladder. Since then had several attacks of cystitis, and his urine always remained turbid.

On admission: Slight frequency of micturition. Stream good; some straining towards the end. Occasionally micturition *à deux temps*. Stricture admitted 17 F easily, and was fully dilated in two sittings. Prostate normal, but a soft elastic swelling could be felt above it. Residual urine varied from 12 to 16 oz.

Urine: Specific gravity 1018, acid, no albumin nor sugar, urea 1.9 per cent., deposit pus and bladder epithelium.

Cystoscopy: Slight general cystitis. Both ureters normal, clear efflux from both sides. Above the interureteric bar and immediately to the left of the middle line was the opening of a large diverticulum. It was oval in shape, with the long axis directed vertically. The cystoscope was introduced into the diverticulum. Its walls were a deep red colour, smooth, and no vessels were visible.

Operation, March 22, 1912: Trendelenburg position. Bladder washed out, emptied as much as possible, and distended with air. Median incision from pubis to umbilicus. peritoneum opened. The diverticulum appeared like an immense bleb on the posterior wall of the bladder, to which it was adherent over a circular area about 3 in. in diameter. The bladder was then opened, and it was found that the diverticulum was completely filled with a mixture of urine and lotion. This was mopped up. As it was impossible to find any line of cleavage between the bladder and diverticulum, the former was split from front to back. The posterior part of this incision surrounded the orifice of the diverticulum, which was excised. The whole of the epithelial lining of the diverticulum was then removed, also the greater part of its outer wall, only two peritoneum-covered flaps large enough to cover the raw surface being left. These were stitched in place, and the bladder wound sutured except for a small space left for a drainage tube. The peritoneal cavity was then closed, and the abdominal walls sutured.

The ureters first passed downwards between the bladder and the diverticulum, then backwards beneath it, and finally upwards along its posterior surface. They formed ridges visible on the inner surface of the sac, and divided its lower portion into three pouches, a median and two lateral. The deepest portion of the diverticulum reached

the level of the middle of the prostate. Its walls were very thin, and were composed of a single sheet of muscular tissue lined by epithelium.

The patient bore the operation well, and left hospital in four weeks, with his wound firmly healed. He returned in three months with a urinary fistula in the scar. He was admitted and the fistula was excised. Since then he has had no further trouble.

He was last seen about two years ago. There was no frequency of micturition. The urethra admitted a No. 26 F instrument easily.

Cystoscopy: Slight general cystitis. A puckered scar occupied the site of the old diverticular opening; from this a linear ridge ran forwards to the apex of the bladder. Residual, 2 oz. The urine was slightly cloudy, and contained pus and colon bacilli.

Both these cases have already been reported [1]:—

Case III.—Male, aged 49, admitted to St. Peter's Hospital September 2, 1912.

History: Patient was treated for stricture twenty years before. In 1901 he was sounded for stone, but none found. Since then he had an attack of hæmaturia about every six months. For the last few months hæmorrhage at the end of almost every act of micturition. Old standing cystitis and pyelonephritis.

Present state: Frequency, every hour by day, six or seven times at night. Stream fair, precipitancy, never retention.

Urethroscopy: No stricture. Residual urine, 11 oz. Prostate normal.

Cystoscopy: Intense cystitis, especially on the base of the bladder. Ureters not visible. Opening of a large diverticulum near fundus, just to the left of the air bubble, and of a smaller one to the right of the middle line on the posterior wall.

Urine: Specific gravity 1010, alkaline, albumin present, no sugar, blood small quantity, urea 1·8 per cent., sediment muco-pus.

Operation, October 4, 1912: Bladder washed out, and distended with air. Suprapubic incision, bladder exposed. Patient then put in Trendelenburg position, and peritoneum opened. Diverticulum at the apex of the bladder, running up into the urachus. Peritoneum stripped off the upper surface of the bladder, and off the diverticulum. Peritoneal cavity then closed. Bladder opened in the middle line, incision prolonged upwards so as to surround diverticular opening. Urachus clamped and divided half way between the pubis and umbilicus, and diverticulum removed. At this point the urachus was found to be patent, and filled with fæculent looking material. It was followed up to the umbilicus, and entirely removed. Bladder sutured round a large tube, and the abdominal wound closed. The diverticulum on the posterior wall only admitted the tip of the index finger, and was not removed.

Patient died, on the twentieth day after operation, of chronic pyelonephritis.

Case IV.—Male, aged 61, admitted to St. Peter's Hospital March 10, 1915.

History: Had been treated for stricture on and off for thirty years. Urethroscoped in the out-patient department and no stricture found. Had a poor urinary stream all this time.

Present state: Frequency of micturition, every two hours by day, twice at night. Stream poor, straining, stream often interrupted, no precipitancy. Patient had numerous attacks of retention which necessitated catheterization. No pain, except slight scalding on micturition.

Cystoscopy: No cystitis, both ureters normal. Orifice of a fair sized diverticulum above and to the outer side of the right ureter. Prostate slightly enlarged. Residual urine, 2 oz.

Urine: Clear, acid, specific gravity 1018, no albumin nor sugar, urea 1·7 per cent., no deposit.

Operation, March 12, 1915: Trendelenburg position. Usual suprapubic incision. Diverticulum found to be about the size of a walnut. Freed from adhesions outside the bladder, invaginated into it, and cut away. Gap in the bladder wall sutured with continuous catgut. Prostate enucleated. Bladder closed round a large tube, lateral space drained.

During convalescence patient developed double epididymitis. Discharged May 9,

58 Joly: *The Operative Treatment of Vesical Diverticula*

1915. Wound firmly healed. Urine clear, no residual. Frequency, four hours by day, once at night.

Case V.—Male, aged 68, admitted to St. Peter's Hospital April 14, 1915.

History: For five years frequency of micturition, sometimes every half-hour. Suprapubic lithotomy, March, 1914; passed stones soon after his return home. Passed as many as thirteen in one day. Litholapaxy September, 1914, and again in October, 1914. Passed more stones after both these operations.

On admission: Frequency every three or four hours by day, twice at night. Stream good, but often interrupted, some urgency, no incontinence. No hæmaturia.

Urine: Turbid, offensive, alkaline, specific gravity 1018, albumin present, urea 1.3 per cent., much ropy muco-pus.

Bladder intolerant, cystoscopy impossible.

Cystogram: Bismuth suspension. Diverticulum size of a hen's egg on right side of bladder. Long narrow neck.

Operation, April 16, 1915: Trendelenburg position. Old scar excised. Peritoneum opened. With the help of a finger in the peritoneal cavity, the peritoneum was stripped off the bladder. Opening in peritoneum sutured, and the bladder opened. A loose stone was removed. Diverticulum found to be packed with calculi, which were removed. It was about the size of a hen's egg, and extended downwards to the base of the bladder. The ureter curved downwards and forwards, first behind and then below the orifice of communication. The diverticulum was very adherent, but was freed and removed from outside the bladder. Gap in bladder wall closed with two layers of continuous sutures. Prevesical space and bladder drained, rest of wound sutured. The orifice of the diverticulum just admitted the index finger. It was about half an inch above and to the outer side of the right ureter.

Convalescence uneventful. Patient discharged on May 26, 1915. Wound soundly healed. No frequency of micturition. Urine still turbid. No residual.

Case VI.—Male, aged 72, admitted to St. Peter's Hospital September 1, 1915.

History: Frequency of micturition and straining for three years. Prostatectomy January, 1914. Was treated for cystitis March and April, 1914. His condition improved for a few weeks, then grew steadily worse.

On admission: Frequency every ten minutes by day, every forty-five minutes by night. Stream dribbling, great straining. Occasionally passed clots. Severe pain along the urethra during micturition.

Urine: Turbid, ammoniacal, urea 1.4 per cent., much muco-pus, albumin present.

Cystogram: Bismuth suspension. Bilateral diverticula, each about the size of a walnut standing well out from the bladder.

Operation, September 3, 1915: Trendelenburg position. Old scar excised. Bladder very adherent to surrounding structures, especially to the posterior surface of the pubis. Adhesions divided, and bladder opened. The orifices of the diverticula were symmetrically placed, each about an inch above and to the outer side of the corresponding ureter. Bladder wall split down to the orifices, and the diverticula dissected free, and removed. Bladder wound closed round a large tube, and each lateral space drained. The whole operation was rendered difficult and tedious on account of adhesions.

Patient discharged from hospital, September 29, 1915. Wound firmly healed; frequency two hours by day and night. Attended the out-patient department for a few months; during that time his urine became clear, and the frequency of micturition abated.

Case VII.—Male, aged 49, admitted to St. Peter's Hospital June 20, 1916.

On admission: Frequency of micturition eight to ten times during the day, twice at night. Pain along the urethra during the act. No stricture, no enlargement of the prostate.

Urine: Bacilluria, no albumin nor sugar, urea 1.1 per cent. Residual urine, 9 oz.

Cystoscopy: Base of bladder, and all round the internal meatus œdematous. Opening of a diverticulum just above and to the inner side of the right ureter. Ureteric orifices normal in appearance.

Operation, June 28, 1916: Trendelenburg position. Bladder widely opened. Diverticulum admitted index finger to a depth of almost 3 in. Incision round orifice, and diverticulum freed by blunt dissection. A quarter-inch tube passed through the bladder, and into the space from which the diverticulum had been removed. Gap in bladder sutured round it. Bladder drained.

Patient discharged August 4, 1916. Wound healed, no residual, no frequency.

Case VIII.—Male, aged 71, admitted to St. Peter's Hospital August 28, 1919.

History: Fifteen years before had some difficulty in passing urine for three or four days. Well until April, 1919, when sudden retention of urine occurred, followed by a severe fit of shivering. Catheterized. Since then on complete catheter life, passing the instrument three or four times a day, and once at night. Previous to April, 1919, frequency every two hours by day, and every hour by night.

On admission: Decidedly senile. *Per rectum*, prostate not enlarged. Cystoscopy. Slight intravesical projection of prostate. In the neighbourhood of right ureter, round orifice of an enormous diverticulum. Orifice about the size of a shilling, cavity very dirty. Right ureter could not be seen; left, normal in position. Severe general cystitis.

Cystogram: Thorium solution. Oblique position showed a double cavity, with the diverticulum low down in the right side of the pelvis. It was about the same size as the bladder. Antero-posterior position, showed cavity overlapping bladder, and extending far to the right.

Urine: Specific gravity 1010, acid, albumin and sugar present, urea 0.8 per cent.; 183 gr. passed in the twenty-four hours.

Operation, September 5, 1919: Stovaine. Trendelenburg position. Median incision from pubis to umbilicus, bladder widely opened. With the hand in the bladder, the finger could just reach to the bottom of the sac. An attempt was made to invaginate the diverticulum into the bladder, by blunt dissection from without, aided by traction by means of a volsellum passed through the orifice. Dense adhesions binding the sac to the seminal vesicles, prostate, and rectum made it impossible to complete this invagination; however the upper wall of the diverticulum was completely invaginated. A semicircular incision was then made, severing the upper portion of the neck of the sac, and the diverticulum was inverted through this opening. This made it possible to reach its outer wall, and the surrounding tissues were then easily separated off by means of gauze dissection. The whole of the diverticulum was by this means turned inside out, and invaginated into the bladder. It was so large that more than half of it came through the abdominal wound. As the vesical orifice of the right had never been identified, the ureter itself was exposed in the pelvis and opened. A catheter was passed down it, and appeared between two folds of mucous membrane, just within the diverticulum, opposite the lowest point of the opening. The sac was then cut away, except for a small semicircular flap surrounding the ureteric orifice. This flap was stitched over the defect in the bladder. The ureter was sutured, and the bladder wound closed round a large tube. The lateral vesical space was drained, and the abdominal wound closed round the tubes.

The patient bore the operation well, but convalescence was slow. The suprapubic sinus closed after a catheter had been tied in for a fortnight. Patient sent to a convalescent home October 25, 1919. Wound healed, but patient entirely dependent on his catheter.

Readmitted April 10, 1920. On catheter life since operation. Prostate, lateral lobes slightly enlarged, smooth, and movable. Cystoscopy. Shallow depression at site of diverticular orifice. Small intravesical projection of middle lobe of prostate. Severe chronic cystitis.

Urine: Specific gravity 1020, acid, small quantity of albumin, moderate quantity of sugar, cloudy yellow colour, deposit of mucus. Phenol-sulphone-phthalein (intravenous injection) appeared in three minutes; 44 per cent. excreted in the first half hour, and 11 per cent. in the second. Urea concentration test: 1.9 per cent. in the second hour, and 1.7 per cent. in the third.

Prostatectomy, April 23, 1920: Stovaine. Bladder opened after it had been freed from the pubis. Peritoneum pushed upwards from the scar. Small prostate easily enucleated. Very little bleeding.

60 Joly: *The Operative Treatment of Vesical Diverticula*

Patient discharged May 18, 1920: Wound healed. Frequency, two hours by day, twice at night. Residual urine, 1 oz. Urine turbid. He attended the out-patient department for a few months. General condition improved very much. Frequency, normal.

I heard from this patient on March 20, 1923. General health good. No retention. Frequency, four hours by day, three times at night. Stream good, urine clear. No pain. No leakage through scar.

Case IX.—Male, aged 50, admitted to St. Peter's Hospital December 28, 1920.

History: Difficulty in micturition for twelve months, which was steadily getting worse. Frequency, every hour by day, not at all during the night. Had cystotomy done one month before for over-distension, told he had a large pouch in his bladder.

On admission: Suprapubic sinus, through which all the urine came. Urine very foul, sinus surrounded by unhealthy granulations. Prostate slightly enlarged, hard nodule right lobe, whole gland movable.

Cystotomy December 31, 1920: Sinus enlarged, opening of a large diverticulum found just outside right ureter. Bladder very foul, almost pure pus coming from diverticulum. Tube inserted into sac, and another into bladder. Daily irrigations.

Excision of diverticulum, January 14, 1921: Stovaine and general anæsthesia. Trendelenburg position. Bladder widely opened, and freed from adhesions. Contents of diverticulum still very foul. Diverticulum freed from surrounding structures, invaginated into bladder and removed. Operation very difficult on account of dense adhesions. Gap in bladder sutured. Suprapubic wound closed round large tube. Lateral vesical space drained.

March 18, 1921: Patient discharged. Slight suprapubic leak. Passing urine freely and without pain.

June 21, 1921: Readmitted. Double pyelonephritis. Sinus enlarged, de Pezzer tube inserted. Patient sent to infirmary.

(As I was out of town, I did not see this patient on his readmission.)

Case X.—Male, aged 51, admitted to St. Peter's Hospital June 26, 1920.

History: Frequency of micturition for two years, gradually getting worse. Difficulty in micturition for one year. Ten weeks before admission complete retention. Metal catheter passed under general anæsthesia, but as it could not be passed a second time a perineal section and suprapubic cystotomy was performed. A diverticulum was then discovered. Bladder drained for ten weeks "in the hope that the diverticulum would contract." Venereal disease denied.

On admission: Suprapubic and perineal fistulæ. Most of the urine came through the former opening. *Per rectum:* prostate not enlarged. In the region of the left seminal vesical a soft, elastic, sausage-shaped tumour was felt. It appeared to be adherent to the pelvis.

Urine: Specific gravity 1020, cloudy yellow, acid, albumin present, no sugar, deposit of pus. Urea concentration test, 1.65 per cent. in the second hour.

July 2, 1920: Stovaine. Urethra easily dilated to 14/16. 22 F. catheter tied in. Perineal wound allowed to heal.

Cystotomy, July 23, 1920: Stovaine and ether. Scar excised, and bladder opened. Diverticulum discovered on the left side. Opening about an inch above and to the outer side of left ureter. It easily admitted the index finger. Cavity very large, about the size of the clenched fist, was filled with exceedingly foul pus. Tube inserted into diverticulum and another into the bladder. Wound partially closed.

Continuous irrigation was attempted, the fluid entering through the tube in the diverticulum and returning by the bladder tube, but it failed, as the diverticular orifice contracted round the tube placed in it, and did not allow the fluid to circulate.

Excision of diverticulum, July 28, 1920: Stovaine, and ether after the first hour. Cystotomy wound reopened. Bladder wall very thick and indurated, and this made it difficult to expose the interior of the viscus. Much pericystitis. Left lateral wall split down to the diverticular orifice. Sac very adherent; was torn during removal, but the whole of it was got away. Peritoneum twice torn and sutured. Left ureter

localized, and kept from injury. Left vas cut across. Bladder sutured round a large tube. Lateral space drained.

Fowler position, Murphy's rectal drip.

Patient discharged from hospital August 30, 1920. General condition good. Wound soundly healed. Urine acid, slight deposit of pus. Could hold urine for two or three hours. Residual less than 1 oz.

March 30, 1928: I heard from this patient. General health fair. Frequency by day six times, once at night. Urine clear. No pain. Stream fair. No leakage through wound.

Case XI.—Male, aged 53, admitted to St. Peter's Hospital March 4, 1922.

History: Since 1915 frequency of micturition, later difficulty and straining. Urgency and some incontinence for three years. For four months has catheterized himself every day, but was able to pass a small quantity of urine naturally.

On admission: No stricture. Prostate slightly enlarged, soft, elastic, could be felt bimanually, freely movable.

Cystoscopy: Residual urine 9 oz. Very marked trabeculation. Numerous small saccules. Diverticulum on right side, at junction of posterior and lateral walls. Stone in post-prostatic pouch. Ureteric orifices not seen on account of intravesical projection of middle lobe of prostate. No cystitis.

Urine: Clear, acid, specific gravity 1017, no albumin nor sugar; daily urea, 1.8 per cent.; urea concentration test, 3 per cent.

Cystogram: Sodium iodide solution. Large diverticulum right side, long narrow neck. Numerous small saccules especially near the base of the bladder.

Operation, March 15, 1922: Suprapubic incision. Calculus removed. Bladder wall very thick, much pericystitis. Bladder wall split down to orifice of diverticulum, and sac excised. Right ureter and vas adherent to it. Prostate enucleated, and prostatic cavity packed with gauze. Wound in bladder sutured round a half-inch tube. Small tube in lateral space. Wound closed.

April 15, 1922: Patient discharged from hospital. General condition excellent. Suprapubic wound healed, passing water freely *per urethram*.

This patient was seen on February 26, 1928. Frequency four hours by day, occasionally once at night. Residual urine 1½ oz. Urine slightly cloudy. General condition very good.

Case XII.—Male, aged 37, admitted to St. Peter's Hospital, April 1, 1922.

History: Eighteen months before, pain in the hypogastrium, which had no relation to micturition. Six months later frequency and difficulty in micturition. Frequency then seven or eight times by day, twice or three times by night. Stream poor, only a small dribble, urgency. April, 1921: Acute retention, lasted two days; similar attack next month. July, 1921: Suprapubic cystotomy for stone, but none found. Since then had passed a catheter once or twice a day on himself.

On admission: Weak suprapubic scar, which broke down from time to time, and leaked urine. Bladder easily palpable when distended. Catheter life. Prostate normal.

Cystoscopy: Distention 22 oz. Chronic cystitis especially in region of trigone. No trabeculation. Two diverticula: (1) Just above and to the outer side of the left ureter; contents turbid, orifice about the size of a lead pencil. (2) Small one just above right ureter. Trigone displaced to right so that the left ureter lay almost in the middle line. Both orifices normal. Edematous mucous membrane round orifice of large diverticulum. Both ureters catheterized. Clear urine from both kidneys. Urea concentration test: Both kidneys gave 3 per cent. concentration. Same test applied to bladder urine gave 1.42 per cent. Internal urethral orifice normal.

Cystogram: Sodium iodide solution; 20 oz. of fluid injected. Bladder displaced to the right. Very large diverticulum filling up left half of true pelvis. Bladder emptied as much as possible by catheter, and second exposure made. Diverticulum still distended, and a small amount of fluid in the bladder itself.

Urine: Turbid, alkaline, no albumin nor sugar; daily urea, 1.8 per cent. in 50 oz., or 306 gr.

62 Joly: *The Operative Treatment of Vesical Diverticula*

Operation, April 19, 1922: Left ureter catheterized. Trendelenburg position. Scar excised, bladder freed from pubis and opened. Diverticulum freed by blunt dissection, and gradually invaginated into bladder. Left ureter lay behind and below neck of sac, and was separated off. Diverticulum removed together with a small portion of bladder wall surrounding neck. Gap in bladder sutured. Lateral space drained, large de Pezzer tube in bladder. Wound closed. Diverticulum on right side too small to admit tip of finger, not removed.

May 8, 1922: Patient discharged from hospital. Wound healed. Micturition normal. Frequency three hours by day, once at night. Residual urine $1\frac{1}{2}$ oz.

March 20, 1923: I heard from this patient. General health steadily improving since operation. Frequency three or four times by day, twice by night. Stream "splendid." No pain. Wound soundly healed.

Case XIII.—Male, aged 62, admitted to St. Peter's Hospital January 5, 1923.

History: Ten years ago hæmaturia after straining at stool. Since then pain "in the bladder" whenever he has a cold, and occasionally a trace of blood in the urine. Five years later frequency and difficulty in micturition. Soon afterwards catheter life, passing the instrument twice or three times a day. Cystoscoped in 1912, and told "he had a sac." Admitted to St. Peter's Hospital October 20, 1919. Prostate removed. Discharged December 27, 1919. Had intense cystitis at the time, and diverticulum apparently not noticed on cystoscopy. Suprapubic wound took twenty-two months to heal. Patient washed his bladder out every day with nitrate of silver by Janet's method. Had slight hæmaturia shortly after leaving hospital, and passed several small stones.

On admission: Frequency of micturition six times by day and twice at night. Micturition normal, stream and projection both good. Abdomen: Suprapubic scar, weak in its lower portion. When bladder was distended, it could be felt in right iliac fossa. Right kidney not palpable, left completely palpable, movable, enlarged, painless.

Cystoscopy: Residual urine 30 oz. Bladder capacity 50 oz. Generalized cystitis with irregular bulging and sacculation. Right ureteric orifice active, fairly healthy; left large, patent, never closes. Opening of diverticulum fairly high up on the left side, difficult to see because left wall of bladder was pushed almost to middle line by the diverticulum. It appeared to be about the size of a sixpenny piece.

Cystogram: Sodium iodide, 5 per cent. solution; 48 oz. injected. Enormous bladder pushed over into the right iliac fossa. Diverticulum filled left side of true pelvis. Small slit-like space between bladder and diverticulum. Narrow orifice of communication. When bladder had been emptied as much as possible by means of a catheter, diverticulum remained nearly full, and there was still some fluid in the bladder itself.

Urine: Hazy, specific gravity 1012, alkaline, no albumin nor sugar; daily urea, 1 per cent. in 75 oz., or 322 gr. Urea concentration test, 1.5 per cent. in 3.5 oz., or 22 gr.

Operation, January 10, 1923: Old scar excised, and bladder opened. Trendelenburg position. Orifice of diverticulum about $\frac{3}{4}$ in. in diameter, situated about 1 in. above and to outer side of left ureter. Catheter passed up left ureter. Diverticulum freed by finger and gauze dissection, and removed from outside the bladder. Gap in wall sutured by two layers of continuous catgut sutures. Internal urethral orifice about the size of a lead pencil, but was incised as there was a "shelf" above prostatic cavity. Marion's tube in bladder, lateral space drained. Wound closed, catheter tied in.

Patient discharged from hospital February 19, 1923. Wound firmly healed. Micturition normal, no frequency. Residual urine $\frac{1}{2}$ oz.

I saw this patient on March 12, 1923. General condition excellent. Micturition normal. Frequency four hours by day, once at night. Urine clear, a few small flakes in it.

Case XIV.—Male, aged 69, admitted to St. Peter's Hospital January 7, 1923.

History: Three years ago difficulty in starting micturition. A few months later

pain at tip of penis before and during act. Nocturnal frequency for eighteen months. Complete retention two years ago.

On admission: Bladder distended to umbilicus. Micturition, much straining, delay, and pain. Stream in drops only, some after dribbling. Prostate only slightly enlarged, rather firm. No stricture. Put on regular catheterization, 18 oz. being drawn off each time the instrument was passed.

Urine: Turbid, alkaline, specific gravity 1022, no albumin nor sugar, much pus daily urea, 2.5 per cent. in 80 oz., or 327 gr.

No cystoscopy when in hospital on account of over-distention.

January 10, 1923: Suprapubic cystotomy. Trivial intravesical projection of prostate. Diverticulum size of a hen's egg, above and to the outer side of left ureteric orifice. De Pezzar tube inserted.

January 24, 1923: Diverticulum excised from outside bladder. It was very adherent. Gap in bladder wall sutured with two layers of continuous catgut. Some thick perivesical fat sutured over opening. Prostate, size of a cherry, enucleated. It was fibrous, and contained several small calculi. Cavity packed. Marion's tube in bladder. No tube in lateral space.

Sharp attack of secondary hemorrhage fourth day. February 5, 1923: Large abscess left side of bladder; tube inserted. Since then did well. March 7, 1923: Passed urine naturally, but fistula not quite closed. March 20, 1923: Still in hospital.

THE RELATIVE VALUES OF THE DIFFERENT OPERATIVE METHODS.

It is universally admitted that the only satisfactory treatment of this condition is excision of the sac, but there is a considerable divergence of opinion as to the best method of performing the operation. However, before discussing this question, I wish to point out the dangers of a preliminary cystotomy in these cases. A cystotomy does not drain the diverticulum. When the bladder contracts round the cystotomy tube, the diverticular orifice closes, and the cavity is completely shut off from the bladder. Under these conditions no amount of vesical irrigations will disinfect the diverticulum, and its contents invariably suppurate. The deplorable condition that patients Cases IX and X were in when I first saw them was undoubtedly due to this cause. In both cases I tried to control the suppuration within the diverticulum by passing a tube into the sac through its vesical orifice. But in neither case was it possible to wash out the sac into the bladder, or the bladder into the sac. Both cavities could be washed out independently of the other, but this was not satisfactory, and in both cases I was forced to operate while the sac was still in a filthy condition. The walls of these sacs have practically no power of contraction, and remain in an inert flaccid condition, which predisposes them to stagnation and infection. When I am called upon in future to deal with such grossly infected diverticula, I shall, as a preliminary to excision, open the bladder, and pass two small tubes into the sac, as well as leaving a larger one in the bladder itself. Continuous irrigation can then be carried out, the fluid entering the diverticulum by one small tube, and leaving it by the other. It is, in many cases, possible to drain the diverticulum independently of the bladder, but I do not see any advantage in this procedure. If the diverticulum lies below, and behind the bladder, it can only be satisfactorily reached from the perineum, and this perineal drainage may render subsequent removal of the sac exceedingly difficult, if not impossible. I consider that a preliminary cystotomy should not be done, except when the kidneys are damaged so severely that it is obvious that the patient would not stand a primary excision. Even in these cases the bladder should only be

drained for as short a time as possible, and means must be taken to drain and irrigate the diverticulum independently of the bladder.

Turning to the operation of excision, I feel that no set operation is suitable for all cases, but I think there are certain broad indications which would lead the surgeon to choose beforehand the operation best suited to the particular case. I also feel that it is not always possible to institute a definite plan of campaign until the bladder has been exposed, as so much depends on the amount of pericystitis present, and on the number and density of the adhesions binding the diverticulum to neighbouring structures. One can learn the size and position of the orifice, the amount of trabeculation of the bladder wall, and the severity of the infection, by means of cystoscopy; and the size, position, and to a certain extent the relationship of the diverticulum, by means of cystograms; but one has no means of telling beforehand whether the sac is bound down by adhesions, or lying free in the fatty tissue of the pelvis.

With regard to the operation itself. Diverticula may be excised (1) from the outer surface of the bladder, (2) by splitting the bladder wall down to the orifice of the diverticulum, and (3) from within the bladder.

(1) *Excision from without the Bladder.*—This operation is best suited for diverticula situated high up on the lateral walls of the bladder, and for those occurring at the urachus. The operation may be either trans- or extraperitoneal. A transperitoneal operation is indicated if the orifice of the diverticulum lies above the line of reflection of the peritoneum from the bladder wall, but these cases are not common, as the orifice is usually situated well below this level. In some cases it is a help to open the peritoneum when stripping it from the upper wall of the diverticulum, but if this is done the bladder should not be opened until the abdominal cavity has been again closed. I have performed this operation on five of my cases (Nos. I, III, V, XIII, XIV), in two of them (Nos. III and V) I opened the peritoneum to facilitate stripping it back from the wall of the sac. The operation is facilitated if the bladder is opened, and one finger placed in the diverticulum, while the upper and outer walls of the sac are cleared by gauze dissection. When dealing with the lower portion of the diverticulum, the finger in the sac is of no further assistance. The wall of the diverticulum is then grasped low down by means of a volsellum, and aided by traction on it the dissection is completed. I have not found it any advantage to plug the sac with gauze, and if the diverticulum lies low down in the pelvis, it is a positive disadvantage. The plug fills up almost the whole of the narrow space one has to work in, and renders the dissection much more difficult. I have tried this method on several occasions, and always had to remove the gauze again in a few minutes. This operation is rendered difficult if the bladder walls are thick and indurated, if the diverticulum is also thick walled, and if there is much pericystitis.

(2) *Splitting the Bladder Wall down to the Orifice of the Diverticulum.*—This operation was first described by me in a paper read before the Urological Section of the Seventeenth International Congress of Medicine, which met in August, 1913 [2], and quite independently by Marion in November of the same year [3]. However in my case the operation was transperitoneal, while in Marion's it was extraperitoneal. The idea underlying both these operations was the same, and was an attempt to obtain better exposure of the deeper parts of the diverticulum. I performed this operation in Case II (transperitoneally), and in Cases VI, X, and XI (extraperitoneally). Case VI was one of bilateral diverticula, and both sides of the bladder were split down to

the necks of the diverticula. The incision is really a racquet, with a long handle and a small blade, as the orifice of the diverticulum is encircled and a small portion of the bladder wall removed with the sac. The operation is indicated (1) in cases in which the diverticulum is situated low down on the posterior wall of the bladder, (2) when the walls of the bladder or diverticulum are thick and inelastic, and (3) when there is marked pericystitis. Case II came under the first category, and I do not think I could have removed this diverticulum by any other means. In Cases VI, X, and XI, troublesome adhesions were present, due in the first case to a previous prostatectomy, in the second to a prolonged suprapubic and perineal drainage, and in the third to marked pericystitis. In Cases VI and XI the bladder walls were very thick. A rough estimate of the thickness of the bladder wall can sometimes be obtained by means of a simple cystogram. In every case of vesical diverticulum I have seen, the walls of the bladder and of the diverticulum are in contact with each other round the diverticular opening. This means that the length of the neck of the diverticulum is equal to the combined thickness of these two walls, so that if the diverticular shadow lies at a considerable distance from that thrown by the bladder, and the two are only connected by a long isthmus, it is fairly safe to assume that the bladder walls are thick. I may also add that the walls of the bladder are always thicker than those of the diverticulum.

(3) *Intravesical Operations.*—(a) Encircling the orifice by an incision through the whole thickness of the bladder wall, and removing the diverticulum by blunt dissection. I have only performed this operation once (Case VII), and have completely given it up, as I consider it dangerous. It is only suited for small diverticula, and it is dangerous, because there is great risk of wounding the ureter, which always lies in close relationship with the neck of the diverticulum.

(b) *Invagination of the Sac.*—This operation should be performed partly from inside the bladder, and partly from outside it. It is a dangerous procedure to insert a volsellum through the diverticular opening, seize the fundus of the sac, and attempt to invaginate it. One operator drew a loop of small intestine into the bladder by this method, but fortunately discovered his mistake before dividing the neck of the sac. If it is decided to remove a diverticulum by this method, the upper wall of the sac should first be cleared as far as possible from the outer side of the bladder, and also from the surrounding structures. This portion of the diverticulum should then be invaginated through the orifice, and seized from within. The dissection can then be completed from without, while traction is made by means of a volsellum gripping the portion already invaginated. This operation is only suitable for thin-walled diverticula, that are comparatively free from adhesions. Young employs suction to invaginate the diverticulum [4]. This method requires an electric pump, as the suction obtained by means of a Sprengel's pump is quite insufficient for the purpose. I have performed the operation of invagination in three cases (IV, IX, and XII); I attempted it in Case XIII, but was unable to complete the invagination, so I turned the sac back, and removed it from outside the bladder.

(c) *Combined Invagination and Inversion of the Sac.*—This operation has been fully explained when describing Case VIII (p. 59), so I need not recapitulate it. I believe this method has not been described before, and I consider it a definite advance on the usual invagination method. As a matter of fact I failed to

invaginate the sac in this case, and I think I would have been unable to remove the diverticulum if I had not thought of inverting it. It can be employed in diverticula springing from any part of the bladder, but is particularly useful for dealing with large sacs situated deep down in the pelvis. It is eminently safe, as the outer surface of the diverticulum is well seen while the adhesions are being separated from it. Of course, it cannot be employed in cases in which the wall of the sac is too stiff to allow of invagination, but this is the only definite contra-indication.

OPERATIVE TREATMENT IN CASES COMPLICATED BY ENLARGED PROSTATE.

Cases of vesical diverticula have frequently been diagnosed as enlargement of the prostate, and the gland has been removed before the presence of the diverticulum was suspected. This happened in Cases VI and XIII of my series. I feel that in both these cases the patient's condition was rendered worse by this procedure. I have already alluded to the bad results obtained in cases where the bladder has been simply drained, and everything I have said regarding cystotomy applies with equal force to prostatectomy. I feel that the ideal procedure is to remove both the diverticulum and the prostate at the same sitting. This was done in Cases IV, XI, and XIV, with excellent results. If, however, it puts too great a strain on the patient's powers of recuperation, the diverticulum should be removed at the first sitting, and the prostate enucleated as soon afterwards as is feasible. This course was followed in Case VIII. Lastly, if the patient's condition is so poor that he will not stand any major operation, the bladder should be opened, and independent drainage provided for the diverticulum in the manner already described. Even under these circumstances, the preliminary drainage should be of as short a duration as possible.

VESICAL DIVERTICULA COMPLICATED BY STONE.

(1) *Calculus lying free in the Bladder.*—The mere removal of such a stone is never a complete operation. There may be others in the diverticulum. Even if there are none, the diverticulum remains, and will probably give rise to symptoms. I do not think that a litholapaxy should ever be performed in cases where there is a definite diverticulum. A case on which I operated some years ago illustrates this point. The patient had a stone about the size of a filbert lying free in his bladder, and on cystoscopy a small shallow diverticulum was seen just to the outer side of the right ureter. I thought it was too small to give rise to any symptoms, and so decided to crush the stone. It was caught with the lithotrite, but when I commenced to crush it, it slipped out of the jaws of the instrument, and I could not find it again. I then cystoscoped the patient, and found the stone in the diverticulum. The bladder was opened, and the stone removed from the sac, which was just large enough to hold it comfortably. The orifice of the diverticulum was then enlarged downwards, and the wound closed. What happened to the whole calculus in this case may easily happen to fragments in others, and I feel sure this is one of the causes of recurrence in these cases. Case V also bears out this point. The patient underwent a suprapubic lithotomy, and two litholapaxies in the space of about a year, yet when I operated there was a stone lying free in his bladder, and the diverticulum was packed full of them. I feel strongly that the correct procedure in all these cases is to open the bladder, remove the calculus, and excise the diverticulum.

(2) *Stone in the Diverticulum.*—In this case the indications for removing the diverticulum are even stronger. Mere removal of the calculus may make the patient's condition worse, just as a simple cystotomy does. A very interesting case illustrating this point has been described by Kummer and Brutsch [5]. Their patient suffered from the usual symptoms of stone for over twenty years. He never had retention, and there was only a small quantity of residual urine. A stone was felt in his bladder, and the diagnosis confirmed by radiography, which showed a shadow filling up the greater part of the true pelvis. The bladder was opened, and two calculi were found lying free in it, while a third was seen projecting from the orifice of a very large diverticulum, which it filled completely. They were all removed, but the diverticulum was left *in situ*. After the operation, the patient was absolutely unable to pass urine naturally, and had to rely on self-catheterization. The authors came to the conclusion that his retention was not due to atony of the bladder, but to the presence of the diverticulum. They advised an operation for its removal, but the patient refused it.

If possible, the diverticulum and the stone in it should be removed at the same sitting, but if the patient's condition will not stand this operation, one must remove the stone first. In this case the diverticulum must be drained independently, and the operation for its removal performed as soon as possible after the lithotomy.

RELATIONSHIP OF THE URETER.

It is important for the operator to bear in mind the close relationship between the ureter and the neck of the diverticulum. In the first place, the orifices of the "lateral group" of diverticula are always found in a limited oval area of the bladder wall, which lies above and to the outer side of the corresponding ureteric orifice. Occasionally the ureter opens into the diverticulum itself, in which case its orifice lies just within the lower and internal quadrant of the mouth of the sac. Outside the bladder the relationship is even more intimate. If the ureter is traced upwards from the point where it enters the bladder wall, it will be found to lie at first immediately below the lowest point of the neck of the sac. It then passes backwards and upwards behind it. This means that it follows the curve of the lower and posterior quadrant of the opening. If the ureter is traced still further upwards, it will be found to lie either in the cleft between the bladder and diverticulum, or to occupy a deep groove in the posterior wall of the latter. If the diverticulum stands well away from the bladder, the ureter lies between the two, but if it is in close contact with the bladder wall, and especially if it lies partly behind the bladder, the ureter occupies a deep *suleus* in its posterior wall. In either case the ureter is compressed, or displaced by the drag of the sac; and dilatation of its upper part, and of the corresponding renal pelvis, has frequently been observed. In eight of my cases the ureter was exposed during operation, and on each occasion it was found either to be dilated or thickened. If the ureter opens into the diverticulum itself, it usually tunnels through the lower and posterior quadrant of the neck. This is simply an exaggeration of the usual relationship. The proximity of the ureter to the diverticular orifice must be borne in mind whenever the condition is treated by splitting down the partition between the sac and the bladder. Such an incision should not be made directly towards the internal meatus, but should be curved forwards a sufficient distance in order to avoid any chance of injury to the ureter.

RESIDUAL URINE.

Perhaps one of the most remarkable phenomena connected with vesical diverticula is the large amount of urinary obstruction they cause. In fact obstruction and infection are the most usual indications for operation. The amount of residual urine is usually measured by passing a catheter immediately after the patient has emptied his bladder as completely as possible by micturition, but in some cases, especially if the diverticulum is large, it is impossible to drain off all the urine by this means. The amount left behind is often considerable, and may for convenience be called the "concealed residual." I first suspected the presence of this concealed residual in Case II. At the operation, the bladder was washed out, and emptied as completely as possible, and then filled with air. Yet when it was opened the diverticulum was full of lotion. The presence of this concealed residual was also demonstrated by means of the cystograms taken in Cases XII and XIII. In both these cases, the first cystogram was taken when the bladder was filled to repletion. It was then emptied as completely as possible by means of a catheter, and the second exposure made. In both cases the diverticulum remained full, and in addition there was a distinct shadow due to the presence of a certain amount of opaque fluid in the bladder itself. Although it is easy to demonstrate the presence of this concealed residual I do not know of any means of accurately estimating its amount. This concealed residual is of great practical importance, as it renders fallacious all attempts to estimate the functional activity of the kidneys, by means of tests applied to the bladder urine. This applies both to the elimination of dyes and to the urea concentration test, and if these methods are used the urine should be obtained directly from the kidneys by means of ureteric catheterization. For example, in Case XII, the urea concentration test carried out on bladder urine gave only 1.4 per cent., while the same test carried out on urine obtained by catheterization of both ureters gave 3 per cent. of urea from each kidney. The latter was the true estimation of the renal function; in the first case the urine was diluted with lotion which could not be evacuated from the bladder by means of the catheter. If it is impossible to catheterize the ureters, one must depend on an estimation of the blood urea.

DIAGNOSIS.

In the majority of cases the diagnosis of the presence of a diverticulum is made by means of the cystoscope. The appearance of the orifice of a large sac is so characteristic that it is impossible not to recognize it at a glance. Occasionally, however, cystoscopy is rendered difficult on account of intense cystitis, or the orifice may be hidden by bullous œdema or a growth, or if the bladder has not been fully distended it may lie between two folds of mucous membrane. In these cases a cystogram should be made. The diagnosis of the size and relationship of the diverticulum can be made by means of cystograms, but to obtain the best results the technique must be carefully thought out. I now use a solution of sodium iodide which has been boiled and allowed to cool to the body temperature. The strength of the solution depends on the capacity of the bladder, which should be estimated beforehand. If it is less than 10 oz. I use a 15 per cent. solution, if it lies between 10 and 20 oz. a 10 per cent. solution is best, while if the capacity is more than 20 oz. a 5 per cent. solution is quite sufficient. If too strong a solution is used, the shadow is so dense that it is difficult to distinguish between the shadow thrown by the diverticulum

and that thrown by the bladder. In Case XII a 30 per cent. solution was used, but the shadow was much too dense, and there was practically no differentiation between the bladder and diverticulum. As a general rule, the weakest opaque solution that will give a satisfactory shadow is the best one to use. When the patient is on the X-ray couch, a catheter should be passed, and the contents of the bladder evacuated as completely as possible. The opaque solution should then be run in, either by means of a syringe or by gravity, until the patient experiences a desire to micturate. A spigot is then placed in the catheter, and a radiogram taken in the antero-posterior position. The patient is then rolled over on his side, until the shadow of the diverticulum is distinct from that of the bladder. The best position is easily determined by means of the fluorescent screen. If the diverticular opening is in the posterior part of the bladder, as it nearly always is, the patient should be rolled to the opposite side. A second exposure should be made in this position. Lastly, without altering the patient's position, the spigot should be removed from the catheter, and the bladder emptied as completely as possible. A third exposure should then be made. This is to ascertain if there is any "concealed residual urine."

CONCLUSIONS.

- (1) Excision of the sac is the only rational treatment of vesical diverticula.
- (2) No single set operation is suitable for all cases.
- (3) If the case is complicated by prostatic or urethral obstruction, both conditions should be treated at the same sitting. When this course is not feasible, it is better to remove the diverticulum first, and treat the obstruction at a later sitting.
- (4) A preliminary cystotomy does more harm than good in most cases.
- (5) The presence of a calculus either in the bladder or in the sac is an indication for excision of the diverticulum.
- (6) As a rule the ureter lies in close relationship with the neck of the sac, and must be guarded from injury during operation.
- (7) The contents of a diverticulum cannot always be evacuated by catheterization. The urine left behind after catheterization, often a considerable amount, I have termed the "concealed residual."
- (8) The presence of concealed residual urine vitiates all tests of the renal efficiency which are carried out on bladder urine. To obtain an accurate estimate of the renal function it is necessary either to catheterize the ureters, or to trust to an estimation of the blood urea.
- (9) The presence of a diverticulum is best diagnosed by means of the cystoscope, and its size and position ascertained by means of cystograms.

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Section of Urology.

President—Sir JOHN THOMSON-WALKER, F.R.C.S.

The Incidence of Malignant Disease in the apparently Benign Enlargement of the Prostate.

By R. H. JOCELYN SWAN, O.B.E., M.S.

I SHALL begin by making a clear definition of the point I wish to raise; it is not my purpose to discuss those cases of prostatic enlargement which are obviously carcinomatous or even suspicious of carcinoma when they first come under the notice of the surgeon, but rather those cases which, clinically, present all the features of the ordinary simple prostatic enlargement, and which are operated upon as such, but in which, owing to some unusual feature in the operation or upon subsequent microscopical examination of the organ removed, malignant disease is found to be present as an unsuspected complication. Four such cases have recently come under my observation, and I am therefore bringing the subject forward in the hope that some points which may have escaped me may be elucidated and assist us in arriving at a correct diagnosis before operation is advised. Thus I eliminate at once those cases of alteration in micturition, with difficulty, frequency and perhaps hæmaturia, which on rectal examination present a firm, inelastic, or nodular swelling of the prostate, with fixation or lateral infiltration in the pelvic lymphatic planes, and I confine myself to those cases in which the digital examination of the prostate *per rectum* shows a soft, elastic, movable enlargement of the gland of uniform consistence which has been looked upon as the type of enlargement variously spoken of as adenomatous, cystic adenoma, hyperplasia, or as chronic lobular prostatitis.

Perhaps it would be best at this stage briefly to record the cases upon which my remarks are based:—

Case I.—J. C., aged 75. Increasing frequency of micturition began about five years ago; this progressed gradually until micturition took place every hour by day and about five times at night, with occasional involuntary dribble. He has to strain to micturate, but there is no pain, no backache, and no increased thirst. No hæmaturia.

On examination: Rather feeble. Pulse 76, vessels slightly thickened. Blood-pressure 160 mm. Neither kidney nor liver palpable. The bladder not felt to be distended. Urine passed in forceless dribble: clear, acid, specific gravity 1021; No albumin nor sugar. Urea 2·8 per cent. Catheter passed easily; residual urine, 8½ oz. *Per rectum*: Prostate uniformly enlarged, smooth, elastic and movable. Easily felt bimanually.

AC—U 1

[May 31, 1923.]

Operation, July 20, 1921: Prostate projected into bladder as a apple-like swelling, smooth and elastic. Enucleation easy, though there was some increased fixation in posterior part of gland. On subsequent examination with patient in the Trendelenburg position and the cavity illuminated, two small firm nodules were found in the posterior part of the prostatic cavity and were removed.

The prostate removed appeared to be of the ordinary fibro-adenomatous type, but it was rather ragged on the posterior aspect. On microscopic examination of this portion of the gland and also of the small nodules removed from the posterior part of the cavity, adenocarcinoma was found to be present.

When seen again four months later, he looked well and passed urine comfortably and with good stream. Frequency five times during day and once at night. A No. 14 steel sound passed easily, but there was a feeling of roughness upon its passing through the prostatic area like rubbing against wet wash-leather. *Per rectum* the prostatic area was found to be small, but there was a firm band of infiltration as thick as the forefinger passing outwards and upwards on the right side, in the area of the pelvic lymphatics. No glands felt.

The patient died on August 17, 1922, thirteen months after the operation, from progressive weakness, but Dr. Hovenden wrote that at no time had he the slightest difficulty in passing a full sized catheter. No post mortem was obtained.

Case II.—Major A., aged 61. Seen with Dr. Shackleton on December 1, 1922. In December, 1919, he consulted his doctor owing to the occurrence of a blood-stained emission; he was then found to have an enlarged prostate, and upon catheterization was found to have residual urine. From this time there was increasing trouble in micturition, hesitation, and frequent desire. In September, 1921, he had acute retention, requiring catheterization. Frequency gradually increased to two-hourly day and night, and a month ago his doctor found his urine very purulent and his bladder considerably distended. He had no backache, but marked thirst and loss of appetite. Had never seen blood in his urine.

On examination: Thin, with flushed cheeks. Arteries thickened; blood-pressure 170. Tongue dry and brownish. Urine passed turbid with pus, alkaline, specific gravity 1004 and albumin present. Bladder remained distended to within 1 in. of the umbilicus after micturition. Neither kidney was palpable. *Per rectum*: Prostate found enlarged, flat, smooth, elastic, and readily movable. Blood urea, 72 mg. per 100 c.c.

On December 7, 1922, a Pezzer tube was passed into the bladder above the pubes under local anæsthesia. On the following day the urea concentration test only showed a percentage urea of 1.5 and 1.6 in the second and third hours.

On December 17 blood-urea had fallen to 38 mg. per 100 c.c. and urine-urea was concentrated to 2 per cent. and 2.2 per cent. in the second and third hours.

Operation, December 19, 1922: Prostatectomy. Enucleation easily carried out except in the upper and posterior part, where the gland was adherent. Prostate gland was of moderate size, showing many adenomatous, rounded masses, but was lacerated on the posterior aspect. Dr. Fry reported that the glandular structure showed irregular invasion of the surrounding tissue, with loss of basement membrane and irregular mitoses, indicating adenocarcinoma.

Except for some trouble from hæmorrhage on the fifth day, the patient did well, and was healed and passing urine naturally on January 5, 1923. On February 14 he looked well, and the tongue was clear and moist. A catheter was passed easily. *Per rectum* the prostatic area was soft, but there was some thickening in the left pelvic lateral space suggesting commencing infiltration of the lymphatic area.

Case III.—F. B., aged 62, had had trouble with frequent micturition and slow forceless stream for four years, gradually increasing until three weeks ago, when he had acute retention, for which a catheter was passed. Again retention nine days ago, and since then a catheter has been passed twice a day; he only passes a few drops of urine *per urethram* when straining at stool. No hæmaturia; no backache nor thirst.

On examination: Thin, hard-working man. Arteries thickened. Tongue moist and clean. Neither kidney nor liver felt. Bladder distended to 2 in. above pubes. Catheter passed and 20 oz. of urine drawn off, hazy, acid, specific gravity 1016, albumin present. *Per rectum*: prostate found enlarged, smooth, elastic, but firm; left side larger than right. Prostate gland movable and could be felt bimanually.

Operation, June 28, 1921: Left lobe of prostate very easily enucleated, but right lobe somewhat adherent in lateral aspect. On subsequent examination this surface found to be ragged, though the whole appeared to be the usual soft so-called adenomatous enlargement. Microscopic examination by Dr. Eastes showed, however, active adenocarcinoma, consisting of polyhedral cells arranged in primitive glandular form.

Patient did well after operation, and the wound closed in twenty days. During convalescence a tube of radium was passed into the prostatic cavity and allowed to remain *in situ* for four hours on alternate days, for six applications. Six months later patient was passing urine with good force, but the frequency had increased to three-hourly by day and twice at night. No straining. Urine clear and free from albumin. Patient complained of aching in right sacro-iliac and right sciatic areas. Glands could be felt to be enlarged in right iliac fossa above Poupart's ligament, and on rectal examination there was found to be a hard, nodular infiltration in the right lateral pelvic lymphatic area. The patient lost weight rapidly, though he had no further difficulty in micturition, and died in February, 1922, eight months after operation.

Case IV.—S. B., aged 78. Seen April 15, 1922. Had had increased frequency of micturition for about five years, chiefly during day, with feeble, forceless stream. No pain. On April 13 had acute retention, and his doctor could only pass a No. 6 catheter with difficulty. The next day, the retention remaining, no catheter could be passed, and he was in considerable pain from distension of the bladder, with overflow dribbling of urine.

On examination his tongue was dry and glazed. He had bilateral backache and marked thirst. The bladder was very distended and tense. *Per rectum*, the prostate was found to be enlarged, soft, uniform in consistence and movable. Attempts at catheterization failed, a stricture and false passage being present in the bulbous urethra. A small tube was passed into the bladder above the pubes and the urine slowly drained off. The urine was blood-stained, specific gravity 1012; it contained albumin but no sugar.

Ten days later his tongue was moist and clean, thirst had gone, and the urine was clear. The blood urea was 41 mg. per 100 c.c., but there was no previous test for comparison.

Under an anæsthetic the stricture was divided and the suprapubic incision was enlarged. The prostate was projecting into the bladder in bilateral form, and was easily and rapidly enucleated. The gland showed the usual rounded fibro-adenomatous masses, but it was noted that the right lobe was more dense than the left, and was slightly reddened. Pathological examination by Dr. Eastes showed that there was undoubted carcinoma present in a small area in the centre of the right lobe. The patient made an excellent recovery from the operation, but died subsequently from diabetic coma.

I would like to here mention another case (for the notes of which I am indebted to my colleague Mr. Percival Cole), though I do not include it in my series.

Case V.—J. C., aged 75. This patient was operated upon in 1917 for an apparently simple prostatic enlargement; he had had prostatic symptoms for three years and for three weeks previous to operation complete retention requiring catheterization. The prostate was enlarged, smooth and elastic and was looked upon as adenomatous. It was easily enucleated and the pathological report returned stated that it was adenomatous, though it does not appear that any microscopic section was made.

In February, 1923, he was again admitted to hospital owing to difficulty in

micturition. Frequency was increased to every half-hour during the day and to four or five times at night with great straining. No catheter could be passed; upon each attempted passage the catheter was arrested in the prostatic urethra. Under spinal anæsthesia a filiform bougie was passed with difficulty and the firm tissue in the prostatic area was incised with an internal urethrotomy knife. This was followed by rigors and hyperpyrexia, sweating and death after nineteen hours. At the post mortem the whole of the prostatic area was found to be occupied by firm, hard tissue which proved on microscopic examination to be spheroidal-celled carcinoma. There was glandular infiltration in the iliac and lumbar glands, but no evidence was found of visceral metastases.

This case is very similar to the case reported by Mr. Thompson at the last pathological meeting of this Section.

An analysis of the records of these cases shows that they present the usual train of symptoms, spread over some years, of gradually increasing difficulty and frequency of micturition associated with the ordinary benign prostatic enlargement, and that clinical examination of the prostate by digital palpation *per rectum* failed to convey any suspicion of malignant disease. It was only at the operation for removal of the gland that any difference from the usual simple enucleation was found, when subsequent microscopic examination revealed the presence of carcinomatous infiltration in the area in which some fixation of the gland to the surrounding "capsule" had occurred. In the first three cases the operation was followed by carcinomatous infiltration in the lateral pelvic lymphatic space which was certainly not present before it. It is a noteworthy fact, however, that micturition could be performed freely even when this recurrence was well established. In one case the enucleation presented no difficulty from fixation, and subsequent microscopical examination revealed a small central focus of malignant disease in one lateral lobe, this finding being even a greater surprise than in the cases in which some capsular adhesion had been found to have taken place. My opinion is that this condition occurs more frequently than would be thought and it is an indication that every gland removed at operation must be submitted to a close microscopical examination before it can be pronounced wholly innocent. In each case the average length of history given by the patient before seeking advice was about four years, so that the progress of each case cannot be looked upon as any more rapid than the ordinary benign prostatic enlargement; neither pain nor hæmaturia had been a feature in any of the cases.

I must confess that in many cases of prostatectomy, the macroscopic appearance of the gland removed has not given rise to any suggestion of malignancy, so that in a large number of my cases no critical pathological examination has been made. Since, however, I have become aware of the possibility of unsuspected malignant disease being present, I have had each gland examined and I find that these four cases form four in a total of twenty-eight consecutive prostatectomies, or roughly 14 per cent. Owing to my failure to have the glands examined in all cases previous to this series, figures from my former series of cases would not be reliable. I have, however, enlisted the services of Dr. Eastes and Dr. Fletcher and have their permission to make use of the following figures and I take this opportunity of thanking them for their trouble in obtaining them for me. In a series of 678 prostates removed by operation and submitted to them at the Laboratories of Pathology and Public Health for pathological examination during the last ten years, no less than 174 or 25·7 per cent. were definitely malignant, whilst they classify 58 others, or 8·6 per cent., as border-line or precancerous; 443, or

65·3 per cent. were innocent (adenomatous), and three, or 0·4 per cent., were tuberculous. Dr. Fletcher adds the following criticism of these figures :—

“I think the findings, however, must be discounted to some extent by the fact that many surgeons send for examination only those prostates about which they themselves have doubt, and destroy the others. On the other hand, in the vast majority of these prostates, only certain parts have been cut and not complete cross sections of both lobes—in fact in many cases I only received a piece of the organ. This would militate against the other factor, but I think not to the same extent and I consider that the percentage of malignant prostates given by these figures is higher than it would be if all prostates removed were examined. Those classified as precancerous are border-line cases about which pathologists might well differ and I should think most are innocent.”

TABLE SHOWING RESULT OF EXAMINATION OF PROSTATES.

						Percentage
Definitely malignant	174	25·7
Borderline cases (precancerous)	58	8·6
Innocent (adenoma)	443	65·3
Tuberculous	3	0·4
Total					678	

These figures and Dr. Fletcher's remarks upon them are very valuable, representing as they do the microscopic findings in prostates removed by operation in the practice of a large number of surgeons; but, as he justly remarks, the incidence of carcinoma in 25·7 per cent. is probably higher than the true proportion. I think, however, that we may fairly assume that the operation for the removal of the gland in the very great majority of these cases was performed under the diagnosis of a simple enlargement of the organ, though I know that I for one have not sent him many prostates that I have removed because I thought the naked-eye appearance was conclusive that the tumour was innocent—an error now rectified.

In 1914 Wade, of Edinburgh, found that in a series of 134 specimens of enlarged prostates, fourteen were carcinomatous.¹ In ten out of these fourteen cases, the prostates were removed by operation and their condition was found to be similar to that found in the cases I have described, that is, they were removed under the assumption that they were affected with benign tumours and were only found to be carcinomatous on subsequent microscopic examination. Wade states that in these cases the carcinoma commenced in the centre of an area of chronic lobular prostatitis (by which term he describes the common adenomatous enlargement), and he remarks that in this fortunate circumstance enucleation is at first easy. I would remark that in three out of four of my cases the malignant disease appeared to commence in the periphery of the gland and in only one in the centre. Wade further emphasizes the necessity of obtaining complete sections of the whole gland before an opinion can be expressed as to the existence of early carcinoma. His opinion is that the malignant change occurs in a gland that has already undergone enlargement by innocent “hypertrophy.”

Wilson and McGrath,² in 1911, noted that carcinoma may be found in a prostate removed by operation for apparent benign enlargement, and they remark that the malignant focus appears to commence in an area of prostatic enlargement.

¹ *Ann. Surg.*, 1914, lix, p. 331.

² *Journ. Amer. Med. Assoc.*, 1911, lvii, pp. 1601-6.

At the meeting of the International Urological Congress held in London in 1911, a long discussion took place on the "Ultimate Result of Prostatectomy."¹ In the report of the various papers there submitted, I am unable to find any definite record of the finding of carcinoma in the apparently benign enlargement. Hugh Young, in a series of 450 cases upon which he operated, gives no record of any found to be malignant, but quotes some cases in which, as the result of operation, the urinary obstruction was not completely removed or else it partially returned. In most of these cases there was improvement after second operations, but in some there may have been unsuspected carcinoma. Proust, Zuckerkandl, Thomson-Walker and Cuthbert Wallace, in their contributions to the same discussion made no reference to malignant disease. Fullerton, of Belfast, mentions two cases in which the firmness of the gland on rectal palpation gave rise to a suspicion of malignant disease and in both the microscopic examination proved this to be present, but neither could be called cases of unsuspected carcinoma. Marion, Casper and Pasteau also do not refer to it.

A serious note of the condition is brought out by our President (Sir John Thomson-Walker) in an address on "Some Problems of Prostatectomy" delivered before the Harveian Society in November, 1922.² In discussing the advisability of operation in early cases, he gives as a reason for this that malignant changes may occur in a prostate the seat of simple benign enlargement. He states that it may occur at the periphery of one lobe as a localized firm, inelastic area, which at operation strips with difficulty, or that it may occur in the centre of a lobe that has shelled out readily at the operation. In a series of 100 consecutive cases of prostatectomy for apparent simple enlargement by Sir John Thomson-Walker, no less than sixteen were found by the pathologist to show areas of carcinomatous change. In eleven of these sixteen cases there was nothing in the clinical examination to suggest malignancy in any form; in four there was a small firm area on one side and in one the prostate was somewhat firm in consistence. These figures are of the utmost importance and if the combined figures reported by Wade, by Sir John, and those above reported by myself, be added together it gives a total of thirty-four cases of unsuspected carcinoma out of 262 cases in prostates removed by operation for apparent benign enlargement in three series of consecutive cases reported without selection—or a percentage rate of nearly 13'0. This is a sufficiently large proportion to be borne in mind when discussing with a patient the necessity for early operation and in outlining the prognosis after operation in these cases.

In the course of the foregoing remarks, I have tried to refrain from any discussion on the true pathology of the enlarged prostate and I only introduce it at this stage in order to institute a certain analogy with the types of carcinoma seen in other organs of the body. The senile enlargement of the prostate has been termed by different pathologists "adenoma," "cystic adenoma," "simple hyperplasia," "chronic lobular prostatitis" and "interstitial prostatitis," and as far as I can gather they have not yet made up their minds as to which class it is to belong. The pathologists are not the only persons in doubt about the prostate, for surgeons still seem to be divided in their opinion as to whether the operation of prostatectomy is to be regarded as a complete enucleation of the whole gland or as the removal of adenomatous

¹ *Ann. des mal. des organes gén. urin.*, 1911, xxix (ii), pp. 2146, 2221.

² *Lancet*, 1922, ii, p. 1121.

masses from a false capsule formed of compressed prostatic tissue. I do not venture to express any decided opinion on these vexed questions, which might in turn, in these days of prostatic surgery, change other opinions forcibly expressed in the earlier days; my motive this evening is rather to direct your attention to the comparative frequency with which carcinoma may exist as a small undetected focus in a prostate the seat of senile enlargement. The analogy which I wish to point out is that between these cases and those of carcinoma in other glandular structures in the body. In the case of the breast, of the tongue, of the stomach and of the intestine, as well as in that of other organs, the opinion has been expressed that carcinoma may occur in areas which have been the seat of simple chronic inflammation or of ulceration. May not the same sequence occur in the prostate and thus carcinoma commence as a focus in a gland already the seat of the benign enlargement?

DISCUSSION.

Dr. C. G. CUMSTON (Geneva) said that in his experience he had found the percentage of malignant foci in apparently simple prostatic hypertrophies to be practically the same as that arrived at by Mr. Swan. He (Dr. Cumston) had for many years taught that the adenomatous prostate was in every way, clinically, closely similar to the adenomatous breast in the female, a neoplastic process which, sooner or later, underwent malignant transformation, and this applied to adenoma of the prostate. These central or peripheral malignant foci, while still small, were impossible to diagnose clinically, and it was only by careful and complete histological sections of the gland removed that they could be discovered. Text-books were misleading in stating that cancer of the prostate could be diagnosed when the gland was large, hard and nodular. When the morbid process had attained the nodular phase the growth would probably have become inoperable or at least would have involved the periprostatic lymph nodes, so that recurrence resulted in a very brief lapse of time following enucleation. Hence every prostate removed should be submitted for histological study of the *entire* gland.

Mr. FRANK KIDD said that Mr. Jocelyn Swan had done well in bringing up this matter for discussion. The moment was timely, as this kind of case was becoming more common. Before the war he (Mr. Kidd) was accustomed to have sections cut of the prostates removed from his hospital cases. Almost invariably the report came back "fibro-myo-adenoma." For that reason he had been inclined to discount the figures given by Albarran and Young of the incidence of microscopic carcinoma occurring in prostates removed as being simple, namely, 14 to 20 per cent. He (Mr. Kidd) supposed that the incidence was higher in France and America than it was in England. Since the war, and especially during the last two years, he had been very much impressed with the increasing number of reports that came back on prostates—which he had removed as cases of enlargement of a simple nature—labelled "precancerous" or "showing evidence of cancerous change in part of the gland." This he had put down to the worry and under-feeding of the war as he believed that worry was one of the most important predisposing causes of cancer. So much impressed had he been with this pathological finding that for some time past he had used it as an argument for early operation in the cases of patients coming to consult him. Whereas previously in an early case one had been inclined to leave it to the patient to decide on operation after the matter had been fairly put to him, now he felt it to be his duty to impress the patient with the fact that an early operation might be saving him from the onset of cancerous change in what appeared to be a simple prostate. It was a very important additional argument in favour of early operation. He thought there was now sufficient evidence to enable them to say that a so-called simple

adenoma could in the course of years develop a focus of malignant change which would eventually spread in the usual manner of an ordinary carcinoma of the prostate. He based this evidence largely on the length of the history. The ordinary straightforward carcinoma had usually a very short history, all the symptoms appeared within nine months or a year, and they all came on more rapidly and more intensively than those in simple enlargement of the prostate. Whereas in those cases Mr. Swan was discussing there was very often a history of prostatic symptoms lasting over four or five or more years. Again, some of these cases had been seen and examined years before and a simple hypertrophy found, but years later the patients had turned up and had been operated upon; and then malignant change had been found in what had been taken to be a simple gland. Was it possible for a surgeon to train himself to detect these carcinomatous changes in a simple adenomatous prostate? If once this possibility were borne in mind, he would, no doubt, become more expert. The point upon which he (Mr. Kidd) would lay most stress was that these prostates were "flat" rather than hard. For many years he had taught the importance of considering the "flatness" when palpating a prostate gland. "Flatness" was the best word to express what was felt, as opposed to the rounded undulating projection presented by a simple prostate on its rectal surface. Diagnosis was of the utmost importance because it was possible, if one could make the diagnosis previous to operation, to carry out Young's total perineal prostatectomy rather than an ordinary suprapubic enucleation.

Only a few months ago he (Mr. Kidd) had made up his mind in dealing with what was apparently a case of simple adenoma of the prostate that there was a small nodule of growth buried in one of the lobes. For that reason he had advised Young's total prostatectomy and had carried it out successfully. Though the specimen appeared to the naked eye to be adenomatous, yet on section he had been relieved to find that there was definite evidence of early carcinomatous change. He had now done a fair number of these operations for early carcinoma of the prostate and could recommend it as giving most satisfactory results. It was surprising to find how little shock there was in carrying out this operation and how well the patients behaved during their convalescence. Young's operation deserved a wider trial in this country.

On the other hand, if it was not possible to make a diagnosis of carcinoma before operating, and if one had enucleated such a gland and had found that the microscopic report was "carcinoma," there was still much that could be done. The question had been asked: "After all was there anything to be done when this discovery was made?" Personally he (Mr. Kidd) always advised these patients to submit to some form of radio-therapy. In his opinion the best results could be obtained by giving the patients an exposure with the Erlangen X-rays, though other forms of radio-therapy might be tried. Was it reasonable to suppose that after the main mass of cancer had been removed by operation, radio-therapy would be able to deal with the small amount of cancer cells left behind in the tissues? At any rate it seemed more reasonable to expect it to do this than to expect it to destroy the whole mass of a cancer not submitted to a previous operation.

Another point that needed the greatest care in considering the diagnosis of the nature of a prostatic enlargement was that of paying close attention to the posterior portion of the base of the prostate and to the portion of tissues just above that which lay between the vesicles. These were the spots at which carcinoma was most likely either to start or to spread, though it could arise in other portions of the gland. The point was well established that prostates removed at operation should be carefully cut and examined by a pathologist. Nevertheless in his experience it was not always possible to base the prognosis on the pathological report. Some cases reported as malignant did well, and never gave any further trouble, whereas other cases reported as non-malignant developed metastases in a few months, particularly in the bones, and the result was disappointing. He recalled three cases operated upon in the same month in 1919, of which the following were brief records:—

Case I.—In this case he had been uncertain whether he was dealing with a fibrous, malignant, or simple prostate. Enucleation proved extremely difficult, and he felt certain that malignant

disease was present, yet the pathologist reported "chronic fibrous prostatitis, certainly no evidence of malignant disease." Six months later that patient had developed a single secondary deposit at the top of the vertebral column and his head fell forward and he died in a moment.

Case II.—The patient's prostate had been adenomatous for some years. It was removed without the slightest suspicion that it was malignant. On cutting it through after the operation he (Mr. Kidd) had noted certain areas that looked to him a little suspicious. Sections confirmed the suspected malignancy of these areas, yet they had been completely encapsulated in large areas of simple adenoma and it would certainly have been thought that all malignant tissue had been removed. In three months this patient had developed large bony deposits of malignant disease and he died within six months.

Case III.—Here the patient had a prostate that was obviously malignant on one side, yet for certain reasons it was thought advisable to attempt enucleation rather than undertake Young's perineal operation. Enucleation was carried out satisfactorily, and ever since that time the patient had had no difficulty whatever in passing water. Nevertheless, from time to time he developed large malignant nodules in the bones of his spine and in the ribs and even in the lungs; he became very ill for some weeks, and was almost at death's door, then he suddenly recovered and all the deposits disappeared.

He recorded those cases in order to show how impossible it was to state a certain prognosis in any given case. If an attempt was made to set a time limit the only certainty was that one would be wrong. In this connexion it was important to remember the extraordinary latency of carcinoma. For instance, he (Mr. Kidd) recalled a case in which Sir Frederick Treves had simply removed the breast and had left the pectoral muscles in 1895 for proved carcinoma of the breast. The patient had remained quite well until 1912, then she developed secondary deposits in the axillary glands on the same side. He (Mr. Kidd) had carried out a complete Sampson-Handley operation as if the breast were still present. The glands had proved to be malignant, and the deposits must have been there seventeen years. The patient had since remained perfectly well for another eleven years.

The following principles, therefore, should be accepted in dealing with carcinoma: (1) In many cases cancer cells lay latent in the tissues for many years, and only lighted up under stress of worry, over-work and exhaustion. (2) Spontaneous healing of secondary malignant deposits did occur, and was perhaps not so uncommon as was often thought. For instance, he had opened a man's abdomen and had found extensive malignant disease of the stomach and peritoneum. Sections were cut. Within four months all signs of trouble had disappeared, and the man had remained perfectly well.

He would relate one very curious case that bore on the diagnosis of malignant prostate:—

Mr. Hurry Fenwick had cystoscoped a patient and could see what appeared to be a malignant ulcer about the size of a half-crown lying near the base of the bladder. Mr. Fenwick sent the patient to the London Hospital with a request to him (Mr. Kidd) to carry out permanent drainage. Upon his opening the bladder he found the ulcer exactly as described and it was lying on the top of the left lobe of the prostate. The edges did not feel thickened or malignant and he could distinctly feel there was a large adenomatous prostate present. He therefore enucleated the prostate, the specimen carrying out the ulcer with it. The ulcer proved to be a simple one and the prostate a simple one. It was therefore possible for a simple prostate to ulcerate into the bladder, though he considered this a rare phenomenon.

Mr. E. T. C. MILLIGAN said that two cases of the condition the subject of Mr. Jocelyn Swan's paper had recently come under his care. In one the prostate at the time of enucleation had been found to be adherent at one part to the capsule. Recurrence of growth was rapid, and a sound was only passed with great difficulty into the bladder on the twelfth day after operation. Post-operative obstruction continued. The man was fitted with a permanent suprapubic drainage appliance, and only lived a few months. This was a contrast to the cases recorded that night, some of which had lived for years. The prognosis after operation was difficult, and there was an analogy in cancer of the rectum in which the same difficulty arose: a small recent growth easily, and apparently completely, removed, recurring with great rapidity, whereas clinically unfavourable growths sometimes did not recur for years. From the point of view of

prognosis, there were these two definite classes, but there was no recognizable pathological or clinical feature by which to assign a given growth to the rapid or slow recurring class. In his second case the growth of the prostate had been found on removal to be adherent to the urethra, so much so that 1 in. of the urethra was removed with the prostate, and it was this adherence that aroused the suspicion of malignancy, although clinically the signs were those of a benign enlargement.

Section of Urology.

President—Sir JOHN THOMSON-WALKER, F.R.C.S.

Two Points in connexion with Chronic Nephritis.

By CHARLES GREENE CUMSTON, M.D.

(Geneva, Switzerland.)

THE two points in connexion with advanced cases of chronic nephritis to which I desire to call your attention are: (1) The total absence of albuminuria in a fair percentage of these cases; (2) the value of Ambard's constant for ascertaining the real condition of the functional activity of the renal parenchyma, and indirectly, the extent of pathological change present in the kidneys.

In non-azotæmic patients one frequently meets with a permanent increase of the constant and, as Ambard has shown, nephritides presenting a functional renal inferiority of 80 per cent. may occasionally remain unrecognized. Although the constant may not give any indication as to the exact duration of the renal morbid process, it is at least an excellent index as far as the intensity of the nephritis is concerned.

Ambard's comparative tables reveal the fact that a constant of 0·07 corresponds to normal renal functioning, while a constant of 0·08 indicates a 24 per cent. loss of renal activity, a constant of 0·09 to 40 per cent.; 0·10 to 51 per cent.; 0·11 to 60 per cent.; 0·12 to 66 per cent.; 0·13 to 70 per cent., and 0·14 to 75 per cent.

FORMULA OF AMBARD'S CONSTANT.

$$\frac{\text{Ur}}{\sqrt{\frac{\text{D. 70.}}{\text{p. 5}} \cdot \sqrt{\text{C}}}} = K = \text{Constant} = 0\cdot06 - 0\cdot08.$$

Ur = Blood urea per 1,000 c.c.

D = Twenty-four-hour total of urea in the urine.

C = Urea concentration in urine in grammes per 1,000 c.c.

p = Body-weight of patient, calculated according to an average weight of 70 kgr.

In the cases studied—fifteen in all—the constant was estimated several times at intervals of a week or ten days, in order to avoid erroneous conclusions arising from a possible acute outburst of the chronic renal process, with the result that the constant was invariably found above normal, varying from 0·09 to 0·12.

In conjunction with the estimate of the constant the phenol-sulphone-phthalein test was used. I am not, however, averse to the methylene blue test and resort to it frequently.

82 Cumston: *Two Points in connexion with Chronic Nephritis*

The following brief outline of two case histories will serve to illustrate the object of this short note:—

Case I.—Patient, aged 56. Prostatic hypertrophy. Nothing worthy of note in the pathological antecedents until five years before coming under observation he began to suffer from constant severe headache. During the past eighteen months has had occasional attacks of vertigo, also cramps in the calves of the legs and some nausea. Slight exertion dyspnoea. Somnolence after meals.

Examination: Fairly corpulent man with very ruddy complexion. Liver small, spleen can be percussed. Heart and lungs normal. Blood-pressure (Pachon), Mx 20, MN 10. Polyuria, 1,700 c.c. to 2,100 c.c. No sugar, no albumin by fractional analysis of urine. Very rare granular casts. Urea of blood: 0.57 gr., K 0.11. Phthalein test, 48 per cent. Since the loss of functional activity of the renal parenchyma was thus estimated at 60 per cent. the patient was regarded as a bad operative risk and prostaticectomy was refused. The patient died eleven months later from uræmia.

Case II.—Patient, aged 62. Prostatic hypertrophy. Early in 1918 had a slight ictus lasting three days, followed by paresis of right arm. At this time and ever since has had some exertion dyspnoea, occasionally nocturnal as well. Constant tinnitus. Congestion of the face and occasionally some painful palpitations.

Examination, November 8, 1920: No paralysis. Pulse very hard at 86. Blood-pressure (Pachon), Mx 21, MN 11. Clanging second aortic sound. Lungs normal. Polyuria, 2,000 c.c. to 3,000 c.c. No sugar, no albumin by fractional analysis. No casts. Blood urea, 0.54 gr.; K 10.099. Phthalein test, 34 per cent.

The loss of functional activity being thus estimated at about 49 per cent., prostaticectomy was done several weeks later after medical treatment, which reduced the blood-pressure and polyuria and increased the phthalein elimination to 52 per cent. In normal subjects the elimination varies between 56 per cent. and 68 per cent. in one hour and ten minutes, hence in this patient the operative risk was not too great. Recovery slow, but without untoward complications.

In practice it is not uncommon to encounter certain types of patients whose clinical history offers very different symptoms, but symptoms which one should be able, after a careful clinical study of the case, to attribute to their true cause, namely, chronic nephritis. These subjects usually are more or less plethoric and look in perfect health.

At times these patients complain of headache, which is increased by the recumbent position and is uninfluenced by the use of sedatives. At other times visual or auditory disturbances are marked, and very frequently attacks of vertigo of the Ménière type occur. The phenomenon of the numb finger is common—an important minor symptom—likewise occasional paroxysms of painful palpitations. But what is particularly striking is the high blood-pressure, the Mx sometimes attaining 20 to 22, MN remaining normal or nearly so, and this should call our attention to the kidney.

Fairly frequently there is some polyuria and pollakiuria, but repeated analyses of the twenty-four-hour urine or fractional analysis will fail to reveal the slightest trace of albumin. Dieulafoy maintained that albuminuria is not a constant symptom of chronic uræmigenous nephritides, and Weill, out of a total of 124 cases of azotæmia, found albumin absent in ten, otherwise 8 per cent. of the cases.

Other clinicians have made the same remark. For example, Dr. Thomas D. Lister in his book, "Medical Examination for Life Insurance," London, 1921, p. 61, says that "a history of nephritis, even if the urine be found free from albumin, may have left after-effects. A high blood-pressure or a hypertrophied heart may be found." This statement is sound, but, unfortunately, it would

appear that Dr. Lister relies on Esbach's picric-citric acid test for detecting albumin. Unquestionably, this test is excellent for routine work, but it will not reveal minute traces of albumin, hence it should be discarded where absolute precision is required.

And again, where exactitude in the measurement of the blood-pressure is essential, I would say that in my opinion, there are only two really reliable instruments on the market, namely, Pachon's and Vaquez's sphygmometers. An instrument giving only the MX pressure is valueless, as the true condition of the blood-pressure can only be determined by both the MX and MN. One should always specify which of the instruments has been employed for taking the pressure. Whereas Vaquez's and Pachon's sphygmometers each give the normal MN, varying between 7 and 10, the normal MX of Vaquez is 13, that of Pachon, 17.

My opinion is that analysis of the total twenty-four-hour urine often may be misleading and that small amounts of albumin voided at some time during the day may be undetected; therefore, fractional analysis has been carried out in all my cases in order to be able to affirm that no trace of albumin was at any time present. It is a well known fact that during the evolution of chronic uræmigenous nephritis an intermittent albuminuria may develop from various causes—cyclical, digestive or orthostatic albuminurias—so that its presence should be searched for by the fractional method, as follows:—

The patient is given two meals, one at noon, the second at 7 p.m. consist of a mixed diet. The patient voids his urine at 7 a.m., and this urine is thrown away. Five sterile glass jars are used for collecting the twenty-four-hour urine. The first collects the urine voided from 7 a.m. to noon; the second from noon to 4 p.m.; the third from 4 p.m. to 7 p.m.; the fourth from 7 p.m. to 10 p.m., while the fifth collects from 10 p.m. to 7 a.m. inclusive.

By this fractional analysis we have found that very frequently chronic uræmigenous nephritides evolve without ever presenting the slightest trace of albumin, and that the number of cases of this clinical type is much higher than is generally suspected. I look upon this as a most important notion, because the absence of albumin should not lead one to eliminate the diagnosis of a renal lesion, especially at the onset of the evolution of nephritis—a phase during which the symptoms presented by the patient give no distinct clue to the diagnosis, because this phase being that of compensation, does not give rise to any pathognomonic sign.

The examination is completed by estimating the percentage of urea in the blood, and the phenol-sulphone-phthalein elimination. A comparison of these three diagnostic procedures will enable us to affirm that the case is one of chronic uræmigenous nephritis—perhaps very advanced—evolving without albuminuria.

0.5 X 0.1 10 X 1.0

In surgical work many patients give no history of any serious medical affection. They have always been well, and no serious disease or chronic infectious process will be found in their antecedents. The classical analysis of the urine is made and no trace of albumin found, with the result that the functional activity of the kidneys is assumed to be intact.

No one more than the surgeon has need of detecting latent uræmia, a condition that can only be discovered by estimating the percentage of urea in the blood, the use of Ambard's ureo-secretory constant and the time elimination of phthalein or methylene blue. If these tests be carried out the most serious surgical catastrophes can be avoided.

In conclusion, I would say that it is essential to submit all surgical cases

84 Kidd: *Candle removed from the Bladder of a Male*

to thorough clinical examination when the patient's condition does not require urgent interference; and that the absence of albumin from the urine is no proof of renal integrity.

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Candle removed from the Bladder of a Male.

Specimen shown by FRANK KIDD, M.Ch.

PATIENT, a male, aged 21, of foreign extraction, was brought to me on June 2, 1923, with a story that for the last three months he had noticed greatly increased frequency of micturition, accompanied by severe pain and hæmaturia. He had been cystoscoped in a provincial town, and told he had a growth which must be cut without a moment's delay. He came up to town to take a further opinion before deciding on such a serious step.

On examination I found that his urine contained a large quantity of pus. A catheter was passed and the bladder washed clear, but it would only hold 4 oz. of lotion. On inspecting the interior of the bladder with the cystoscope I could see a stone of moderate size lying on the floor of the bladder and evidence of a moderate cystitis. There was no sign of growth or tuberculosis. I took an X-ray picture, which I exhibit. This displays the stone, the centre of which is translucent and surrounded by an outer ring of opaque substance. I expressed the opinion that the shadow was a very peculiar one and advised litholapaxy, which was performed on June 5. On catching the stone I found that it crumbled in a very curious manner, which puzzled me, and I experienced unusual difficulty in sucking the fragments out of the bladder. Finally I got the bladder empty of fragments and confirmed this by cystoscopic examination. Upon my examining the fragments I noticed that they consisted largely of a wax-like substance looking like candle grease and that there was only a small quantity of chalky or gritty matter, which consisted of calcium phosphate.

The next day I challenged the patient and asked him to account for the presence of candle grease in his bladder. He then informed me that he placed it there on the advice of a friend and had since not told anyone about it. He had suffered from frequent nocturnal emissions. His friend told him that an unfailing cure was to melt candle grease, work it up into a rod shape and insert it into the urethra. The result was not so happy as he had confidently expected.

When he was last heard of his cystitis had cleared up and he felt quite well again.

Pyelogram illustrating the Breaking of Two Shadows into Multiple Shadows as the Result of Injection of Sodium Bromide.

By W. GIRLING BALL, F.R.C.S.

J. H., FEMALE, aged 26, post office clerk, was admitted to St. Bartholomew's Hospital on April 30, 1923, with a history of dragging pain in the left loin, which had been persistently present for nine months. She had not had any attacks of renal colic or other symptoms relative to the urinary tract. The only other symptom was an increase in the pain after vigorous exercise, usually relieved by resting.

The girl was healthy in appearance and presented no obvious physical signs to account for her symptoms. The urine was natural. She pointed to the left lumbar region as the site of her pain. The X-ray appearance demonstrated the presence of two shadows which I thought might be tuberculous glands.

A pyelography was then carried out, which demonstrated that the renal pelvis was situated in the site of these shadows and that there was a degree of hydronephrosis, as the pelvis held some 30 c.c. of sodium bromide. The interest of the investigation, however, lies in the fact that a picture taken the next day showed that the two shadows had been broken up, demonstrating that calculi were present. A hydronephrotic kidney with eighteen small stones composed of a mixture of calcium oxalate, calcium phosphate, ammonium magnesium phosphate and uric acid were found.

Case of Malignant Growth of the Renal Pelvis, with Calculi.

By Sir JOHN THOMSON-WALKER, F.R.C.S.

J. N., MALE, aged 63. Examined May 19, 1915. Had suffered from renal colic as a young man and a swelling had been noticed in the right side. Twelve years ago he fractured his femur and since that time the attacks of renal colic had ceased. For some years there had been slight attacks and three months ago a severe attack of hæmaturia. For three weeks he had been passing gravel with a recurrence of the pain. If he lies on the left there is dragging pain in the right side. There had been some loss of weight.

On examination there was found to be dragging pain in the right loin. There was increased frequency of micturition to two hours during the day and once or twice at night. A large mass was found in the right flank, bulging laterally and obliterating the waist line. From the main mass a rounded mass projected anteriorly and about reached the level of the umbilicus. The main mass was dull on percussion and the second mass had a half-tympanitic note.

An X-ray examination showed two rounded opacities in the right flank and a smaller opacity at a lower level. The right kidney shadow was increased in size but the outline was indefinite.

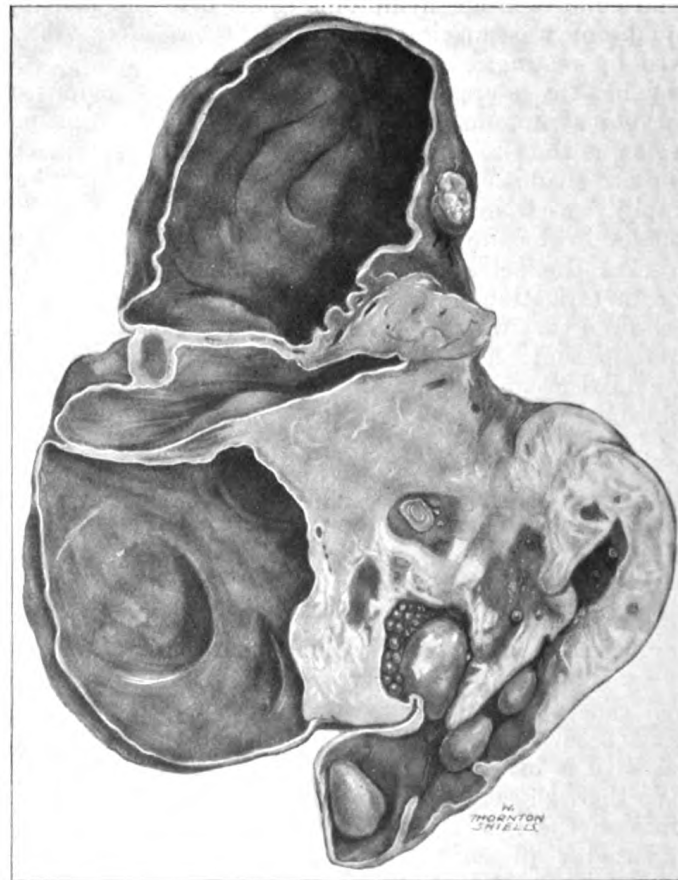
Cystoscopy showed a copious efflux of dark blood from the right ureter.

The urine drawn from the right ureter had the following characters: Acid, 1022; semi-solid on boiling; urea, 1.25 per cent., contained red blood corpuscles fresh and as "shells"; a large amount of granular blood pigment; a considerable number of pus cells; casts of red blood corpuscles and of blood

pigment and a few epithelial casts; large masses of both small and large epithelial cells; mucous oxalate crystals.

Urine of left kidney: Acid, 1030, contained a small amount of blood; urea, 2.05 per cent. Microscopically there were red blood corpuscles, blood pigment, tailed cells and small round epithelial cells; a few cellular casts and granular casts; no crystals.

Operation, June 9, 1915: Right kidney exposed and found to be a very large thin-walled hydronephrosis containing several pints of dark blood-stained fluid. On removing the sac it came away easily at first but there were dense



Calculi and malignant growth of the renal pelvis.

adhesions to the colon along the front. The lower pole and posterior surface were also densely adherent. On attempting to expose the ureter it was found to be surrounded by a dense hard nodular mass which lay at the inner and lower aspects of the kidney. The vascular pedicle was exposed and tied from above and the kidney and dense hard mass were dissected away from above downwards.

Convalescence was rapid and uninterrupted. In October, however, his temperature began to be raised. On October 16, a little indefinite resistance

could be felt in the right loin. Ten days later a mass the size of a fist could be felt, and this rapidly increased and he lost weight. He died on November 4.

The specimen (*see figure*) shows a large multilocular hydronephrosis of the renal type with two stones the size of date seeds in the narrow lumen of the pelvis. The wall of the renal pelvis is replaced by a large irregular dense mass of tissue. On microscopical examination this shows the histological structure of a carcinoma. The cells are very degenerate for the most part and are irregular in shape. They are arranged in small groups in a fibrous stroma.

Case of Vesico-urethral Calculus.

By Sir JOHN THOMSON-WALKER, F.R.C.S.

C. C., AGED 33, railway porter; admitted February 2, 1923; discharged February 28, 1923. Complaint: Pain in peritoneum and tip of penis at beginning of micturition.

History: Passed a small stone when aged 7. Since then has occasionally passed gravel, and had scalding pain in the urethra during micturition. No serious trouble until Christmas, 1922, when he had considerable difficulty in micturition; much delay, straining and frequent interruptions; relieved by medicine. One week later had retention for six hours relieved by catheter. This was the first instrumentation and the only time any blood was passed. Since Christmas, frequency: day, two hourly; night, once or twice; urgency; occasionally incontinence if he delays to relieve himself; pain in perineum and penis.

Condition on admission: Act of micturition: delay, straining, poor projection, good stream, interrupted; instruments obstructed in prostatic urethra. Bladder: not distended. Perineum: tender, no stone felt. Rectal examination: stony hard mass felt, size of cherry, just above prostatic urethra; very tender on palpation. Urine: turbid with threads, alkaline, 1020, no albumin or sugar. Microscopically, pus cells and phosphates seen. Daily urea: 1·4 per cent. in 49 oz. 295 gr.

X-ray report: Dumb-bell shaped opacity in pelvic view suggestive of vesical calculus with a prolongation downwards into the prostatic urethra.

Operation, February 5, 1923: General anæsthetic; Trendelenburg position; suprapubic lithotomy; mushroom-shaped calculus easily removed with fingers. Bladder mucosa and prostatic urethra remarkably healthy; $\frac{1}{2}$ -in. tube into bladder, small prevesical drain. Uneventful convalescence.

PROCEEDINGS
OF THE
ROYAL SOCIETY OF MEDICINE

EDITED BY
SIR JOHN Y. W. MACALISTER
UNDER THE DIRECTION OF
THE EDITORIAL COMMITTEE

VOLUME THE SIXTEENTH

SESSION 1922-23

WAR SECTION



LONDON
LONGMANS, GREEN & CO., PATERNOSTER ROW
1923

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CONTENTS.

Squadron Leader H. E. WHITTINGHAM, M.B., D.P.H., D.T.M., R.A.F., M.S.	PAGE
Observations on Sand-fly Fever in Malta	1

Surgeon Rear-Admiral WILLIAM BETT, M.V.O., R.N.						
Venerable Disease as a War Casualty	15
" " "	(Corrigendum)		44

Colonel R. C. CAMPBELL, D.S.O.						
Physical Training in the Army	31

Squadron-Leader T. S. RIPPON, R.A.F. Medical Service.
The Effect of Tropical Climate on Physical and Mental Efficiency ... 45

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LONDON :
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OXFORD HOUSE,
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War Section.

President—Sir JOHN GOODWIN, K.C.B., C.M.G., D.S.O., A.M.S.

Observations on Sand-fly Fever in Malta.

By Squadron Leader H. E. WHITTINGHAM, M.B., D.P.H.,
D.T.M., R.A.F., M.S.

IN the spring of 1921 the D.M.S. of the Royal Air Force decided to appoint a Commission to investigate the causation, prevention and treatment of sand-fly fever. This measure was considered necessary owing to the high sickness-rate from that disease which occurred during the previous year amongst the R.A.F. personnel in the Middle East. Malta was chosen as the best locality in which to carry out these investigations; the chief reasons being its isolation, freedom from malaria, and the relatively small chance of encountering epidemic jaundice, relapsing fever, and other tropical fevers—compared with such a country as Mesopotamia.

A fever having a fairly definite clinical picture, and commonly called "sand-fly fever," has been known to affect British forces serving in the Mediterranean since the year 1799 [1] [2]. Not only is the fever of a fairly definite type, but the date of the onset of cases of the fever can be predicted yearly with reasonable accuracy [1] [2] [3] [4] [5] [6]. Still the diagnosis of this fever is far from a simple matter. This may be gathered from a perusal of the excellent summary of the "total continued fevers" of the Mediterranean made by Birt. In his survey of the historical evidence of these fevers he is able to divide most of the cases into the following groups: (1) Typhoid, (2) Malta fever, (3) simple continued fever. The latter heading accounts for most of those milder cases termed "sand-fly fever." It was obvious in the past, before this subdivision of the fevers in Malta, that there was more than one type of fever, but it is hardly to be expected that all mild cases of fever are of necessity true cases of sand-fly fever. Nor are all so-called cases of sand-fly fever simple in their nature [1] [2] [3]. Thus the Army Medical Report of 1823 [7], dealing with Malta [7], states that the symptoms of summer fever (sand-fly fever) during the early part of summer were chiefly severe frontal headaches. Later in the year liver symptoms, resembling yellow fever, were not infrequent. There were three deaths out of 304 cases. Again Marston [8] refers to the varying types of fever with different years, the period of fever varying from three to five or seven days. Relapses occurred in 15 per cent. of the cases. Whereas the Army Medical Report, 1897, divides "Mediterranean or simple continued fever" into two distinct types: (1) With two to three days fever, ending by rapid recovery, (2) with seven to ten days fever, ending by gradual recovery, a glance at Table I will convince the most casual

2 Whittingham: *Observations on Sand-fly Fever in Malta*

observer that the continued fevers of the so-called tropics are very numerous. The differential diagnosis of such fevers must take into consideration the following diseases:—

TABLE I.—COMMONER CONTINUED FEVERS OCCURRING IN THE TROPICS.

(1) PHLEBOTOMUS OR SAND-FLY FEVER.	(10) WEIL'S DISEASE OR EPIDEMIC JAUNDICE.
(2) INFLUENZA.	(11) Trench fever.
(3) DENGUE OR BREAK-BONE FEVER.	(12) Relapsing fevers.
(4) Seven day or Saku fever.	(13) MALTA FEVER.
(5) Rat-bite fever.	(14) Malaria.
(6) Yellow fever.	(15) PNEUMONIA.
(7) Japanese river fever.	(16) SEPSIS.
(8) Spotted fever of the Rocky Mountains.	(17) Trypanosomiasis.
(9) Typhus, Brill's disease or Tarbardilla.	(18) ENTERICA.

The fevers indicated by capitals are those likely to be encountered in Malta.

The work thus resolved itself into:—

(A) *A preliminary investigation* to determine areas in which sand-fly fever was most prevalent.

(B) *A direct investigation* comprising (1) methods of differential diagnosis; (2) attempts to isolate the causal virus; (3) breeding and lethal experiments on the sand-fly (*Phlebotomus papatasi*).

(A) *The preliminary investigation* to determine the areas in which sand-fly fever was prevalent was adopted early in the year as a tentative measure prior to sanction being obtained to investigate the disease in a sand-fly fever district. The only method applicable when most of the work had to be performed in this country was to arrange for the collection of a blood smear from every case of pyrexia occurring overseas. By this means and by the clinical data supplied it was hoped to arrive roughly at the percentage of errors in diagnosis. Moreover, there was the possibility of detecting sand-fly fever and sand-fly-fever-free areas. This latter information would aid materially in the choice of a ground for future research.

(B) *The direct investigation* was carried out at the R.A.F. Station, Calafra, Malta. A nominal roll of the personnel at that station was prepared, showing the term of service out of England, the nature of any previous fever, and the date of T.A.B. inoculation. From this roll it was easy to isolate all those who had not spent a summer overseas before. These men were inspected and had their temperature taken daily. Any who had fever were immediately admitted to the sick quarters. Those with a temperature of 99° F. were instructed to report at the laboratory a few hours later to have their temperature again recorded. In this way cases of fever were diagnosed in their early stage.

(I) METHODS OF DIFFERENTIAL DIAGNOSIS.

Every case admitted to the sick quarters was carefully examined, and only those showing typical symptoms of sand-fly fever were accepted for the investigation. In all, twenty-six cases of apparent sand-fly fever were investigated. To aid in the differential diagnosis all the cases were subjected to a definite routine.

(1) *Blood cultures* were made as soon as possible after admission on the first day of fever in every case. The blood was disposed of as follows:—

- 2 c.c. were inoculated into 1 c.c. of Wenyon-Noguchi medium
- 5 c.c. were inoculated into 30 c.c. of Wenyon-Noguchi medium
- 5 c.c. were inoculated into 30 c.c. of nutrient broth
- 3 to 5 c.c. were inoculated into guinea-pigs intraperitoneally in fifteen cases

(2) *Agglutination tests* were performed with Garrow's agglutinator on the first and tenth day of disease against *Bacillus typhosus*, *Bacillus paratyphosus* A and B, *Bacillus enteritidis* (Gaertner), *Bacillus dysenteriae* (Shiga and Flexner), *Micrococcus melitensis* and *para-melitensis*. It was necessary to test the blood serum for agglutinins on these two days in order to interpret correctly the effects of preventive T.A.B. inoculation.

(3) *Wassermann tests* were collected on the first and tenth day of disease to ascertain whether syphilis was present or not, or whether sand-fly fever had any effect on complement deviation.

(4) *Blood Counts*.—As the degree of anæmia is only slight in sand-fly fever, the estimation of the red blood corpuscles and the percentage of hæmoglobin in the peripheral blood was not adopted as a routine measure. Daily total and differential leucocyte counts were done on twenty-six cases over a period varying from eight to fourteen days. In two cases hourly leucocyte counts were performed for twenty-four hours in order to ascertain the onset of the leucopenia stated to occur in this disease. Four normal control bloods were examined daily.

Technique.—The daily counts were collected at the same hour of the day (9 a.m.), with the same instruments, and by the same observer in each case. The blood was collected from the lobe of the ear. The lobe was rubbed with cotton wool to stimulate the circulation and then punctured with a sharp needle. The first drop of blood was wiped away and then the specimens were collected. The total white counts were mounted three times and the leucocytes on the 400 squares of a Thoma-Zeiss hæmacytometer slide counted with each mounting. An average of the three mountings was accepted as the correct total white count. The smears for differential counts were stained by Leishman's method and a count of 400 cells made in every case; 100 cells were counted along one edge, 200 by zig-zagging through the central portion of the smear and 100 at the other edge of the film. The temperature (oral) of the patient was taken while the blood was being collected. This routine eliminated as far as possible errors of technique.

(5) *Urine examination* was carried out daily on all the cases. Special search was made for albumin, bile and spirochætes.

(6) *A thorough clinical examination* was made and particular attention paid to (a) the accurate recording of temperatures; (b) the conjunctivæ for jaundice and congestion, the skin for bites, rashes and ulceration; (c) spleen and liver for size and tenderness; (d) the lymphatic glands for enlargement or tenderness; (e) the naso-pharynx or air passages for inflammation.

(II) ATTEMPTS TO ISOLATE THE CAUSAL VIRUS.

From previous work done by Birt and others [3] [9] it was proved that the blood of man was infective by direct transmission to other men only on the first day of disease. Thus it was decided to limit the blood culture work to the first day of disease.

The isolation of the causal virus was attempted in two ways: (1) *By direct blood culture* in Wenyon-Noguchi medium; (2) *by intraperitoneal inoculation of guinea-pigs* with blood from sand-fly fever cases.

(1) *Direct Blood Culture*.—As previously stated, blood was intubated into nutrient broth in order to ascertain the presence or not of such organisms as those of the enteric group, *Micrococcus melitensis* and *Streptococcus*; into Wenyon's modification of the special spirochæte medium advocated by Noguchi, by which means it was hoped to detect organisms of this nature if present.

4 Whittingham: *Observations on Sand-fly Fever in Malta*

Aërobic and anaërobic cultures were prepared. The broth cultures were incubated at 37° C., the Wenyon-Noguchi cultures at 27° C. The successful cultivation of spirochætes depends to a large extent on the medium employed. The composition and preparation of the Wenyon-Noguchi medium [10] was as detailed below: A 0·2 per cent. fluid agar was very accurately neutralized to 7·6 Ph.; 15 to 20 drops of sterile rabbit blood were added to this shortly before the medium was required for use.

The formula for the medium was as follows:—

Agar-agar	2	per cent.	or 20 gm.
Sodium chloride	0·5	per cent.	or 5 gm.
Peptone	1	per cent.	or 10 gm.
Lemco	0·2	per cent.	or 2 gm.
Distilled water to	100	c.c.	or 1,000 c.c.

Great care had to be exercised in the neutralization of the agar. This was attained by the hydrogen-ion method [11].

Neutralization with Cresol Red.—Five c.c. of the melted agar was taken. This was diluted with 20 c.c. of hot doubly-distilled water. A few drops of 0·02 per cent. cresol red was added. $\frac{N}{20}$ NaOH was gradually added from a burette until the first purplish tinge was obtained. This occurred with a hydrogen-ion concentration of Ph. 7·5.

Further Neutralization with Phenol Red.—The medium was then neutralized with $\frac{N}{10}$ NaOH almost up to the point recorded by the cresol-red method and tested with phenol red. The determination of the end point with this latter reagent was more complicated.

The apparatus necessary included the following: (1) Set of standard Ph. solutions with indicator added to show Ph. reaction from 6, 6·2, 6·4, 6·6, 6·8, 7·0, 7·2, 7·4, 7·6, 7·8, 8·0, 8·2; (2) a comparator to hold the tubes for comparison; (3) special cordite tubes (Coles' tubes); (4) phenol-red solution 0·01 per cent.

One c.c. of the hot melted agar was added to 4 c.c. of hot distilled water in one of the cordite tubes and 0·5 c.c. of a 0·01 per cent. solution of phenol red added. The tube was put in the comparator in the slot between the standard tubes representing Ph. 7·4 and 7·6 respectively. In order to render the reading more accurate two other cordite tubes were partly filled with the agar diluted 1 to 4, and one placed behind each of the standard tubes. Behind the tube of media under test to which the phenol red was added was placed a cordite tube containing distilled water. The colours were compared, and then the tube under test was cooled beneath the cold water tap until its contents were at room temperature, and the comparator again used. If the amount of alkali added was not sufficient a little more was added, and after the medium was thoroughly shaken the reaction again tested.

The stock agar was kept thus: The agar for use was diluted 1 in 10 with 0·85 per cent. NaCl and, before the rabbit's blood was added, it was again tested with phenol red to determine the hydrogen-ion concentration. The correct reaction being thus established the medium was run into test tubes, 10 c.c. to each tube, or else 30 c.c. into 2 oz. bottles.

The addition of the rabbit's blood was accomplished in batches of fifteen to twenty tubes as required. The method adopted was that of Wenyon. A rabbit was placed in a special box so that the ears could be freely manipulated without the animal moving. One ear was cleaned up and the hairs cut and shaved off a good area at the outer margin. (Whiskers of that side of the animal were also cut as the blood dropping from the ear to the test tube would

be liable to contamination from this source.) The shaven area was then treated with 4 per cent. iodine in spirit. Thereafter, molten paraffin wax was applied with a sterile glass rod. This application was carefully made to the edge and under surface of the ear. Several layers of melted paraffin were applied. In order to make the marginal vein prominent the ear was clipped near its root with a pair of bull-dog forceps. A sharp sterile needle was inserted into the vein and immediately the blood welled out. As the drops fell they were collected into the tubes of 0·2 per cent. agar heated to 50° C.; fifteen drops were collected into each 10 c.c. of agar. This amount gave a distinct fibrin network. At one sitting it was usual to add the necessary amount of blood to fifteen to twenty tubes without ill-effects to the animal. The same rabbit was used again after intervals of three weeks.

(2) *Inoculation of Guinea-pigs with Whole Blood.*—Three to 5 c.c. of citrated whole blood from fifteen cases of sand-fly fever were inoculated intraperitoneally into guinea-pigs. These pigs had their rectal temperatures taken morning and evening for three days before inoculation in order to ascertain their normal temperature. Control pigs were inoculated with a similar quantity of sand-fly fever-free blood.

(III) BREEDING AND LETHAL EXPERIMENTS ON THE SAND-FLY (*Phlebotomus papatasi*).

These have been devised and will be carried out in Malta this summer.

RESULTS.

(i) *Differential Diagnosis.*

(1) *Blood cultures* sown in broth as a routine measure proved sterile in every case.

(2) *Agglutination tests* were negative in all cases, due allowance being made for the protective inoculation with T.A.B. vaccine.

(3) *Wassermann test* was negative in every case chosen.

(4) *Blood Counts.*—The total leucocyte count showed in practically all the cases a leucopenia (4,000 per cubic millimetre) for the first three to five days of disease, followed by a leucocytosis shooting up to 15,000 to 20,000 per cubic millimetre on or about the ninth to eleventh day of disease. This was typical of most cases (eighteen, i.e., 69 per cent.), but a few did not follow this rule (eight, i.e., 31 per cent.). In order to arrive at a fair conception of the leucocyte count as it occurs in sand-fly fever the daily total leucocyte counts of the twenty-six cases were averaged with the result depicted in Chart I (p. 8). The lowest average count was on the first day of disease (5,400 per cubic millimetre); after three days the count became normal and remained so for four days; thereafter a moderate degree of leucocytosis occurred—the count swinging up to 13,600 per cubic millimetre on the tenth day of disease. This leucocytosis was only transitory and disappeared in the course of two to three days. Hourly total leucocyte counts (two cases) showed that the count was normal at the onset of disease, but as the temperature rose the count fell, reaching its lowest point six to nine hours after the onset of disease. Later as the temperature fell the count again rose to normal limits.

The lowest count recorded in this series of cases was 2,700 per cubic millimetre on the first day of disease, the highest 20,600 per cubic millimetre on the tenth day of disease.

6 Whittingham: *Observations on Sand-fly Fever in Malta*

The differential leucocyte count showed that in the first six days of the disease the large mononuclear lymphocytes were relatively increased, registering between 12 and 25 per cent. The leucopenia chiefly affected the polymorphonuclear leucocytes. In some cases the small mononuclear lymphocytes were increased above 30 per cent., but this did not occur until after the tenth day of disease. Broadly speaking all through the disease for fourteen days there was a total lymphocytosis of 40 to 50 per cent. Many of the cases showed a relative polymorphonuclear increase on the first day of disease, and from the figures obtained from hourly counts it was seen that a lymphocytosis was established after a few hours. As regards the eosinophils these tended to disappear during the fever, but returned to normal after the sixth day. In dengue there was stated to be a late eosinophilia, so special attention was paid to this point [14] [15] [16]. The figures obtained were:—

Eosinophils over 3 per cent. in nine cases (33 per cent. of the cases examined)
Eosinophils over 4 per cent. in five cases (20 per cent. of the cases examined)
Eosinophils over 5 per cent. in three cases (11 per cent. of the cases examined)

There was only a true eosinophilia in 11 per cent. of the cases. This eosinophilia occurred on or about the sixth to ninth day and passed off within three days in all except one case. This latter case gave the highest eosinophil count (9 per cent.).

The differential count showed very little change day by day; even when the total leucocyte count rose sharply, as stated, about the tenth day, there was no alteration, except that any eosinophil increase became normal at that time.

The daily total leucocyte count, supported by a differential leucocyte count, helped in establishing the cases as examples of the disease recognized as sand-fly fever by other authorities.

The leucopenia accompanied by a lymphocytosis has been noted by many observers in the past. As early as 1816, Dr. Skey [12] remarked on the absence of the buffy-coat from the blood of sand-fly fever cases. Only one case out of twenty-eight showed the buffy-coat. McCarrison, in 1906 [13], and Birt, in 1910, dealt with the subject more fully. Birt states that "there does not seem to be any great fluctuation in the number of the leucocytes from day to day. The leucopenia may continue into convalescence. The polymorph leucocytes are diminished; they number about 56 per cent. instead of the usual 65 to 75 per cent. The large and medium-sized mononuclears are increased: the lymphocytes and eosinophiles are decreased." Later in 1913 [14], the same observer refers to a late eosinophilia both in dengue and sand-fly fever.

The recent observations made in Malta confirmed these points as a whole, except that there was found to be a marked daily fluctuation about the tenth day; the leucopenia passed off in every case before the sixth day, and only in three cases was there an eosinophilia which passed off in about three days.

Of the long list of diseases that might be confused with sand-fly fever those which show a leucopenia during the febrile period are: (a) Dengue, (b) influenza, (c) seven-day fever, (d) enterica, (e) malaria.

Dengue shows a leucopenia associated with a lymphocytosis during the fever. Balfour [15] and Harnett [16] refer to a late eosinophilia. This may be due to the occurrence of dengue in areas where helminthic infection is common. Generally speaking the eosinophils disappear from the peripheral blood during any fever, so that in a person with a helminthic infection a return of the eosinophils is likely after the pyrexia has subsided.

In a typical case of influenza a chart of the daily total leucocyte count and temperature appears identical with that of a typical case of sand-fly fever [17] (cp. in Chart I). As in sand-fly fever, there is a transitory eosinophilia about the sixth to ninth day, but a point of distinction is that the leucopenia in influenza is associated with a small mononuclear increase (see Tables II and III).

TABLE II.—LEUCOCYTE COUNT IN A TYPICAL CASE OF SAND-FLY FEVER.

Day of disease	Temperature degrees F.	In 1,000's				Differential count per cent.								Per cent.	
		Total white cells	Small lymphocytes	Large lymphocytes	Neutrophils	Small lymphocytes	Large lymphocytes	Hyalines	Transitionals	Neutrophils	Eosinophils	Basophils	Leucoblasts	Non-granular	Granular
1															
2	99.8	4.3	0.76	0.6	2.3	17½	14½	1	12½	53½	—	½	—	33	67
3	100	4.7	1.03	0.5	2.3	22	12½	2½	12½	50	1½	—	—	36	64
4	98.4	3.5	0.5	0.5	1.5	15	14½	2	23	43½	1½	½	—	32	68
5	98.2	4.2	0.8	0.9	1.3	20½	23	2½	16½	32½	3	1	½	46	54
6	98.2	6.8	1.3	0.9	2.4	19½	14½	2½	25	36½	2	½	—	36	64
7	98	6.0	1.2	0.9	3.0	20½	15	½	10½	51	1½	1	—	36	64
8	98	8.2	1.4	1.3	3.8	18½	17	½	15½	46½	1½	½	—	36	64
9	97.6	7.6	1.5	0.5	4.5	20½	7	—	8	60	3½	½	—	28	72
10	98.6	18.8	2.2	1.6	12.9	12	9	½	7	68½	1½	½	½	22	78
11	98	8.8	1.3	0.4	5.4	15½	5½	½	14½	61½	2	½	—	21	79
12	99	10.2	2.1	1.6	4.6	21½	15½	½	15	45½	1½	½	—	38	62
13	98.4	7.2	2.0	0.6	3.3	28½	9	—	13	47	2½	½	½	37	63
14	98.4	8.6	3.0	1.0	3.7	35½	½	—	6½	43½	½	½	½	48	52

Note the Large Lymphocyte column (*) and compare the double figures in this column in sand-fly fever with the single figures in Influenza (Table III).

TABLE III.—LEUCOCYTE COUNT IN A TYPICAL CASE OF INFLUENZA.

Day of disease	Temperature degrees F.	In 1,000's				Differential count per cent.								Per cent.	
		Total white cells	Small lymphocytes	Large lymphocytes	Neutrophils	Small lymphocytes	Large lymphocytes	Hyalines	Transitionals	Neutrophils	Eosinophils	Basophils	Leucoblasts	Non-granular	Granular
1	102.6	7.5	1.2	0.3	4.8	16	5	1	11	65	1	—	—	22	77
2	102.8	5.6	1.0	0.3	3.5	18	6	3½	8½	64	—	½	—	27	72
3	100	10.0	4.0	0.5	4.4	40	5½	1	8½	44	—	—	—	47	53
4	100.2	10.6	3.0	0.9	5.6	28	8½	1½	8½	53	—	½	—	38	61
5	99	5.0	2.2	0.4	1.6	44	8	1	12	32	1	2	—	53	47
6	99.2	8.1	1.8	0.5	5.1	22	7	—	6½	63	½	½	—	29	71
7	97.6	15.0	4.4	0.7	8.2	29	4½	—	8½	55	1½	½	—	34	65
8	98.5	8.7	2.8	0.4	4.6	32	5	—	7½	53	½	½	—	37	62
9	98	10.6	3.3	0.8	5.4	32	8	1½	5½	51	2½	½	—	41	59
10	98	10.6	3.3	0.3	6.2	31	3½	1	5½	58	½	—	—	35	64
11	97	6.5	1.7	0.3	3.5	27	6	3	5½	54	½	½	½	35	64
12	97.2	16.2	5.9	0.5	7.6	36	3½	2	8	47	2½	1	—	41	58
13	98	7.5	2.5	0.2	4.0	34	3	1	5	54	1½	1½	—	38	62
14	97.2	6.8	2.9	0.2	3.1	42	3	2	4	45	2½	1	—	47	52
15	97.4	23.7	6.3	1.0	12.8	26	4½	1½	12	54	1½	½	—	32	67
16	98	26.8	6.2	1.4	14.6	23	5½	1½	11	54	3	1½	—	30	70
17	97.2	31.2	4.8	2.2	17.2	15	7½	1½	18	55	2	½	—	24	75

* See also Large Lymphocyte column in Table II.

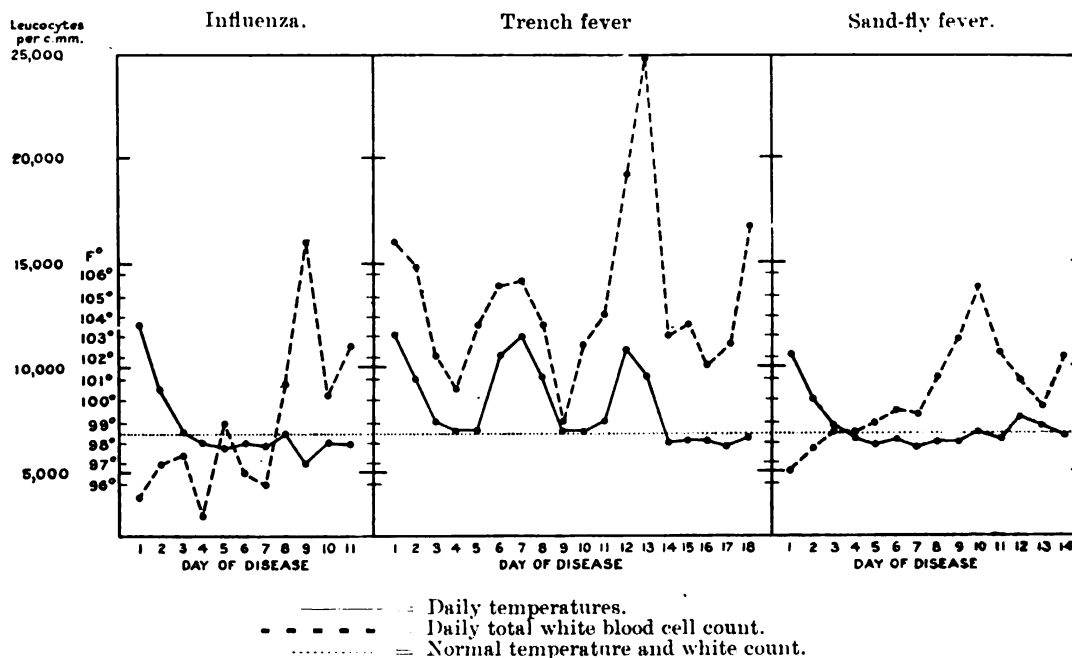
8 Whittingham: *Observations on Sand-fly Fever in Malta*

Enteric fever usually shows a normal count to begin with, later there is a leucopenia associated with a lymphocytosis (large or small) [18]. Blood culture and agglutination tests should decide the diagnosis here.

In benign tertian malaria the total leucocyte count is slightly increased (10,000 odd per cubic millimetre) just before the occurrence of the fever; as the temperature rises a leucopenia sets in (3,000 per cubic millimetre) but a differential leucocyte count shows the hyalines increased to over 4 per cent. Moreover, the malarial parasite can usually be detected. A daily count, taken at 10 a.m. on a case of benign tertian malaria shows a count of 10,000 or more white corpuscles per cubic millimetre on the day of fever and on alternate days following this, and a relative leucopenia on the day of apyrexia; this alternate daily rise and fall of the leucocyte count is very helpful in making a diagnosis of malaria when parasites cannot be found.

A glance at a chart of trench fever shows an entirely different picture. Here the temperature and total leucocyte counts run parallel. The fever is associated with a definite leucocytosis (15,000 to 20,000 per cubic millimetre)

CHART I.—GRAPH SHOWING THE TEMPERATURE AND LEUCOCYTE CURVES IN INFLUENZA, TRENCH FEVER AND SAND-FLY FEVER.



Note—the leucopenia in the febrile state of influenza and sand-fly fever, whilst in trench fever the fever is accompanied by a leucocytosis.

which persists for four to five days and recurs regularly for several weeks, perhaps months. Moreover, the polymorph leucocytes are increased in this disease (see Chart I).

(5) *Urine Examination*.—The urine of two cases showed a small amount of albumin; one of them also contained bile in a relapse on the eleventh day of the disease. No spirochaetes were found in the urine.

(6) *Clinical Examination*.—In most cases the symptoms were typical—the temperature in practically every case reached its highest limits on the first day of disease—on an average 102° F.; fell to normal within three days and

remained normal. Five cases (20 per cent.) showed a recrudescence between the ninth and twelfth days of disease—the temperature was over 99° F.; in fact, three of them had temperatures over 102° F. and all the symptoms of an acute sand-fly fever attack. One showed jaundice with this recrudescence. The latter case developed fever again on the twenty-fifth day after the primary disease had appeared—hence this case was included amongst those of true relapse. Three cases (12 per cent.) relapsed on the twenty-fifth, twenty-sixth and forty-sixth days respectively after the first sign of fever. It will be noted that leptospira were recovered from the blood of six cases—two of which cases showed relapse, and one a recrudescence of the fever.

Marston in 1855-6, in Malta [8], observed that relapses occurred in about 15 per cent. of cases at indefinite periods. Birt, 1910 [3], reports second attacks in 6 per cent., and once or twice a recrudescence of the fever on the fourth or fifth day. In the "Memoranda on Medical Diseases in the Tropical and Sub-Tropical War Areas, 1919" [20], an after-rise of temperature is said to be by no means uncommon in some outbreaks.

From a consideration of such points as have been described the cases that were chosen for this investigation conform to the diagnosis of "sand-fly fever."

(ii) *Attempts to Isolate the Causal Virus.*

(1) *Direct Blood Culture.*—Dark-ground illumination of the primary blood culture from twenty-six cases in the Wenyon-Noguchi medium revealed after the fifth to sixth day of incubation at 27° C., the presence of spirochætal bodies in six cases. These spirochætes morphologically resembled leptospira. Three of these primary cultures were successfully subcultured. Unfortunately one of the subcultures was contaminated, and lost in transit from Malta to England.

The leptospiræ isolated were morphologically indistinguishable from *Leptospira icterohæmorrhagiæ*, the average length being 10 to 15 μ , and width about 0.3 μ . They were very actively motile, proceeding in either direction at will. A marked vibratory movement, like a piece of string being twisted and quickly pulled taut, was very common. The organism crawled backwards and forwards through any fibrin masses encountered, like a worm entering or leaving the soil—a distinct elastic-like movement. Some forms had a hook at one end, others at both ends, and others a hook shaped like a "?-mark."

As these organisms were found in cultures treated aërobically, in the latter cultures the procedure of pouring sterile paraffin oil on the upper surface of the medium was dispensed with. The organism was most abundant about $\frac{3}{4}$ to 1 in. below the surface of the medium. In fact after about ten to fourteen days in some cultures a fine but distinct white haze formed a ring about an inch from the surface of the medium; this ring consisted of masses of leptospira.

Secondary organisms, so common in the dust-laden air of Malta, quickly killed off the leptospira in any cultures to which they gained access.

One primary culture was maintained alive for fourteen weeks, a secondary culture for nineteen weeks; after that it was lost through contamination.

For a time the organism grew readily in subculture, but recently after seven months, difficulty has been found in recovering it from subcultures. The technique has remained constant, and the media were very carefully neutralized. As the organism could not be recovered from guinea-pigs at

10 Whittingham: *Observations on Sand-fly Fever in Malta*

any time, the method of animal passage to increase its viability could not be utilized.

It is interesting to note that Couvy, 1921 [21], has reported from Beirut the finding of spirochætes in the blood of cases of dengue.

(2) *Inoculation of Guinea-pigs with Whole Blood*.—Most of the guinea-pigs inoculated with the whole blood of sand-fly fever cases showed a rise of temperature of about 2° F. for the first forty-eight hours after inoculation. Control pigs inoculated with blood from presumably sand-fly-fever-free cases failed to show this rise. Two of the control pigs collapsed and died in the first twenty-four hours after the inoculation. There was an epidemic of diarrhœa amongst the pigs—only a small number were really healthy before experimentation. Great difficulty was experienced in getting healthy pigs. All the pigs that died were examined, cultures were made of their heart blood, liver and kidneys, and the latter organs sectioned. In no case were leptospiræ found. None of the pigs developed any symptoms of epidemic jaundice.

As has already been stated, the blood from fifteen cases only was inoculated into guinea-pigs. Owing to it being impossible to tell which cases would be likely to yield leptospiræ the cases chosen were taken at random. Thus two of the cases from which leptospiræ were obtained by direct blood culture failed to have their blood inoculated into guinea-pigs; one of these cases later developed jaundice. Fortunately four of the cases which gave a positive blood culture were controlled by animal inoculation. These all proved negative.

The cases which showed leptospiræ in the primary cultures were the following:—

(1) *Case 17 (W.)*.—Sharp typical attack of sand-fly fever, June 30, 1921. Two-day type of fever. Maximum temperature 102° F., registered on the first day of fever. Recrudescence on twelfth day—another typical attack of sand-fly fever, maximum temperature 102·2° F.; returned to normal in forty-eight hours. Thereafter made good recovery. Culture brought to United Kingdom (*see* Chart II).

(2) *Case 2 (J.)*.—Admitted July 2, 1921, with typical attack of three-day fever. Maximum temperature 102·2° F. on the first day. Slight amount of albumin in the urine on third and fourth days of disease. Temperature rose to 99° F. on twelfth day. Leucopenia for first three days; lowest count 3,400 per cubic millimetre; marked leucocytosis (20,600 per cubic millimetre) on tenth day. Felt weak for some time, otherwise made an uninterrupted recovery. Culture brought to United Kingdom.

(3) *Case 14 (M.)*.—Admitted July 8, 1921. Severe attack of three-day type of fever. Highest temperature 102·8° F. on first day of disease. No leucopenia. Leucocytosis (12,000 to 18,000 per cubic millimetre) after the third day. Felt very weak after first attack. Relapsed on twenty-sixth day of disease; temperature 103° F.; three-day fever. Weakness followed. Culture contaminated in transit from Malta to England.

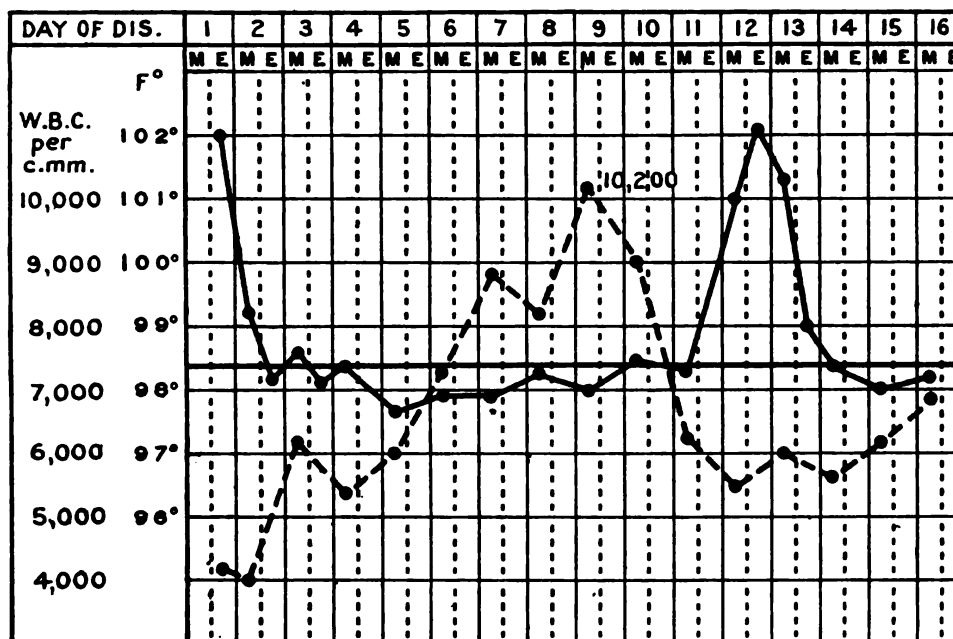
(4) *Case 5 (D.)*.—Admitted July 2, 1921. Mild attack of three-day fever. Maximum temperature 100·6° F. on second day, and 99° F. on twelfth day. Leucopenia for seven days; distinct leucocytosis on tenth day (18,000 per cubic millimetre). Uninterrupted recovery. Primary culture contaminated in Malta.

(5) *Case 15 (G.)*.—Severe case of four-day type of fever. Admitted July 8, 1921. Maximum temperature 103·6° F. on first day. No relapse. Leucocyte count normal until fifth day, when there was a leucopenia (4,500 per cubic millimetre); it rose on tenth day to 16,000 per cubic millimetre. Patient remained weak for some time. Primary culture contaminated in Malta.

(6) *Case 21 (P.)*.—Acute attack of three-day fever started June 29, 1921. Maximum temperature 103° F. Recrudescence on eleventh day; temperature 102° F.

Jaundice developed after three days, disappeared in six days. Relapsed twenty-five days after primary fever; temperature 102.4° F.; three-day fever with all the symptoms of sand-fly fever. Primary cultures contaminated in Malta.

CHART II.



Broken line --- total leucocyte count.

TABLE IV.—HISTORY OF SAND-FLY FEVER AS IT OCCURRED IN THE SIX CASES FROM WHICH LEPTOSPIRÆ WERE ISOLATED BY DIRECT BLOOD CULTURE.

Case No.	Type of fever	Recrudescence	Relapse	Blood inoculated into guinea-pig
17 (W.)	Two-day	Twelfth day (102.2° F.)	—	Yes
2 (J.)	Three-day	Twelfth day (99° F.)	—	Yes
14 (M.)	Three-day	—	Twenty-sixth day (103° F.)	Yes
15 (G.)	Four-day	—	—	Yes
21 (P.)	Three-day	Eleventh day 102° F., jaundice fourteenth day	Twenty-fifth day (102.4° F.)	No
5 (D.)	Three-day	Twelfth day (99° F.)	—	No

EXPERIMENTS WITH LEPTOSPIRA ISOLATED.

(a) Inoculation of Guinea-pigs with Cultures of *Leptospira*.

As it was thought that the primary cultures might contain toxins as well as the leptospira, they were not used for inoculation experiments on animals or man. Three guinea-pigs were inoculated intraperitoneally with primary subcultures—seven, eight and ten days old respectively. None of these pigs died or showed any symptoms of jaundice or hæmorrhage. Six guinea-pigs were inoculated with one-week-old secondary subcultures. One of these pigs

12 Whittingham: *Observations on Sand-fly Fever in Malta*

died on the tenth day after inoculation. There was neither fever nor any symptoms of note. Attempts to isolate leptospira from the heart blood, liver and kidneys failed. Six guinea-pigs were inoculated with four weeks' old primary subcultures. One of these pigs died on the twelfth day after inoculation without symptoms of fever, hæmorrhage or jaundice, nor could leptospiræ be recovered from its blood or organs.

(b) *Inoculation of Man with Cultures of Leptospira.*

Only three men have been inoculated up to date—two in an area where sand-fly fever occurs, and one in a sand-fly-fever-free area. Case 1 was inoculated with $\frac{1}{2}$ c.c. of primary subculture (six days old), Case 2 was inoculated with $\frac{1}{2}$ c.c. of primary subculture (six days old), Case 3 was inoculated with 1 c.c. of primary subculture (six weeks old), and nine days later a further inoculation of 1 c.c. of a six-day old secondary subculture was carried out.

In none of the cases was there very definite fever. Case No. 1 had a temperature of 99° F. or over, from the sixth to the eighth, after inoculation. The highest point registered was 99·8° F. There was pain in the legs and suffusion of the conjunctivæ for several days in Cases Nos. 1 and 2. Case No. 3 developed headache, anorexia, marked nausea, tendency to syncope, and pains in the calves on the seventh to ninth days after the first inoculation. The blood picture showed a distinct leucocytosis (15,000 per cubic millimetre). A similar result, with more pronounced symptoms, lasting for several days, followed the second inoculation.

On account of jaundice developing on the eleventh day of disease in one of the sand-fly fever cases under investigation in Malta it was considered inadvisable to do further inoculations in man.

CONCLUSIONS.

A disease clinically resembling sand-fly fever was studied in Malta. Owing to the small number of cases investigated the following conclusions must be accepted tentatively:—

(1) *The type of fever* was chiefly of the three-day type. In 20 per cent. of the cases a recrudescence occurred between the ninth and twelfth days. In 12 per cent. of the cases relapses took place between the twenty-fifth and forty-sixth days.

(2) *The leucocyte count* aided materially in the diagnosis of the disease and it was established that a typical case of sand-fly fever showed an initial slight leucocytosis which passed off in a few hours. A leucopenia soon set in and persisted for three to four days. On or about the tenth day a distinct leucocytosis occurred; this was transient. The large lymphocytes were increased during the first six days of the disease. There was no abnormal eosinophilia in uncomplicated cases. The return of the lymphocytes to the normal number was not completed in a fortnight. A third of the cases (thirty-one per cent.) did not conform to this, but registered a more or less normal total leucocyte count or a leucocytosis accompanied by a certain degree of polymorph increase.

(3) *A leptospira*, morphologically resembling the *Leptospira icterohæmorrhagix*, was isolated on the first day of fever by direct culture from the blood of six clinically sand-fly-fever cases.

Koch's postulates have not been fully carried out in respect of this organism; it was isolated from the human blood stream and cultivated outside the body; but inoculation into men and animals failed to reproduce the

typical disease, nor could the organism be recovered from the animals inoculated. This failure may partly be due to the fact that the organism must pass some part of its life cycle in another host (the *Phlebotomus papatasi*) before it becomes properly infective.

The leptospira differed from the *Leptospira icterohæmorrhagiæ* from its not being pathogenic to guinea-pigs. It has been found, however, that the pathogenicity of cultivated spirochætes may be quickly lost. Griffith [22] states: "A culture grown at 37° C. which, when a fortnight old produced typical spirochætosis in a guinea-pig, lost its virulence within the next fortnight. In another experiment with spirochætes grown at 25° C., a fourteen days' old primary culture produced fatal hæmorrhagic jaundice, while the same culture when three and a half and four months old was completely non-pathogenic." Moreover, spirochætes have been isolated from cases of jaundice in France and Albania, which are non-pathogenic to guinea-pigs.

The point to consider is, were all the cases investigated at Malta true cases of sand-fly fever? The varying types of fever met with in Malta from year to year and even during different stages of the hot weather have been commented on by many authorities, including very old residents [1], [2], [3]. There have been outbreaks of sand-fly fever varying from mild influenza-like attacks to severe forms termed "yellow fever." In fact, yellow fever was reported in Malta in the autumn of 1822, while the fever during the summer was of a milder type. Dr. Lightbody remarked on the absence of the buffy coat (i.e., leucopenia) during the summer, and its presence (i.e., leucocytosis) in the autumn epidemic. Some of the cases investigated in the summer of 1921 showed a slight leucocytosis, some relapsed, one even developed jaundice. It is just possible that the spirochæte found was a modified form of that which causes epidemic jaundice—modified by passage and perhaps atmospheric conditions. The latter point is suggested by the experiments carried out by the R.A.M.C., at Millbank, in 1909 [3], with phlebotomi infected in Malta from cases of sand-fly fever; there was no definite fever though abdominal and syncopal symptoms were present.

Sand-fly fever is thought by many to be a disease scarcely worth considering. The fever is soon over and the patient back to work. But outbreaks vary. Convalescence may be prolonged for weeks and weeks. The nervous system may be involved and such symptoms as weariness, mental depression, and even insanity follow. D.A.H. may occur leading to syncopal attacks. These nervous phenomena are serious enough on *terra firma*, but they are magnified considerably in the case of the air pilot. A little delay in the reaction time may mean a serious crash. In short a condition of "flying exhaustion" or "neurasthenia" is produced. This may be due to any continuous strain on the nervous system, whether it arises from physical or chemical causes. The toxins of sand-fly fever come into the latter category.

It is obvious that sand-fly fever is a serious menace to the man-power of the R.A.F. and further investigation into its causation and prevention is urgently required, especially as there is the chance of a mild spirochætal disease developing under suitable conditions into a serious one.

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14 Whittingham: *Observations on Sand-fly Fever in Malta*

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War Section.

President—Sir JOHN GOODWIN, K.C.B., C.M.G., D.S.O., A.M.S.

Venereal Disease as a War Casualty.

By Surgeon Rear-Admiral WILLIAM BETT, M.V.O., R.N.

IN all recent wars the problem of venereal disease has been a pressing one, not that there is usually more disease during war, but because in war every man is urgently required. I have a few figures and tables showing the incidence of venereal disease in various forces and at various times; it will not be necessary to burden you with many of these. Looking back, we find there has been a very marked improvement in the British Army and Navy since 1860.

The incidence per 1,000 of strength in the Army was, in 1860, 316; in 1895, 238; in 1911, 114; in 1912, 56; in 1921, 40; in 1922 (first half), 34. In 1898 the percentage of venereal diseases to all diseases was 30·7; in 1912, 16·3; in 1921, 7·5. It is remarkable that in 1888 the number of cases of gonorrhœa and syphilis was approximately the same; in 1912 the proportion is estimated as 3 gonorrhœa to 2 syphilis; in 1921, 3·3 gonorrhœa to 1 syphilis (Southern Command).

In the Navy, in 1870, the admission ratio was 98; in 1888, 154; in 1913, 93; in 1915, 67.

As regards early wars we have little precise information. Syphilis was almost unknown in Europe until near the end of the fifteenth century. It appears to have been first really noticed soon after the return of Columbus and his followers from the discovery of the Western World, and to have been disseminated over Italy, France, Spain and England by the demobilized adventurers of the army of Charles VIII of France that had besieged Naples. Complete absence of immunity and lack of proper treatment were the probable causes of its rapid spread and great virulence. It is remarkable that the disease was not at first recognized as "venereal" (this is probably explained by the comparatively long incubation period). Its venereal origin, however, was acknowledged by 1519, and Ulrich Van Hutten, writing in that year, says that the disease was then less virulent than when it first appeared. Van Hutten himself contracted the disease, and was under the impression that he cured himself with guaiacum after failing with mercury.

Gonorrhœa appears to have been known in all countries from the earliest times: the word is first found in the writings of Aretaios of Cappadocia in the first or second century A.D.; the disease is, however, alluded to in the

Papyrus Ebers, the oldest Egyptian medical record, and in the Book of Leviticus; it is also said to be well described in a Japanese manuscript of about 900 B.C. Great confusion in nomenclature existed until almost recent times, and certainly as late as 1830 some reputable physicians affirmed that syphilis, gonorrhœa and chancroid were essentially the same disease.

Very little is said about any form of venereal disease by the military historians of the sixteenth, seventeenth and eighteenth centuries, yet we know from other sources that these diseases were prevalent in armies and were very severe. The Duke of Alva's army in Portugal, in 1579, suffered from a very virulent form of venereal disease; it is said that amputation of the penis for phagedæna was performed in 5,000 cases. This general (the Duke of Alva) was a believer in the regulation of prostitutes; he organized them into bands, with uniform and mounted officers; any member of this force lacking a health certificate might be ordered 200 lashes. Another well-known general, Strozzi, believed in the suppression of prostitution, adopting on one occasion the barbarous method of throwing 800 prostitutes into a river.

In 1706, an Anglo-Portuguese army near Madrid was so badly infected by prostitutes, said to have been sent from that city for this express purpose, that 6,000 cases are said to have been in the hospitals, of whom the greater number died.

It was supposed that syphilis was very prevalent in the French army during the eighteenth century (two out of every three soldiers said to be syphilitic).

In the Peninsular War the official history hardly alludes to venereal disease, but in a paper read by Ferguson¹ before the Medical and Chirurgical Society of London, it is stated that a very severe type of disease occurred; there was some idea that the Portuguese did not use mercury, and for this reason had developed a certain racial immunity while the virulence of the disease remained unabated. The total number of cases is said not to have been very great (about one to thirty-six of all diseases), and the reasons for the small number of cases are given as hard work, strict discipline, and absence of opportunity.

In the Crimean War the army does not appear to have suffered very severely from venereal disease. It was noted that in most cases the disease had been contracted at home. In the Navy, the incidence in the Baltic Fleet, where there was frequent communication with various ports, was 47 per 1,000; in the Black Sea Fleet only 20·6 per 1,000.

In the American Civil War the facts as regards syphilis are instructive (I have not got the figures for all venereal diseases). The general incidence-rate per 1,000 which had for many years ranged in the neighbourhood of twenty-five, remained much the same for some time. In 1865, however, when the war was practically over, it rose rapidly, and during three years, 1867-68-69, was over 100 per 1,000. It fell gradually to 9·3 in 1899, but rose gradually again to 25·5 per 1,000 in 1909.

In the Franco-Prussian War the German rate per 1,000 for all venereal disease was approximately 42; these cases were about 7 per cent. of all sick cases, that is to say excluding wounds in action, &c. Very strict measures were taken for the regulation of brothels, and the registration and examination of prostitutes. The ratio rose after the war, but fell in a short time. Since

¹ *Med. Chir. Trans.*, 1813, iv, pp. 1-16.

1874 the admission rate for all venereal disease has been lower in the Prussian Army than in any other.

During the South African War the incidence of venereal disease in the Army was diminished. There was some increase after the war.

THE LATE WAR.

It is difficult to get precise figures as to incidence of venereal disease. In 1917, from 7,500 to 9,000 men were constantly in hospital in the United Kingdom suffering from venereal disease—a total of 54,884 admissions: annual rate per 1,000 of strength, 43. In France during the same year, it may be calculated that there were as a rule 8,000 cases constantly in hospital. In the Egyptian Army the incidence was about 46·8 per 1,000 in 1915, and in 1917 about 32. According to one estimate, during about two and a half years of war, English hospitals took in 70,000 cases of gonorrhœa, 21,000 cases of syphilis, 6,000 cases of chancroid, and it is said that during the same period about 40,000 or 50,000 cases of syphilis and 150,000 to 200,000 cases of gonorrhœa occurred in France. In one camp of Canadians 5,250 cases of venereal disease occurred in one year (numbers not known).

I do not vouch for the absolute accuracy of these figures, but at any rate they give some idea of the situation.

At a time when every man was most urgently required, approximately 20,000 men were constantly sick from one disease alone, and that, a disease which must be looked upon as to a large extent preventable; nor does the loss stop there by any means. These cases occupied large hospitals and large medical and nursing staffs which would otherwise have been available for other sick and wounded. Their transport to and from hospital, and transport for their reliefs, added largely to difficulties already great enough; and although they were kept in hospital for long periods in order that they might return to the fighting line as fit men, it may be looked upon as certain that many of them would not really recover their full strength and vigour for a very considerable period. Some of course were permanently incapacitated and had to be invalided.

I believe the stay in hospital of a venereal case in France averaged nearly two months, though many of them occupied a good part of this time in undergoing physical training.

The main question to be considered is: What steps can be taken to reduce the incidence of venereal disease in the Army and Navy in future wars and during peace.

Here I will quote a remark made by a very able and experienced Army Medical Officer, from whom I have had much valuable help in preparing this paper. He said: "I do not think the title of your paper is sound; venereal disease is not, properly speaking, a war casualty at all. These diseases are prevalent in the civil population in peace, much more prevalent than is popularly supposed. In the late War they came from the civil population to the Army and Navy, where they were discovered, diagnosed and tabulated. People are apt to talk as if the Services were a sort of reservoir of venereal diseases from which they spread abroad; the exact contrary is the case." This point will again be alluded to.

A great deal has been done, and is being done, in the way of prevention, both in the Services, and what is equally, or more important, among the civil population. In both Services, lectures are given to all ratings; in these

lectures, the possibility and the advisability of living a clean life is dwelt upon, not merely for the sake of avoiding venereal disease but on account of higher ethical and social considerations. As it is quite certain that this excellent advice will be frequently disregarded, some account of the possible consequences of impure intercourse is given in the lectures and measures for personal prophylaxis are described in detail. The necessity for reporting any venereal disease at once is always strongly insisted on. Men are made acquainted with the earliest symptoms in order that they may be able to do this.

It is our experience in the Navy, and I am also told the same holds true in the Army, that concealment of disease is now quite uncommon, and, in fact, it is an ordinary occurrence for men to report, who think they may have contracted venereal disease in whom no symptoms whatever can be detected.

The modern treatment of syphilis by the organic arsenical compounds, constituting perhaps the greatest advance in medical science that has been made for centuries, is, in itself, a most valuable means of protection to the community. If men report the moment a sore is noticed, the spread of the disease is limited both directly—for many men formerly continued their sexual habits long after the first appearance of a chancre—and, ultimately, the chance of complete cure being so much greater.

The treatment of gonorrhœa has, unfortunately, not advanced in the same way: still, the prospect of complete cure is much better when the disease is seen in an early stage.

It is not proposed to enter to any extent into the controversy with which we are all so familiar, between the societies for "combating" or for "preventing" venereal disease. But my opinion is that this evil is so great and so widespread, so terrible in its effects, so fatal to our national efficiency, both during war and peace, and so apt to involve the innocent wives and families of those who have contracted disease through their own imprudence, that almost any measures are justifiable if only they are efficient.

In the Navy, for many years, men have been provided with disinfecting materials—packets. The packet system may be said to include the issue of ointments, solutions or condoms for the purpose of personal prophylaxis. All these have been recommended in the past and are not so modern as they are thought to be. Dr. Conton, a French physician, practising in London, invented condoms in the reign of Charles II. Sir William Forsyth recommended the application of mercurial ointment as a prophylactic in the year 1767. John Hunter advised corrosive sublimate solution in 1786 for the same purpose. De Piacenza, writing as early as 1474, advises "washing thoroughly immediately after coitus as a means of preventing corruption." His book is quoted by Swediaur, who found it in the library at Bologna.

One objection to "packets" of any kind is that they are not quite reliable. They are not reliable because they are often not properly used. This is what one would expect, taking into consideration not only the circumstances, the excitement, or the apathy, frequently the partial intoxication, of the men who have to use them, but also the fact that they are likely to be mislaid, or in the case of solutions, spilt, or, in the case of condoms, used in a filthy condition. It has to be remembered also, that the act of coitus is often repeated several times, this making it virtually impossible for a man to have a sufficient supply of material.

I believe that the best method of prophylaxis for men consists in thorough washing with soap and water immediately after the act, together with micturition, and I think that great stress should be laid on this in all lectures:

immediately—that is the great point. It is true that soap and water are not always available in houses and rooms used ; they ought to be—they often are—and if men insisted on them they would be more often there ; men will insist on them if properly impressed with the necessity. Ointments or lotions can be used—should be used in addition if available.

I am aware of the objection that intercourse often takes place out of doors, in lanes, hedgerows, &c. ; but I do not believe that there is much chance of ointments or lotions being properly used under these conditions, and a man should be advised, even if he uses them, to make his way, as soon as possible, to some place where he can wash himself thoroughly. Here comes in the importance of the ablution room. This should be provided with trough and taps at suitable heights and should be supplied with disinfectants which may be applied with the maximum efficiency after thorough cleansing of the parts. Warm water should be supplied if possible. Ablution rooms are being fitted up in large numbers of naval establishments and ships and are proving of great value.

I may say that in my opinion, condoms are more efficient (if properly used) than either lotions or ointments, and I find this view advocated by German medical officers of experience.

Extraordinarily good results have, it is true, been reported both in the Army and Navy from the use of permanganate of potash solution only, but we have to be careful before drawing the conclusion that low incidence of venereal disease, following the adoption of any given measures, has been actually and *solely* due to those measures.

The incidence varies a good deal owing to a large number of conditions. Some regiments have been known to suffer habitually more than others ; the advent into any district of a number of badly diseased women, or conversely, of a number of healthy or recently treated women would be a deciding factor, one way or the other.

One difficulty in drawing precise conclusions, arises from the fact that we are never able to tell how many men have taken no precautions and still escaped disease, or how many have escaped after using precautions. Sir Jonathan Hutchinson said that he thought disease was contracted about once in a hundred serious risks and his opinion on a point of this sort carries a good deal of weight, though I cannot help thinking his estimate is too low.

Another difficulty consists in the fact that one cannot believe the statements made by men who have contracted disease as to the precautions taken. Again, all lecturers inculcate the desirability of living a chaste life ; none but a most confirmed optimist expects this advice to be taken by all his hearers. On the other hand, it may be, and probably is, taken in some instances.

It seems to me quite clear, however, that the use of packets and the employment of preventives of this nature have had a very large measure of success in diminishing disease, and that they should be issued to men in the Services who should be instructed in their use, as well as to the necessity for thorough cleansing of the parts at the earliest possible moment. One can hardly imagine what the state of this country would have been to-day but for (a) *Arsenical treatment*, (b) *Prophylaxis*.

Very good results are said to have been obtained in some United States ships—every man who went ashore in any suspicious locality was made to cleanse and disinfect himself on returning to his ship, whether he admitted exposure or not.

Any form of punishment either for contracting venereal disease or for not using prophylactics is to be deprecated. It only leads to concealment and subterfuge. About 1873, Lord Cardwell issued instructions for the punishment of men with venereal disease. This notoriously led to concealment and eventually to a large increase in the number of cases of secondary syphilis.

The relation of alcoholic habits to venereal disease has been much discussed. A certain amount of alcohol may temporarily stimulate the sexual system, large amounts probably depress it; this is in accordance with what is known of the effects of alcohol in general. Total abstainers are probably not more chaste than other men. The real point is that alcohol lessens a man's power of discrimination, and lowers his standards so that he has recourse to degraded women, with whom, if sober, he would be ashamed to consort.

Dr. Lomholt, of Denmark, made observations on about 500 cases in which he was able to obtain some history of the origin of the disease, and the man's state when it was contracted; and his results, as far as they go, rather bear out the above remarks which, I believe, are also in accordance with the recently expressed opinion of Major Harrison.

Very hard work and very small pay have been advocated as likely to minimize venereal disease, and these conditions did, probably, largely account for the small amount of disease in the German army; they can hardly be thought of in a voluntary force.

I may say here that I object to the term "misconduct" as applied to men who contract venereal disease; as medical men we are not judges of conduct, and if we were, we should have to make allowances for young, full-blooded men, well fed and but imperfectly instructed, placed under entirely unnatural conditions and exposed to the strongest of all temptations. Therefore I do not think we should talk much about *misconduct*.

Apart from these considerations, a certain number of men contract venereal disease from their wives; no doubt they form a small proportion of the whole, but it is probable that their actual numbers are under-rated. Many men are too proud to mention the disgrace which seems worse than the disease.

(a) MEASURES IN THE SERVICES.

The importance of encouraging healthy and manly games and sports among the men of both Services cannot be too strongly insisted upon—a man who wants to keep fit will not infect himself with venereal disease if he can help it. It is also most essential to provide men with other interests, reading and recreation rooms and the like where he can be comfortable; lectures on interesting subjects, opportunities for instruction in handicrafts, and so on; also concerts or theatricals in which men can take part. It is fair to say that nearly all senior officers of both Services are alive to the necessity of these things, and that every effort is made by the Admiralty and the War Office to provide facilities for them.

(b) VENEREAL DISEASE IN CIVIL POPULATION.

To what extent does venereal disease exist in the civil population? This is a most difficult question to answer. Many investigations have been undertaken in various countries at various times and with various results. In a report issued in 1868 it was stated that 6·92 per cent. of all patients at certain institutions were the subject of venereal disease clinically; 33 per cent. of all

prostitutes were thought to be infected—this was probably far too low an estimate.

Some investigations made at the London Hospital showed that among 1,000 patients under treatment for other diseases 10·3 per cent. of males and 5·1 per cent. of females had positive Wassermann reactions.

Sir John Collie, as a result of the clinical examination of 1,119 cases of accident and of 557 presumably healthy persons examined for purposes of their work, found that the case percentage of venereal disease was 5·5. He also had the blood of 491 apparently healthy persons subjected to the Wassermann test—9·36 per cent. of the tests were positive.

In fourteen asylums, representing all classes of the community, 15·4 per cent. of bloods yielded a positive Wassermann reaction. Sir Frederick Mott¹ found that Poor-Law infirmaries in London yielded 19·9 per cent. of positive blood Wassermann reactions: mothers of new-born infants in Shoreditch, 19·7 per cent. positive; mothers of new-born infants in St. Pancras, 6·6 per cent. positive; unmarried mothers in Shoreditch, 27·6 per cent. positive.

General mortality-rates are misleading, and practically no importance can be attached to them. It has been estimated that 27,000 deaths (antenatal and in first fourteen days of life) occur annually in England and Wales from venereal disease.

Experienced persons, for whose opinions I have recently asked, have estimated the amount of venereal disease among urban populations as at nearly 10 per cent. I think this is probably too low.

Investigation by various methods on bodies examined post mortem resulted, approximately, in 7 per cent. showing some signs of venereal disease.

Both in America and Germany it has been thought by competent observers that the rate of incidence on adult males in civil life was higher than in the Army (for corresponding ages). One thing, and that the most important, is quite certain; the number of cases in an actively contagious stage is infinitely greater among the civil population.

(c) CLINICS.

The question of what steps in the way of prevention should be taken among the civil population is a very vexed one. Very important steps have been taken in the provision of free treatment on the most approved lines at clinics where all the necessary blood tests and other bacteriological examinations, &c., can be carried out. The following figures are of great interest:—

CIVIL POPULATION MEASURES. CLINICS.²

Ministry of Health Report, 1920. Duration and Effects of Treatment.

	Syphilis	Gonorrhoea	Total
(1) Number of persons dealt with during 1919-20... ..	105,619	87,792	193,411
(2) Number of persons stopping—			
(a) Before completing a course of treatment... ..	30,459	28,869	59,328
(b) After treatment, but before final tests as to cure	9,350	6,481	15,831
(3) Number of persons discharged after completion of treatment and observation	8,240	13,300	21,540
(4) Number of persons who, on January 1, 1921, were under treatment and observation	47,894	28,822	76,716

¹ Report of Royal Commission on Venereal Disease, 1914-16; and *Brit. Med. Journ.*, 1916, i, p. 346.

² *Brit. Med. Journ.* October 8, 1921, p. 566.

I am informed that there has been a considerable falling off (something like 20 per cent.) in the attendance at these clinics since 1920.

(a) *Weak Points*.—One obvious weak point is that so many patients leave before they are cured. Another, less obvious, is that many prostitutes (among others) do not go at all, being afraid of losing their occupations if it should become known they were attending, and no doubt for other reasons.

(b) *Notification*.—Notification of venereal disease to medical officers of health has often been proposed, but has not been favourably reported upon.

One scheme has been proposed which seems to me to possess certain merits. It is that medical officers of clinics or private practitioners should notify medical officers of health *whenever a patient suffering from venereal disease in a contagious form is found not to be attending for the proper treatment*.

It is proposed that the medical officer of health should then communicate with the person in question, and if he or she still refused or neglected treatment that this should be made a punishable offence and dealt with accordingly. The one drawback that I can see in this scheme is that it might lead to increased numbers of persons, especially prostitutes, avoiding the clinics altogether. This point will be alluded to again.

Information as to venereal disease and the dangers of promiscuity should certainly be given to civilians. The information need not be very detailed, and though preserving to a certain extent an open mind on the matter, I am rather opposed to the film and lantern methods, whether as used in the Services or among civilians. Verbal pictures should not be painted with too black a brush. A moderate statement of the subject is sufficient, anything more tends to produce syphilophobia and similar conditions among the healthy and undue depression and despair among the diseased.

After considerable hesitation I have come to the conclusion that some information as to personal prophylaxis should be given to young men, but not to young women. This point, however, should, in my opinion, be decided by medical women. Needless to say, all instruction on these subjects should be given with the greatest care and discretion, and I should recommend the appointment of State lecturers for this purpose.

The fact that large numbers of prostitutes do not attend clinics, and that those who do frequently cease to attend before they are cured, and are often known to be pursuing their trade while in an actively contagious condition, led me to make a study of the Contagious Diseases Acts of the sixties. I began by being entirely opposed to these Acts, but have now come to the conclusion that in a modified form some measure of this description is absolutely necessary if any considerable further reduction in venereal disease is to be brought about, for there is a reservoir of venereal disease in the prostitutes of this country.

(d) CIVIL POPULATION MEASURES.

Contagious Diseases Acts.

The history of these Acts in this country is briefly as follows:—

In 1864 an Act was passed which allowed the compulsory examination of prostitutes known, or suspected for good reasons, to be diseased, and for their treatment if found to be diseased, in certain hospitals. This Act was applicable to certain districts—naval and military stations.

In 1866 this Act was repealed and another one passed, authorizing the compulsory examination of women as to whom satisfactory evidence could be obtained that they were prostitutes—if diseased they could be detained in

certain hospitals (lock hospitals provided under the Act) up to a period of six months. They could be ordered to attend periodically for medical examination—fortnightly. They could be relieved from these regulations on producing evidence that they had ceased to be common prostitutes.

Owners or occupiers of brothels could be fined £20 or imprisoned for six months, with or without hard labour, for allowing diseased prostitutes to resort to their houses for purposes of prostitution. In 1868 and 1869, the provisions of the Act of 1866 were somewhat extended.

Numerous committees inquired into the working of these Acts and practically all reported that they were beneficial.

A Royal Commission in 1871 made a remarkable report. The members said: "These Acts have purged the towns and encampments to which they have been applied of miserable creatures who were mere masses of rottenness and vehicles of disease, providing them with asylums where their sufferings could be temporarily relieved, even if their malady was beyond cure, and where their better nature was, probably for the first time, touched by human sympathy." They said they had found no instances in which the police has abused the powers entrusted to them. They found that the Acts had had a beneficial effect on the health of the men of the Army and Navy. They found that the Acts tended to deter women from becoming prostitutes, that owing to them, an increasing number of women were reclaimed, and that they had a distinctly deterrent effect on juvenile prostitution. They actually recommended the extension of the Acts throughout the country, *but* they advocated the abolition of power for compulsory periodical examination of all prostitutes. They recognized to a certain extent the value of this, but they thought that it would be unpopular in the country generally, and that its place could be taken by voluntary examination with power to detain diseased women in hospital.

A Select Committee sitting in 1882, endorsed to a large extent, the report just alluded to. They said the repeal of the Acts would result in:—

(a) Full license for venereal disease of all kinds to disseminate itself unchecked, either by police control or voluntary treatment.

(b) A serious diminution in the effective strength of the Army and Navy, which would be especially felt in the event of their services being called into requisition.

(c) The relegation of numbers of these unhappy women to the state of hopeless misery, squalor and disease, in which they lived before the system was introduced.

(d) The letting loose of increased crowds of abandoned and diseased women and girls of all ages, upon streets and thoroughfares swarming with soldiers and sailors.

The Select Committee believed that the extension of these Acts to the United Kingdom generally would be desirable and beneficial, but they did not recommend this on account of public opinion. They strongly recommended the continuance of the Acts in the districts in which they were then in operation. They advised the institution of lock hospitals in unsubjected districts for voluntary treatment. They made certain other recommendations strengthening the Acts.

The Committee were not unanimous, and a minority report recommended abolishment of compulsory examination of prostitutes, and the minority report was acted upon, and this, considered by most people the most useful feature of the Acts, was abolished in 1883.

Results of Modification and Ultimate Repeal of Acts.

The figures refer to a period of ten years during which the Acts were fully operative from 1870 to 1882 :—

<i>Fourteen Stations under Act</i>		<i>Fourteen Stations not under Act</i>	
Average strength	... 47,394	Average strength	... 19,218
Total admissions (S. 1)	... 81,105	Total admissions (S. 1)	... 29,582

A short calculation shows that more than 40,000 cases of syphilis and that more than 12,000 cases of gonorrhœa were apparently prevented by Act.

The admissions per 1,000 for the same period were :—

<i>Fourteen Stations under Act</i>		<i>Fourteen Stations not under Act</i>	
Syphilis	... 50	Syphilis	... 118
Gonorrhœa	... 84	Gonorrhœa	... 105

Constantly in hospital, same period :—

3·97

9·16

The proportions became rapidly equal when the Acts were repealed. The modification in 1883 was disastrous.

1883	1884
110	188
138	160

The Acts were less efficacious with regard to gonorrhœa than syphilis on account of the impossibility *then* existing of diagnosing gonorrhœa in women in whom discharges of other descriptions are common. The Acts were then generally found to be useless and were repealed in 1886.

I believe that these Acts did an immense amount of good, and it is probable they might have done still more good had they been persevered with and extended. I also believe that the strongest objections to them would be removed if the investigation duties were undertaken by women, and the medical examinations carried out by women doctors, though I am, of course, aware that the general feeling—the feeling of women and women doctors in particular—is opposed to this.

The argument that the Acts discriminated unfairly as between men and women, can, I believe, be rebutted ; not because I think that prostitutes are worse, morally or socially, than the men who resort to them, nor because—poor creatures—they prosecute their dreadful calling for monetary consideration (I refrain from pronouncing any moral judgments in these matters), *but because the worst consequences of venereal diseases fall on women* ; not only on the unfortunate prostitute, but on the innocent wife, who sees her own life and the lives of her children poisoned and maimed, and has no remedy. Is it not a fact that women's hospitals are crammed with the victims of gonorrhœa ? For the sake of womanhood, women should be prepared to make sacrifice, to do unpleasant work, even to risk occasional indignity.

If I were asked what class of people in this country I most revered and admired, I should say the mothers of the poorer classes. Many of the lives of these women are continuous acts of heroism and self-sacrifice. It is these women I wish to protect, both for their own sakes and for the sake of the strong sons they should bear to the Empire.

We must either end prostitution or mend it. Supposing any body of persons were to say : “ We abhor the drink traffic, we decline to lend any countenance

to it, we will not license it or regulate it or do anything which might possibly be construed into recognition of it. We will, therefore, allow any kind or quantity of drink to be sold anywhere and by anyone, provided it is not drunk in public places during daylight or in brightly lit streets at night." The parallel is almost exact.

We have accepted the fact that prostitution cannot be abolished, and, broadly speaking, we sit with folded arms. One difficulty is, of course, clandestine prostitution. I have no doubt that this would be much increased, anyhow, for a time, by any determined attempt at the closing of all brothels. On the other hand, almost all the evidence is in favour of the view that the old Contagious Diseases Acts tended to prevent or limit it.

REMARKS BY LIEUTENANT-COLONEL P. H. HENDERSON, R.A.M.C., M.B.

I have been asked by the Secretary to give my views, based on any experience I have had, on the following points:—

(1) Men deliberately acquiring venereal disease with the object of escaping service in the front line.

(2) The conditions, if any, under which men suffering from venereal disease might be made to serve in the front line.

(3) The question of maintaining discipline in hospitals for venereal diseases.

With regard to (1) I have never known of British troops deliberately acquiring venereal disease with the view to escaping service, but a good many instances occurred amongst the Cyprist and other transport drivers with certain British units in the Salonica Army. Men with gonorrhœa disposed of swabs covered with urethral discharge to their friends at various prices. Others bought condensed milk which they poured down their urethra in the hope of deceiving the medical officer, and adopted various other devices for simulating venereal disease. These men are dealt with under Section 18 of the Army Act.

(2) I do not consider that any man suffering from gonorrhœa, even in the later stages of gleet, should be returned to duty in the front line. He should be kept in hospital until cured.

Men suffering from syphilis should be kept in hospital until all physical signs have disappeared; and their return to front line, or other units, would depend on the military situation and the facilities for continuation of treatment which could be arranged at a conveniently near medical unit. Of course, in the event of urgent military necessity, venereal patients would be turned out of hospital and sent to front line units, and the best arrangements possible made for their treatment.

(3) In my opinion the chief difficulties in maintaining discipline in venereal hospitals during the war were due to the hospitals being used solely for venereal patients, and being designated "venereal hospitals." The name cast a stigma on the hospitals, and the patients disliked going to them. The patients were apt to look upon themselves as prisoners and not as patients in such places, with the natural result that they rapidly lost their self respect and became intolerant of discipline.

To my mind the solution of this problem lies in providing venereal centres as special departments of general hospitals, where the afflicted feel that they are being treated not as prisoners and criminals but as ordinary sick men.

Further, as soon as their disabilities will permit of it, venereal patients should receive as much exercise in the form of work and games, as is compatible with the efficient treatment of their disease.

DISCUSSION.

Lieutenant-Colonel P. H. HENDERSON said that those officers who were present in February when he (Lieutenant-Colonel Henderson) read a paper on this subject before the Society of Medical Officers of Health, or who might have subsequently read his paper in the *Journal of the Royal Army Medical Corps*, would appreciate how closely his views were in agreement with those of Surgeon Rear-Admiral Bett. As a matter of interest, and with the permission of Sir Alfred Blenkinsop, D.D.M.S., Eastern Command, he would give a few figures regarding gonorrhœa and syphilis which substantiated certain statistics which Surgeon Rear-Admiral Bett had given them.

EASTERN COMMAND.

Equivalent Annual Ratio per 1,000 of Strength :—

	Year	All cases	Cases contracted with their units in the command, or while on furlough, or detached from their units		
Gonorrhœa	1913 ...	24·15	Not available
	1921 ...	20·12	14·75
	1922 ...	21·48	11·83
	(9 months)				
Syphilis	1913 ...	16·3	Not available
	1921 ...	5·7	2·17
	1922 ...	6·9	2·12
	(9 months)				

It would be observed that there was a very marked reduction in the incidence of syphilis, but a comparatively slight reduction in the incidence of gonorrhœa. Now, in his opinion, medicinal prophylaxis as at present carried out, was chiefly accountable for this change in incidence—syphilis infection, being mostly extra-urethral, was more easily prevented than gonorrhœal infection which was intra-urethral. Some doubted the value of early disinfection, but how otherwise could this sudden reduction in the incidence of syphilis be explained? Although, doubtless, a slight decrease might be attributable to better facilities for recreation, a greater interest taken in the men's welfare by the officers, &c., one would expect such factors to bring about an equal reduction in the incidence of gonorrhœa and syphilis, but from the figures just given it was apparent that this was not the case. How then could the reduced incidence in syphilis be explained? The present-day soldier was better fed and better paid than the pre-war soldier, and the average recruit was more illiterate and not so well disciplined as the pre-war recruit, and these were all factors which would rather tend to an increase in venereal incidence. With the introduction of a better prophylactic outfit and more suitable instructions as to its use, gonorrhœal infection might also, to a large extent, be prevented.

He understood Surgeon Rear-Admiral Bett to say that the title of his paper was really wrong, as the incidence of venereal diseases was not greater in war than in peace and that such diseases could therefore not be looked upon as war casualties. He did not altogether agree with this view. It depended on the theatre of war in which they were serving. He (Lieutenant-Colonel Henderson) had served for about a year in Transcaucasia and there the incidence of venereal diseases was very high indeed and necessitated the conversion of three field ambulances into hospitals for venereal diseases. He therefore considered it most important that in war special arrangements should be made in advance for the reception of venereal cases and that it should not be necessary to admit them with wounded and sick to any hospital where special facilities for their treatment did not exist. Special venereal centres ought to be provided, but these should not be isolated hospitals labelled with the title "hospital for venereal diseases." They should form part of a general hospital. Every officer with experience of such matters knew of the innumerable difficulties associated with the administration of venereal disease hospitals during the late war. The stigma attached to them had lowered the self respect of the men and this had led to breaches of discipline and numerous other troubles. These troubles were avoided when venereal patients were treated, not as outcasts, but as ordinary patients, in a special department of a general hospital.

He was not absolutely sure what methods, legislative and otherwise, Surgeon Rear-Admiral Bett recommended for dealing with the venereal disease problem. Until they learned to look upon these diseases in the same light as they looked upon other important contagious diseases, and dealt with them on similar lines, not only in the Services but in the civil community, they could never expect to achieve satisfactory results. His motto was: "Prevent them if you can by the use of all legitimate means, and if you cannot prevent them, insist on the patients coming under treatment at the earliest possible moment and remaining under treatment until they receive a certificate of cure from their doctor." For the purpose of this certificate a definite standard of cure for each disease must be laid down and legislation must be introduced enforcing disciplinary action when patients failed to continue treatment until cured.

Major-General Sir W. G. MACPHERSON regretted that he was not present at the commencement of the reading of Admiral Bett's paper, but he understood from what Colonel Henderson had just said that Admiral Bett did not regard venereal disease as a war casualty. If this was so, he was in cordial agreement with Admiral Bett's opinion. With 10,000 venereal patients constantly sick in hospital and an average duration in hospital of two months, it was not difficult to see that in a strength of some two million or so the incidence, reduced to admissions per thousand of strength, would be comparatively small and in fact in no way excessive as compared with peace-time rates. Colonel Henderson had cited the incidence amongst the troops in the Caucasus as evidence of venereal disease being undoubtedly a war casualty, but it should be remembered that the incidence of venereal disease in peace time in certain localities in the East was extremely high, and, in order to prove that venereal disease was a war casualty because it was excessive in the Caucasus, one ought to compare that incidence with what would be the normal incidence of venereal disease in the same locality in time of peace. Colonel Henderson had also referred to a paper which he (Colonel Henderson) had read before the Society of Medical Officers of Health, and Sir W. G. Macpherson referred to the fact that in the discussion on that paper he (Sir W. G. Macpherson) had said practically the same as Admiral Bett, namely, that the simplest and best method of preventing the disease was to wash and be clean.

Major A. T. FROST said he thought that the present system of prophylaxis needed simplification. He had questioned men who came to hospital suffering from venereal diseases and found that nine men out of ten did not use the present packet efficiently. The ideal packet should contain in one tube antiseptics against both syphilis and gonorrhœa. Calomel cream had been proved by experiment to be efficient against syphilis and if a salt such as oxycyanide of mercury were added in a strength of one in a thousand to the calomel cream it would be an advance on the present packet, and active against both diseases. Soap, in the form of soft soap, placed inside a small swab of cotton wool would be an advantage in any form of packet as an additional safeguard.

In war time, either a general hospital or part of one, should be equipped as a venereal hospital of the first expeditionary force. Experience had shown that the medical equipment of a general hospital was not convertible for the efficient treatment of venereal diseases. A special scale of equipment should be added to army medical tables to meet the requirements of a venereal hospital.

Surgeon Rear-Admiral Sir PERCY BASSETT-SMITH said that it was a common misconception of people in civil life that the Services brought venereal diseases to the shore, whereas the opposite was almost always the case; it was the healthy blue-jacket who contracted the disease from the infected shore population. Experience of clinics in London showed how widespread was the infection on shore. Venereal diseases were infective diseases and could only be stamped out by methods used for other infective diseases, namely, notification, isolation, &c. This the public would not at present tolerate, but there was an increasing opinion that those who ceased to attend clinics before being cured, which was a common event, should be held responsible for any spread of infection and should be controlled. If this were done it would be the thin edge of the wedge for more complete notification.

Major-General Sir ALFRED BLENKINSOP said that the importance of efficient treatment of venereal disease had been referred to more than once. They could not too strongly insist that efficient treatment was the primary means of preventing the spread of venereal disease. They had heard of the large proportion of patients attending venereal disease clinics who discontinued their attendance long before a cure was established. Such people were a very grave danger to the public, and some means should be contrived to compel them to continue treatment until they had reached a standard of cure which should be definitely laid down and rigidly adhered to. He was convinced that one of the causes of the great reduction of the incidence of venereal disease in the Army was that all cases were now kept under treatment until they were definitely no longer likely to be a source of infection.

Admiral Bett referred to the influence of alcohol as a cause of infection. When he (Sir Alfred Blenkinsop) was dealing with the prevention and treatment of venereal disease in India, Lord Kitchener was Commander-in-Chief. He (Lord Kitchener) took the keenest interest in the subject, and issued instructions that freedom from venereal disease would be looked upon as a test of efficiency, and that any unit showing a high incidence would not be selected for active service. He endeavoured to obtain figures to show that men addicted to alcohol were more liable to fall victims to infection than the more temperate, and he looked forward to producing statistics in proof of this when addressing a meeting of the Army Temperance Association. He found, however, that the incidence was, if anything, higher amongst the members of the Army Temperance Association than amongst others. Whether this was due to the men who did not spend money on drink having more cash available for other purposes he (Sir Alfred Blenkinsop) was not prepared to say.

Perhaps the low incidence of venereal disease in the German and other conscript armies as compared with the voluntary armies of England and America might be accounted for by the conscripts being drawn from all classes of society, and being trained under a territorial system which did not alienate them from the influence of their families and friends and the girls they might hope to make their wives.

With regard to the conduct of venereal hospitals, venereal patients should be looked upon as sick men and not as prisoners, and every effort should be made to maintain their morale and self respect. For this reason it was advisable to treat these patients in sections of hospitals to which general cases were admitted, rather than in special institutions, at any rate in times of peace. Most of those present had had experience of the great difficulty of maintaining a proper tone and due sense of discipline in special venereal hospitals during the Great War.

Colonel Henderson had spoken of the special opportunities of acquiring disease, and of the special temptations to which soldiers and blue-jackets had been exposed in certain phases of the late war. When he (Sir Alfred Blenkinsop) was Director of Medical Services in Mesopotamia, the incidence of locally contracted venereal disease was low as compared with that on other fronts, though they were constantly importing cases from India; but when their troops advanced up the Persian road, and especially when they reached Persia itself, venereal disease became a very serious cause of inefficiency. The Persian women were, he believed, at no time remarkable for their sexual morality. When they made their advance into Persia these women were in a state of great misery and destitution, and were quite prepared to prostitute themselves in return for even a ration biscuit. Unfortunately a large proportion of them were diseased, and special precautions had to be taken to prevent them from infecting the British and Indian troops.

Sir W. G. Macpherson had urged the importance of cleanliness as a means of prevention, and with this he (Sir Alfred Blenkinsop) fully agreed. At the same time he did not think this was the only precaution which should be taken. Colonel Henderson and Major Frost were right in recommending the simplification of the preventive outfit. Permanganate of potash was particularly objectionable as a constituent of this outfit, as it rapidly became oxidized and inert in solution, and it would obviously be dangerous to supply it in crystals.

Surgeon Rear-Admiral BETT (in reply) said that he regretted that he had not had the advantage of reading Colonel Henderson's paper published in the *Journal of the*

Royal Army Medical Corps, but was glad to note a general agreement in their respective views. He fully agreed as to the desirability of dealing with venereal disease as with any other contagious disease. The measures he advocated were: (1) Modified notification, as sketched out; (2) re-introduction of measures resembling the old Contagious Diseases Acts, with modifications as mentioned; (3) an endeavour to improve general social conditions; these in addition to measures being carried out at present. The comparison was not an easy one, but he still thought that venereal disease as a war casualty did not really exceed venereal disease in peace. He agreed with Sir William Macpherson that a venereal hospital should be part of a general hospital.¹

ADDENDUM BY LIEUTENANT-COLONEL P. H. HENDERSON, R.A.M.C., M.B.

As Major-General Sir William Macpherson devoted most of his remarks to a criticism of my statement that "venereal diseases should never be lost sight of as a war casualty," and as I did not feel justified in taking up more time at the meeting, I should now, with all due respect to his great experience, like to make a short reply to his criticisms.

(1) Wherever men are congregated together away from their homes and female relatives, as is the case in war, the tendency to indulge in promiscuous intercourse when the opportunity arises, as during leave, is very much more evident than it is under peace conditions.

(2) Lax morals are much more evident amongst women during a war than they are in normal times of peace.

(3) Our troops do not normally serve in countries outside the Empire, except in places like Egypt, where we have a responsible share in the government. It is only during a war, or for some time after a war, *and as a result of the war*, that we keep troops in places like Russia, Constantinople, Germany, &c., where the incidence of venereal disease is very much higher than it normally is in peace time. Therefore the argument that the incidence of venereal disease would be as high in these countries under peace conditions as under war conditions does not arise.

(4) You cannot compare the incidence of venereal diseases in an army which is largely composed of conscripts, many of whom are married men, with the incidence of venereal diseases in our normal Army, which is a voluntary one, mostly composed of single men.

(5) The incidence of venereal diseases during war increases directly with the length of the war and the opportunities for short leave, and the increased prevalence of these diseases was greatly exercising the minds of the Army authorities towards the end of the late war.

(6) Unless the great importance of venereal disease as a war casualty is fully recognized, suitable and sufficient hospital accommodation for the efficient treatment of these cases will not be available, and they will, perforce, have to be admitted along with other sick and wounded to ordinary hospitals, which have no special venereal disease department. This unfortunate state of affairs was very much in evidence during the earlier stages of the late war.

¹ The following reply (inadvertently omitted at the meeting) should have been made to remarks of Major Frost: "Surgeon-Commander Boyden, late of the Naval Service, has made some experiments with a 'chinosol and calomel cream' which will, it is hoped, be a considerable protection against gonorrhœa as well as against syphilis. The difficulty is that the gonococcus is probably implanted on the mucous membrane of the urethra at some distance from the meatus."—W. B.

War Section.

President—Sir JOHN GOODWIN, K.C.B., C.M.G., D.S.O., A.M.S.

Physical Training in the Army.

By Colonel R. C. CAMPBELL, D.S.O.

(Inspector of Physical Training for the Army.)

(1) IMPORTANCE OF PHYSICAL TRAINING.

MUCH ground is covered by physical training, but the following paper deals only with the physical training of the soldier. In physical, as in any other form of training, science and art must work hand in hand, for science keeps art on the right lines and gives it that confidence which endows it with inspiration. I shall touch only on the more important aspects of the training from a physical and psychological point of view.

(2) THE PART TAKEN BY TRAINING IN WAR.

The wider the scope of the art of war, the more urgent is the need for study and training for its successful application. As science depends on art for its application, so does art depend on training.

Never in the history of warfare has training had so important or so responsible a part to play in the conduct of war. The example and steadying influence of veterans is no longer available for the inspiration and control of the recruit in a battle. The toll of casualties and physical breakdown is too great to allow a soldier, however staunch and stout-hearted, to be a veteran of many fights.

Young soldiers, not old soldiers, therefore form the majority in a battle. More than ever will victory depend on the efficiency of the training staff and on its ability to fill the gaps of a battle with keen and well trained men direct from the training areas. The overwhelming advantage in the future will lie with the army which possesses the most virile system of training and can most quickly replace casualties with well-trained men.

(3) EFFECT OF SCIENTIFIC PROGRESS ON WAR.

What have we learned from the study of past wars? That with the progress of science, war has advanced stage by stage from a series of tooth-and-nail combats between selected champions lasting a few minutes, to a campaign which directly involves millions, and to battles prolonged into months. We have also learned that with the advance of science the more wholesale is the destruction in war and the more harrowing is the strain on the human factor. The defeat of an army is the collapse of its human factor under the strain of war.

(4) THE HUMAN FACTOR IN WAR.

What can we learn by looking into the future? Very little. The progress of science is so rapid that even now it would be extremely difficult to say what weapons would be employed were a war to break out within a month. It would be as absurd to prophesy what weapons would be used in a war waged twenty years hence as to attempt to predict who would be the Prime Minister when war was declared.

By a logical application of the lessons learned from past wars, we arrive at a practical conclusion: that there is one never-varying factor on which all training must be based, and that is the human factor. Both weapons and methods may change, but the soldier always will be required to apply to his use and contend against those future weapons and methods whatever they may be. War will only end when the human factor can no longer bear the strain.

(5) PREPARATION OF THE HUMAN FACTOR.

The art of war, now more than ever, demands great intelligence and character in the soldier to apply the inventions of science, and great physical efficiency and character to withstand them. The demand will be increased and not decreased in the future.

An army now is the manhood of the nation launched straight into battle with very little technical preparation. The character and physical efficiency of the army will depend on national education and training, the discussion of which does not come within the scope of this paper.

The form of training which deals most directly with the human factor is physical training. What time is there available in which to prepare soldiers for the field? In the last war it was about fourteen weeks. This greatly limits the scope of physical training and confines it to two main functions: (a) that of hardening and preparing a recruit for battle and (b) that of assisting in the maintenance of the fighting efficiency of an army in the field.

(6) GRADING THE HUMAN FACTOR.

Physical training could be of the greatest value not only in building up a recruit to a requisite standard of physical efficiency, but in grading him as to the type of fighting for which he would be best suited and then preparing for the mode of fighting which he will carry out in the field.

Each arm requires a different type of man, in just the same manner as each position on a football field requires a different type of player. It would

be absurd to jumble the players in a haphazard fashion. A good back may make a poor forward and a useful forward a useless back. In the same way a good infantryman, full of dash and devil, may be hopelessly wasted as an artilleryman, and a cool, self-possessed gunner be a poor bayonet fighter. If the best recruits are selected for the "back" line of an army and the infantry gets the poorest type of recruits, it will be only a matter of time before the best of these are killed off and the army loses its "scoring" power, in the same way as a football team would lose it by deciding to sacrifice its forwards for a good back line. The push and penetration of an army depend on its infantry.

(7) VALUE OF GRADING.

The efficiency of an army would be greatly increased if recruits could be graded and selected on the lines of a football or cricket team. By means of physical training, recruits could be graded, not only as to their physical efficiency, their strength and activity, but also as to their temperament, nerve and endurance. They could be graded as to their personality and intelligence by means of games and mental tests.

It would be of the greatest assistance to, say, an infantry commander if he knew, on the arrival of a reinforcing draft, that each man would be labelled as to his physical efficiency and temperament. He would know at once which men were suited for the trench mortar section, which for the machine and Lewis gun, and which best suited to use rifle, bomb and bayonet. By this practical help he would be saved much time and disappointment and the hiatus between training area and battlefield would be considerably lessened.

(8) PURPOSE OF PHYSICAL TRAINING.

Physical training in the army must be purposeful as well as constructional. It must dovetail into, and be supplementary to, other forms of military training. During physical training a recruit should perform exercises which will make him better able to apply his technical weapons, and at the same time imbue him with the spirit essential to the successful application of those weapons. To illustrate what I mean, let us study the methods of a boxer training for a contest. His forms of physical training are purposeful and do more than make him merely physically fit. He does "road-work" to develop the endurance essential to a stubborn contest. He practises short sprints to quicken his mind and increase his speed of thought as well as his quickness of hitting. To introduce the spirit of the ring into his training he fights the sack. He throws and dodges the "medicine bag" to improve his direction and defence. In shadow fighting he stirs up and works upon his imagination and develops his initiative. Every form of training he carries out is to a purpose, which is physically and psychologically sound. So it must be with physical training.

As the boxer associates the "ring" with all forms of training which he practises, so must the battlefield be associated with the exercises performed during physical training. For instance, exercises which require dash and daring should be associated with the bayonet; those which teach a man steadiness and control, with the firing of rifle, Lewis gun or machine gun, exercises which require direction and development of the throwing muscles with bombing, digging, carrying ammunition and wheeling guns with all

heaving and leg exercises. Quickening exercises and games stimulate the brain and improve alertness and quick resource in emergencies. Self-effort is fostered by response to signals ; how to listen to and assimilate instruction, by giving all explanations and words of command in a quiet and natural voice. Physical training is complementary to the steady drill on the square. It supplies the means of reaction after a period of sustained suppression. What is the result of this ? There is a link and a mutual understanding between physical training and other forms of training. Mutual support and the team spirit are essential during training as they are during a battle. It is during training that this team spirit is inculcated and the men are taught that one weapon it dependent upon another.

Each squad, when it first parades for physical training, should be tested and then graded according to its natural average physical, temperamental and mental characteristics, and exercises should be selected which are most likely to raise the squad to the required standard of efficiency.

The tables of exercises must be arranged to suit the temperament and physique of the classes being trained. For squads in which most of the recruits are slow-witted and ponderous, the exercises should be of a stimulating nature. In the squads composed mostly of sharp-witted and highly strung city men they should be of a restraining type and teach steadiness and self-control.

(9) CONTINUITY OF PHYSICAL TRAINING.

It is absolutely essential that physical training must be continuous and be kept up throughout the whole of a soldier's service. To ensure this the instruction of trained soldiers in physical training must not be confined to experts. Every commander and leader should understand the value of physical training and its practical application.

During peace training, soldiers should be classified annually as to their physical efficiency, as they are in musketry. For trained soldiers physical training should include such pastimes as boxing, wrestling, swimming, obstacle training, gymnastics and some practical team game such as basket ball. All of these have a direct military value but require careful instruction when taught to beginners.

(10) PHYSICAL TRAINING AND CONVALESCENTS.

Equally important and valuable work can be done by means of physical training in salvaging convalescents. The morale of a man from hospital is generally sub-normal. While a patient in hospital he is treated with every care, everything is done for him, his life is effortless so far as he is concerned. He is soft, morally and mentally, as well as physically. He requires special care before he returns to his unit. His will, as well as his body, must be strengthened. His training should be attractive and gradual. His physical training should commence with games and quickening exercises, which create an interest and stimulate self-effort. From these the training should progress towards exercises of self-control and sustained effort and the more strenuous British games and sports.

Although physical training is a sympathetic link between the hospital and the battlefield, yet it must be in no way associated with the hospital and looked upon as a remedial form of training, or the spirit of the training would be

killed. It would be as demoralizing to associate the gymnasium with the hospital, as it would be to associate the hospital with the cemetery. Physical training in the army is a creative training which deals with a man who is passed as organically sound. It is on this hypothesis that it is based and carried out.

(11) PHYSICAL TRAINING IN PEACE AND WAR.

In peace, the physical training of a recruit coincides with other forms of training in the depot and lasts about sixteen weeks; the course will consist of eight tables or series of exercises. The tables are progressively arranged, the progress being more gradual in the early tables than the later ones.

In order to arrive at concrete results and see monthly the effects of the training, practical physical training tests are being carried out with recruits in all commands. The charts I have brought show the results of the tests which were carried out during October and November, during which time over 11,000 recruits were tested.

In the event of war, the same tables would be employed, the only difference being that no exercises which require apparatus would be used. War training will be peace training shorn of its adornments; this will enable the recruit's course of training to be completed in about twelve weeks instead of sixteen, and to be carried out in any training centre.

(12) PHYSICAL TRAINING AND MOBILIZATION.

During the first few weeks of mobilization, the training camps are deluged with enthusiasts, eager for battle, and so unused to restraint or military discipline that it is practically impossible to carry out any form of technical training. Instructors are scarce, weapons and uniform are not available, the small and harassed staff experiences a most difficult and trying time. It is during this period that physical training can be most helpful, as it is a sympathetic form of training understood alike by civilians and soldiers. No arms, apparatus or uniform are needed. It can be made the means of organizing a mass into military formation, accustoming it to restraint and discipline and inculcating soldierly thought and ideas. It is a most useful leading rein from civilian life into the army.

(13) IMPERATIVE NEED OF LARGER PHYSICAL TRAINING STAFF.

On mobilization, the physical training staff will be increased to cope with the influx of recruits: it is for this purpose that physical training staff reserve is required.

The situation will be considerably helped by having a physical training staff instructor attached to the permanent staff of each Territorial Force unit, and a non-commissioned officer trained as an assistant instructor during peace for each company, or its equivalent, in the Territorial Force.

The establishment of the physical training staff at the end of the last war was 2,200, or more than twenty times the strength required for the training of the army during peace.

(14) CREATION OF PHYSICAL TRAINING INSTRUCTORS.

The training of assistant and staff instructors is centred at the Army School of Physical Training at Aldershot.

The assistant instructors are selected from the N.C.O.'s who reach a high standard of physical efficiency during their recruit's course of physical training and who have continued to show aptitude for the work. These selected N.C.O.'s undergo a short preliminary course in their commands before assembling at the Army School of Physical Training.

The preliminary courses, besides preparing the N.C.O.'s for the longer and more strenuous course at Aldershot, also act as a sieve by means of which those not likely to make efficient instructors are eliminated. The N.C.O.'s who qualify at Aldershot return to their units and are employed under staff instructors training recruits. During this probationary period they are closely supervised and helped by the staff instructors. When proficient as trainers the second portion of their certificate is endorsed. They then become fully qualified assistant instructors and gain the distinction of wearing the "crossed swords" on the right arm.

Those assistant instructors, who show special keenness for the work and prove that they have character and the knack of being able to impart instruction, are selected to go through an advanced course of physical training. At this course, in addition to physical training, subjects of a recreational nature are included in the syllabus, so that when the N.C.O. returns to his unit he is able to assist in the organization of games and sports and is capable of training the regimental officers and N.C.O.'s to become instructors in the trained soldiers' physical training exercises.

As the chief duty is to train and teach teachers, the physical training staff is selected only from those who have passed through and attained a high degree of proficiency in the advanced course. The greatest care is taken in the selection of the staff, only those of the highest integrity, as well as of the highest proficiency, being considered.

To train and supply a sufficient number of physical training instructors for units during war, there should be a school in each command to train assistant instructors, while the Army School of Physical Training should be used solely for training staff instructors for all the Forces of the Empire.

(15) THE CO-OPERATION WITH CIVIL BOARDS AND ASSOCIATIONS.

Physical training staff instructors during war would be recruited from the teachers of physical training trained by the Board of Education and from athletes of prestige and personality. The majority of the excellent physical training staff instructors during the last war were obtained from this source. For this reason alone the army physical training staff should work in sympathy with, and keep in the closest touch with, the Board of Education and the different amateur sports and games associations. One means of realizing these aims would consist in having a universal system of physical training throughout the Empire. The Army is about to adopt the Board of Education terminology in physical training, and it is hoped that the Dominions will do likewise. The advantages of having one system of physical training throughout the Empire are many and far-reaching, both from an Imperial and from a training point of view.

The physical training staffs of the Royal Navy and Army work together in the closest co-operation. Officers from one Service undergo courses held by the other Service. An interchange of lecturers is arranged and technical points are brought up and discussed at joint conference. This unity and interchange of ideas in a common cause has been of great assistance to the army, and has done much to stimulate progress in physical training; and it is a policy which should be extended to include the whole Empire.

DISCUSSION.

Surgeon Captain A. GASKELL, R.N., asked three questions: The first and most important was: How could the curse of charlatanisin be removed from everything connected with physical training? The science was one on which every Tom, Dick or Harry felt competent to speak and it was common knowledge that many people took it up simply because it proved easy and lucrative. Just as horse-racing was said to pollute the honour of every one who touched it, so physical training seemed in danger of corrupting many otherwise honest men. Logically the foundation of physical training was physiology and to physiologists they should turn for instructions, but medical men should be continually supervising, lest any irregularity crept in. The athletic medical man was the right person to direct physical training, but even he must be watched lest he take up this deplorable charlatan clap-trap. He (Surgeon Captain Gaskell) referred to an article which appeared in the Blue Book, "The Health of the Navy for 1905." The second question was: Had any method been devised for estimating the amount of "guts" in a recruit? Physical measurements were all very well but the Great War had shown us that fine physique was not necessarily a token that a man would "stick out" all sorts of privations and dangers. The third question was: Had any new tests been devised for recruits on the lines of those introduced by the Air Force? These tests had been introduced to discover the men whose nervous equilibrium might be so unstable as to lead to accidents. Thus these men could be invalided before the accidents instead of afterwards.

Surgeon Commander K. DIGBY BELL, R.N. (of the Royal Naval School of Physical Training) pointed out what a unique situation this was for medical officers of the War Section of this Society to be addressed on a subject, which was, after all, a branch of preventive medicine, by a Colonel with no medical training. They were indeed fortunate to have had this opportunity, and of hearing in particular of the good work carried on at the headquarters of physical training in the Army. Colonel Campbell had dealt with his subject almost too briefly, but yet, what he had told them was full of lessons for them all. Colonel Campbell had explained the *continuity* of the training which was conducted in the Army—a state of affairs for which those in the physical training branch of the Navy envied. Continuity in teaching and of work at training establishments was the essential factor for obtaining satisfactory results, and with the practice of changing round appointments every two years or so, as carried out in the physical training branch of the Navy, this continuity was handicapped very seriously. He (Dr. Digby Bell) drew special attention to another remark made by the lecturer: that physical training was a sympathetic link between the Services. The truth of this had been fully realized at the Royal Naval School of Physical Training. Physical training went far further—it was a sympathetic link between the Mother Country and the Colonies (who met on a common ground in all sports and recreations): it was a very real sympathetic link between all physical trainers and the medical profession (who met each other on the common ground of public health and eugenics). Indeed—as had been so often pointed out—if the whole subject of physical training could only be governed and protected by a properly appointed Central Board of Control, like the medical profession had to-day, it would be of vast benefit to the health and

morale of the whole nation. When degrees for the practice of physical training were granted by Universities on the same lines as medical degrees, then no doubt the taint of charlatanism, to which Surgeon Captain Gaskell had just drawn attention, could no longer be associated with this noble profession.

Lieutenant-Colonel BOYLAN SMITH stated that he had had difficulty in getting physical training instructors to take interest in the physical training of men in a convalescent depot in France who were suffering from slight disabilities, but who were well able to undergo a graduated course of exercises, though unfit for the strenuous full course. He believed that the need for training such men would always arise in any war in which convalescent depots operated as they did in the late war. These men were not disabled enough for orthopædic hospitals and they were too fit to remain in general hospitals. Although they were expected to pass out of convalescent depots to their reinforcement camps as fit fighting men, the physical training instructors in many cases were uninterested in their physical development. The minds of the physical training instructors should be disabused of the notion that these were "medical cases." Lastly, how did Colonel Campbell account for the rise in efficiency immediately succeeding the drop shown on all the wall charts exhibited?

Lieutenant-Colonel P. H. HENDERSON said that he would touch upon two points in the discussion: First, he cordially welcomed Colonel Campbell's announcement that the Board of Education was about to introduce a universal system of physical training for boys throughout the United Kingdom, that the system of training in the Army would be identical with that of the Board of Education, and that there was a hope that this system might soon become universal throughout the Empire. It was obvious that such a training would greatly increase the physical capacity of the nation, and the benefit to the Empire would be inestimable. This training should be particularly valuable in assisting recruiting officers to obtain suitable recruits for the Army. Most of them knew how difficult it was at the present time to get suitable recruits. Large numbers were now rejected because they were not up to the standards considered necessary for the Army; others, in considerable numbers, who were passed fit had subsequently to be discharged because, under training, they failed to acquire the physical capacity necessary for the Army. This was not only bad for the Army but bad for the taxpayer. After a considerable experience he could not help thinking that the present standards adopted for recruits were not altogether satisfactory and his suggestion was that if on leaving school the physical and mental capacity of every boy were to be recorded on an index card in a way similar to that in which the condition of his organs, teeth, &c., were recorded on his health index cards, it would be of the greatest assistance to recruiting officers and medical examiners of recruits in selecting men for the Army. Convenient formulæ for recording the physical and mental capacity could easily be devised and he (Colonel Henderson) hoped Colonel Campbell would use his influence in order to have a satisfactory system universally introduced.

His second point had reference to the peculiar dull white complexion which was such a common characteristic of highly trained instructors of the physical training staff. Had any physiologist ever satisfactorily explained the cause of this peculiar pallor? Colonel Campbell thought it was due to the indoor nature of their employment, but this explanation hardly satisfied him (Colonel Henderson). Their pallor was quite different from the anæmic complexion associated with certain factory workers and others who spent long hours indoors in unhealthy atmospheres. Highly trained long-distance runners and boxers who might do most of their training out of doors also developed the same peculiar pallor and he (Colonel Henderson) suggested that it was possibly associated with some alteration in the heart and blood-vessels but he did not suggest that the condition was necessarily an injurious one.

Air-Commodore D. MUNRO, C.I.E., referred to Colonel Campbell's plea that physical training should be largely in the hands of physiologists; he stated that in the

Royal Air Force they had successfully applied to the control and assessment of results of physical training certain physiological tests which had been initially designed for the determination of physical fitness for flying duties in officers; that these tests had been applied both before and after a course of physical training and that, at the latter stage, they accurately recorded the resulting degree of improvement in physique. He felt that on the grounds of co-operation towards a standardized system of physical training throughout the Empire, as proposed by the author of the paper and subsequent speakers, there was ample indication that these Royal Air Force tests could be introduced into the scheme with considerable benefit as a scientific method of recording the results of training and the standard of physical fitness of individuals at any given moment.

Colonel C. H. MELVILLE asked if it might not be possible to increase the number of hours given to physical training to two per day during the first month of training of recruits. Physical training was much liked by the recruits and the more they had of it and the less of barrack-square drill, the more gradual and pleasant was the change from civil to military life. He asked whether the supply of gymnasium kit could not be made a regular issue. It would be of great assistance in ensuring cleanly habits. It was impossible for a man to work well in serge clothing.

Major N. V. LOTHIAN, M.C., bore testimony to the very great value of the existing method of physical training, as outlined so clearly by Colonel Campbell, and stated that as a medical officer, and one who had had the good fortune to pass through the Instructor's Course at the Central Gymnasium, he fully supported Colonel Campbell's claims, not merely as to the physical results of the existing system, but also as to the associated effects on the psyche. He would raise a question however on one point to which Colonel Campbell had referred, namely, the need for *continuity* in training. It seemed that some better organization was required to ensure continuity of physical training among trained soldiers. The subject was sufficiently safeguarded while the soldier was a recruit, and afterwards to some extent also while he was in his depot; but once he was posted to his unit, it was very questionable if he got sufficient physical training to maintain those qualities of speed and agility, quick response and endurance which, as a six months' recruit, he undoubtedly acquired. Many regimental personnel—and a large proportion of non-regimental personnel in administrative corps—never did any more physical training after leaving their depot. The physical condition of such men, more especially the clerks, cooks, and storemen, could hardly be perfectly fit on mobilization; yet all had to march together. The retreat from Mons—very marvellous effort as it was, averaging some 16 miles per day—showed how, despite the best will in the world, and even allowing for the inevitable dilution with reservists, a proportion of casualties occurred through men falling out physically exhausted and unable to struggle further. It would appear that the condition of troops, who might at any time have to take the field, might well be improved as a whole if physical training were continued throughout a man's service. The question of reservists was, of course, a larger issue.

Lieutenant-Colonel C. R. SYLVESTER BRADLEY said that, speaking as a medical officer, interested more particularly in the physique and training of the recruit, there were one or two questions on which he would be glad if Colonel Campbell would give him the benefit of his experience. His first question dealt with physical standards. He stated that the physical standards for the recruit on enlistment were fixed presumably with the view to providing, among other things, the most suitable material from which to build the trained soldier; but had they any facts that would justify the assumption that physical capacity (in other words the capacity of the individual to perform a definite amount of work) could be correlated to physique? Had Colonel Campbell any information as to the physical capacity of the average untrained recruit to run a hundred yards, or to high jump for instance? To what extent was this capacity

improved by physical training, and could this capacity be shown to be related to physique?

Another question he would ask Colonel Campbell referred to the psychological aspect of physical training. The number of recruits who were discharged from the Army for defective intelligence whilst at their training was so large that some effort was needed to eliminate these ineffectives either on enlistment or before any large amount of money had been expended on their training. The ideal arrangement would be for all recruits to be mentally tested by some approved method before enlistment, but the obstacles to such a course were very great, not one of the least being the lack of trained personnel to carry out these tests; and it had occurred to him that the solution of the question could be found, if the estimation of a recruit's mental capacity were carried out on his arrival at the depot by the physical training staff, in conjunction with the educational and medical branches, some modification of the Binet-Simon method being used. A short time ago, some initial experiments on these lines had been carried out under his direction, and the results were most encouraging, and would appear to justify further investigation.

His last question referred to the remedial side of physical training. His experience had been that Army physical training instructors were rather inclined to look upon physical training solely as a means of developing the "fit" to the exclusion of the unfit: but he would point out that no man was perfect, either morally, mentally or physically. Fitness was only a matter of degree, and remedial exercises for such minor defects as slight flat-feet, scoliosis, deficient chest excursion, &c., were worthy of more care and attention than they were receiving at the present time.

Colonel R. C. CAMPBELL (in reply):—

(I) *To Surgeon Captain Gaskell, R.N.*

(1) Charlatanism, without a doubt, often is associated with physical training, but it is generally in connexion with its medical or curative side. Professional strong men and others advertise extravagant claims as to the number of diseases they can cure, and working upon the minds of the hypochondriacal, carry on a lucrative business. On the other hand they may do some amount of good if only they can stimulate their flabby-minded patients to carry out any form of exercise which requires self-effort and perseverance. The physical training carried out in the Services is based on the Swedish system—a system which has been in vogue for over a hundred years, and has been proved scientifically to be sound and without the slightest inherent taint of charlatanism. If the principles of this system of physical training are observed there is small danger of any harmful irregularity creeping in. There is a medical officer "hygiene specialist" on the staff of the Army School of Physical Training. His knowledge is most helpful in testing scientifically what had often to be tested empirically with much labour and loss of time.

(2) Those in charge of physical training should have a sound knowledge of physiology, they should be athletic and have a practical knowledge of the work, but, more important still, they should possess a full appreciation of the psychological aspects of physical training. The chief value of any training is psychological, and in no training does this apply more forcibly than in physical training. The goal is the mind, which must be reached by means of the body. Every physical expression has a psychic equivalent, the mind is the creative and dynamic force which inspires and compels all the movements of the body. The character and mentality of the recruit is shown in the manner in which he performs his exercises. Physical training is subjective, not objective, as in drill, during which movements are often performed subconsciously. An instructor may be a renowned athlete and have a thorough knowledge of physiology, but unless he has force of character and personality behind his prestige and knowledge he will never reach the goal to which he is striving—the mind and soul of the recruit. An instructor without character will, in a very short time, be appraised by a class of

recruits. He will be found to be a sham. To my mind, he is the real charlatan in physical training.

(3) Tests are carried out with recruits in the Army; they include tests of endurance, speed, strength, agility, dash and control. I trust these will be found to have a practical value, and be suitable and helpful to the Army as the tests of nervous equilibrium are to the Air Force.

(II) *To Lieutenant-Colonel Boylan Smith.*

(1) Physical training instructors should certainly take an interest in and be able to train all types of men at convalescent depots. All convalescents should be graded according to their physical fitness, whether they are normal or subnormal. Those who are normal and suffering from minor disabilities should be classed together and given suitable graduated exercises and games. With the subnormal the training should progress towards the normal as the disabilities disappear. It is by mixing fit men with men who are not fit that the latter get neglected and become disheartened. The first day a recruit attends for physical training his natural abilities and peculiarities are studied in a series of tests and noted down in the instructor's squad book. These observations, together with those of the medical officer, diagnose fairly accurately the mental, temperamental, and physical condition of each recruit. This method of studying each recruit before commencing his physical training would also apply to the physical training of convalescents, who could be diagnosed and classified, and be given graded exercises according to their classification. I was fortunate enough to see the physical training energetically carried out at the convalescent depot in question, when it was under both Colonel Cowey and Colonel Boylan Smith.

(2) As yet it is difficult to account for the rise after the fall, shown on the charts. I think that the fall was due to recruits having been taken for musketry during their physical training course, and the rise was the result of the resumption after the hiatus.

(III) *To Lieutenant-Colonel Henderson.*

(1) The Army has decided to come into line with the Board of Education, and to adopt its terminology, and as the Army caters for recruits and soldiers from the age of 18 upwards, it is hoped that by this means the recruit will find in physical training a sympathetic link between his youthful life and his training in the Army. If the scheme can be extended throughout the Empire a bond will be formed which will help to keep all the Dominions in sympathetic touch with the Mother Country and with one another.

(2) I agree with Colonel Henderson that it would be an excellent thing if at school boys and girls had to pass standards of physical efficiency as well as educational standards. A boy who achieves Standard VII in education, but is only Standard I physically, is a greater menace to the State than a boy who is Standard I in education but Standard VII physically. Such a scheme would have a far-reaching effect on the the national health and life generally, and be of the greatest national value, not to mention the assistance it would be to the Services in grading and classifying recruits.

(3) With regard to the peculiar dull-white complexion of staff instructors. I had put it down to the fact that practically all their work is carried out indoors. In France, where the work was carried out in the open air, the instructors were bronzed and sunburnt. Our war diarist in France, in August, 1918, writing of the outdoor training, said: "From the same cause (the sunshine) the instructors turned nearly black, and were hardly distinguishable from the brown-skinned coolies who at times were employed in constructional work about the camp and assault course." The ultimate effect is illustrated by the aspect of a healthy old gentleman of 79 years—Major J. C——, —who may be seen daily taking long walks around Aldershot. He is ruddy of face and bright of eye. Back in the sixties he was the first Sergeant-Major of the Army

Gymnastic Staff. The clear eye and the general mien of the staff instructors, proves, whatever their pallor, that they are fit and in hard condition. I had never associated the white skin with any alteration in the blood-pressure.

(IV) *To Commodore Munro.*

In my paper I advocated the assistance and co-operation of the physiologist, but the point I wanted to make was that the chief factor in determining the purpose of the training was psychological. Each Service requires a different type of man, and a different type of training. A Rugby footballer forward requires different training from a boxer. Individual boxers themselves differ, and require different handling. Methods suitable for a hulking ponderous heavy-weight would not be suitable for a "live-wire" bantam-weight. The anatomy of Joe Beckett is the same as that of Jimmy Wilde; but the character and temperament of each are completely different. The successful trainer is the one who can understand the psychological aspect of his form of training and apply it to the mind of the man he is training. It would be of the greatest interest to know how the Romans trained their recruits, most of whom, even when Rome was at the height of her power, must have come from the slums of her cities. The campaigns which they succeeded in accomplishing called for the severest tests of endurance and physical hardihood. The knowledge of physiology in the times of the Romans was very elementary. To make up for this they must have been wonderful psychologists, if I may be permitted to use this term. I agree with Commodore Munro that it would be of the greatest assistance if there were a close co-operation between the Services, and if the tests carried out by each Service were passed round to the others, with a view to establishing a standardized system of training and tests for the forces of the Empire.

(V) *To Colonel Melville.*

(1) An hour a day for five days in the week is sufficient time for the physical training of a recruit now that he has an increased number of subjects to learn. A recruit's syllabus of training should be so co-ordinated that one form of training reacts on and assists another. For example, I consider that physical training is the natural reaction to drill on the square and that one helps the other.

(2) Steady drill on the square is essential, to instil into the recruit the unhesitating obedience to orders, and the habit of working with his comrades, which are the basic qualities in a soldier. I agree that the first month on the square is extremely tiring and a great strain upon the nervous system of a recruit, but to what extent this training can be lessened it would be hard to say. I would not advocate more physical training.

(3) Shorts and vests are now worn by all recruits during physical training. At some depots, recruits bring towels to the gymnasium and have a rub down after work. In suitable weather I advocate that recruits should do their training stripped to the waist, and allow the sun and air to act upon their skin. This, together with the rub down after their work, would have a healthful and stimulating action upon their skin. Nothing can be more insanitary and contrary to all principles of hygiene than to allow soldiers to wear the same thick underclothing, often sweat-laden, day and night for a week without changing. It is a great blessing that recruits do change their clothes at least once a day, if only for an hour, during physical training. Perhaps in time some form of sleeping suit will be provided for the soldier. I believe we have to thank Colonel Melville for being one of the pioneers who obtained suitable kit for recruits in which to do their physical training.

(VI) *To Major Lothian.*

(1) I agree that it is essential to have continuity in physical training. The importance of this is realized by many units which carry out physical training daily under regimental arrangements. I trust it will be only a matter of time for all soldiers to be classified annually according to their physical efficiency, as they are in musketry. Such a

regulation would ensure that clerks, cooks and storemen, passed a standard of fitness at least once a year.

(2) The stifling weather and heavy packs, together with the trying nature of the whole situation, I consider were the reasons why so many fell out during the retreat from Mons. The reservists were soft, especially their feet, which could not bear the added weight of the pack. This was specially noticeable when marching along *pavé* roads.

(3) I agree, again, that a fighting force should be kept fighting fit. There is a great danger of judging the efficiency of an army by its man power, instead of by its fighting power.

(VII) *To Lieutenant-Colonel Sylvester-Bradley.*

(1) Every recruit, when he first parades for physical training, is tested by means of natural activities as to his agility, strength, control, and intelligence, in order to gauge his natural abilities. His first field test is carried out after he has completed about twenty attendances and it is continued monthly until he completes his course of physical training. The tests are still in the experimental stage, but they include tests of speed, endurance, agility, dash, control and strength. The record of these tests was only commenced in October, 1922. Up to now 11,000 recruits have been tested and the results recorded in the form of graphs as shown to the meeting. By means of these tests we hope to follow the improvement, or any fluctuations, during the course of physical training. Tests are also a concrete means of letting a recruit observe the value of the training and the results of his efforts.

(2) I quite agree that all recruits should be tested mentally, but I suggest that the tests should be carried out by the Educational Corps which gets recruits daily while they are at the depot.

(3) All observations made by the medical officer are noted in the instructor's squad book. It is found that most of the minor defects automatically correct themselves without any special treatment. There are cases which require special treatment, but during my four years as Superintendent of Physical Training in the Southern Command, these were extremely few. As a rule, slight "flat" foot is caused by weak supporting ankle muscles. These become strengthened as the training advances. Scoliosis, when caused by one leg being shorter than the other, or by a defective pelvis, is difficult to correct by means of the ordinary exercises, but scoliosis, when merely the result of one-sided vocational occupation becomes automatically righted in a very short time. Work in the open air, regular meals and exercise increase the size and movement of the chest by natural means. The unfit in a class should receive special care and such encouragement as will induce them to attend voluntary work in the evenings (at which an instructor is always present) and be shown special exercises which they can carry out "on their own." I found this system better than grouping the defectives and giving them special exercises, the moral effect of which is not good and more than counterbalances the physical benefit derived from the exercises. All recruits should pass the "standard" test by the time they complete their physical training. The names of those who fail to do this are submitted to the superintendent of physical training of the command. This, I hope, will ensure that no weak and unfit recruit is allowed to slip through the "mesh" of physical training. In connexion with the treatment of recruits who are suffering from slight defects we get the willing support and able assistance of medical officers at depots.

Venereal Disease as a War Casualty.**CORRIGENDUM.**

IN the discussion on Surgeon Rear-Admiral Bett's paper on "Venereal Disease as a War Casualty," published in the February number of the *Proceedings*, an addendum by Lieutenant-Colonel Henderson appeared,¹ criticizing the remarks made by Sir William Macpherson during the discussion. It is only fair to Sir William Macpherson, therefore, to note in reply that his remarks were intended solely to support Admiral Bett's view that venereal disease could not be regarded as a war casualty, and that any exaggeration of the "war casualty" point of view, so strongly urged by Colonel Henderson, diminished the importance of dealing with venereal disease in time of peace. It was, therefore, with the object of urging the need of combating venereal disease in time of peace, and regarding it as a peace casualty more than as a war casualty, that Sir William Macpherson's remarks were made, and in making them he could not avoid referring to Colonel Henderson's statements, which had been made just before he rose to speak and which were opposed to the views that venereal disease was not necessarily or specially a war casualty.

¹ *Proceedings*, 1922-23, xvi (War Sect.), p. 29.

War Section.

President—Sir JOHN GOODWIN, K.C.B., C.M.G., D.S.O., A.M.S.

The Effect of Tropical Climate on Physical and Mental Efficiency.

By Squadron-Leader T. S. RIPPON, R.A.F. Medical Service.

(I) PRELIMINARY INVESTIGATIONS INTO VISUAL FATIGUE IN INDIA.

DURING the course of my first year's work in India, I noticed that there was a tendency for pilots, who, in Europe, had not had any difficulty in this respect, to make bumpy landings. Moreover, when tested with the "orthotelemeter" they showed evidence of a lowered degree of binocular stereoscopic vision, and with Bishop Harman's diaphragm test, they showed a wider "ocular poise," indicating inability to maintain an even ocular muscle balance, which they did not show in England.

I inferred that these signs pointed to some degree of visual fatigue, and in October, 1920, I discussed the question with Flight-Lieutenant P. C. Livingston, R.A.F.M.S., who was in medical charge at Ambala, and found that he had noticed similar results. We then agreed to work together, and all officers and men who came to him complaining of discomfort due to glare were taken to the Medical Research Laboratory, and we examined them together. The following examples are briefly recorded:—

Case I.—Flying-Officer W., aged 27. Hours flying 450. Iris blue. Vision: Right, $\frac{3}{4}$; left, $\frac{3}{8}$. Two diopters of manifest facultative hypermetropia. Conjunctivæ healthy. States that he has a tendency to "flatten out" too soon, which he puts down to glare. He was not sure where the ground was, so he flattened out early to avoid flying into the ground. In March, his eyesight was normal, but later, as the discomfort from the glare got worse, his landings deteriorated. He suffered from headaches and aching eyes. He had to blink and to strain in order to see objects.

Case II.—A.C./2 E., aged 21, mechanical transport driver. Complains of frontal headaches whilst driving in the sun. After he returns from duty he has difficulty in reading, and says "the print looks out of focus," and a headache follows. At these times his eyes water. Has photophobia but no conjunctivitis. Cannot see as clearly at night as in England, a most important symptom, and had an accident last year whilst driving a car at night, when he failed to perceive the road whilst turning a corner and ran through a fence.

Case III.—A.C./2 W., aged 20. Complains of dimness of vision after reading and writing which has come on since arrival in India. Dull pain over eyes.

Case IV.—L.A.C./M., aged 26, mechanical transport driver. "Used to drive in France without lights, but cannot do so in India as he cannot see the road." (This came on after six months residence in the country.)

Case V.—A.C./1 S., aged 20. Complains that three months ago pain began in back of eyes and frontal area. When reading, the letters often get blurred and run into

each other. When facing the glare everything he sees becomes blurred, but his vision improves within about half a minute. Sees yellow dots floating before his eyes.

During April we examined thirteen cases of discomfort due to glare, and twelve cases during May and June.

SYMPTOMS.

Inspection of the symptoms of which the patients complained showed that they could be roughly classified into three groups:—

(1) *Superficial Irritative Group*.—The most prominent symptoms were conjunctivitis, blepharitis, lacrymation and frontal headache.

(2) *Retinal Fatigue Group*.—Symptoms of dimness of vision at night or dusk, prolonged after-images and slow adaptation.

(3) Lastly we have cases in which the patient complains of inability to sustain accommodative efforts or convergence for near objects for any length of time. Lacrymation, headache, mental fatigue and frowning are noticed.

These cases form the majority of the subjects who complained of glare. They are very similar to those cases described by Donders as asthenopia, in his report on "Anomalies of Accommodation and Refraction," issued by the Sydenham Society, in 1864.

ACCOMMODATIVE ASTHENOPIA.

To the best of my knowledge it was Donders [1] who first described asthenopia in detail, as follows: He said that though the power of vision in those affected with asthenopia was usually acute, nevertheless during reading or writing or other close application of the eyes in indistinct light, objects became blurred or confused and a feeling of fatigue and tension came on, in and above the eyes.

Cause.—Donders believed the cause to be a moderate degree of hypermetropia. The exciting circumstance, however, should not be confounded with the cause, and the exciting condition for the occurrence of asthenopia was the presence of insufficient range of accommodation.

Lieutenant-Colonel Maynard, I.M.S., late Ophthalmic Surgeon to Calcutta Medical College, in his "Manual of Ophthalmic Practice" [2], states that accommodative insufficiency is very common in the tropics, where the climate of the plains is so enervating. He considers it to be due to weakness of the ciliary muscle, and states that it is preceded by illness, all the toxic conditions, neurasthenia, and so on.

INVESTIGATIONS OF AMPLITUDE OF ACCOMMODATION IN CASES COMPLAINING OF EFFECTS OF GLARE.

(1) *Definition of Amplitude of Accommodation*.—The quantity of accommodation possessed is termed the amplitude. It is the dynamic force necessary to convert the eye from the refractive condition at the far point to that required for vision at the near point. The far point is the most distant point at which clear vision is possible, and in emmetropia is at infinity. The near point is the nearest point at which clear vision is possible. If the emmetrope reads clearly at 10 cm. we know that he must be exerting 100/10 diopters of accommodation (10 diopters = the amplitude). Fine print, or a coarse hair on a white background, is approached towards the eye, and the nearest point

at which it is seen clearly is measured. Owing to changes in the lens, the near point recedes as we grow older, and the amplitude at thirty years is about half what it was at the tenth year. The curve of amplitude worked out by Donders is still found in all text-books of physiology and ophthalmology and is the one we used as the standard in our investigations.

(2) *Method of estimating Amplitude of Accommodation by the Optometer.*—After experimenting with various types of optometers we settled on the "orthops" pointer, which is described in "Visual Optics" by Lawrence [6]. This consists of a flat wooden bar scaled in centimetres and diopters showing the corresponding age opposite each figure, and with a sliding carrier which holds a card with printed words which is used to focus on.

We estimated the refraction subjectively and allowed for any myopia or apparent hypermetropia. Then the closest point of distinct vision was obtained by running the carrier near to the eye until the print became blurred, and then slowly moving it away until it became quite clear and sharp. The examiner covers one eye with a card, and tests each eye separately.

RESULTS.

The results of our investigations into the association between insufficient "range of accommodation" (as Donders puts it) and asthenopia is shown in the attached diagrams.

Series 1 (glare cases), fig. 1.—Shows twelve cases complaining of symptoms due to glare. The vertical figures on the left indicate the amplitude of accommodation in diopters—and the curve represents Donders' figures and shows the average power of accommodation at different ages. In Series 1 there were twelve cases, each eye was tested separately and plotted on the graph on the line corresponding to the correct age. By comparing the distance of the dots from the normal curve of Donders, we see the amount of the insufficiency of accommodation. Thus we notice in the first series, that twenty-two out of twenty-four tests are below the curve.

In *Series 2* (glare cases) (fig. 2), twenty-three out of twenty-four dots are below the curve and the bulk of the dots indicates a slightly greater insufficiency than Series 1, which was taken a month before. These cases were not suffering from any disease, or the effects of any disease which might have caused a general loss of tone and consequent deterioration of the ciliary muscle. The only condition found by Flight-Lieutenant Livingston and myself was that of "effects of glare." The number of cases examined is not enough for us to venture to state any figure indicating the percentage of fatigue of the power of accommodation, but if we were to draw a circle round the largest group in each series, the centre of the circle would give us a figure, indicating that the power of accommodation was about half what it ought to be. My own practice is to grade cases as "marked fatigue" when the amplitude is 30 per cent. less than normal.

The *Control Series* (1 and 2) consisted of all the officers available on the station. There was no selection of any kind. All had been exposed to glare for an average of nineteen months. None of them had complained of glare sufficiently to apply for medical relief, but when asked if they had noticed discomfort from glare there were four individuals in Series 1 and two in Series 2 who replied in the affirmative. Inspection of the results shows that the eye condition of the controls varied considerably, and so the result was a purely negative one, though very useful. Thus we notice that whilst the controls are

roughly grouped round the normal curve, there are some individuals who are above normal and others below. The four persons who complained of symptoms of glare were all suffering from marked fatigue of accommodation. Working out the degree of insufficiency of accommodation in percentage gave a rough

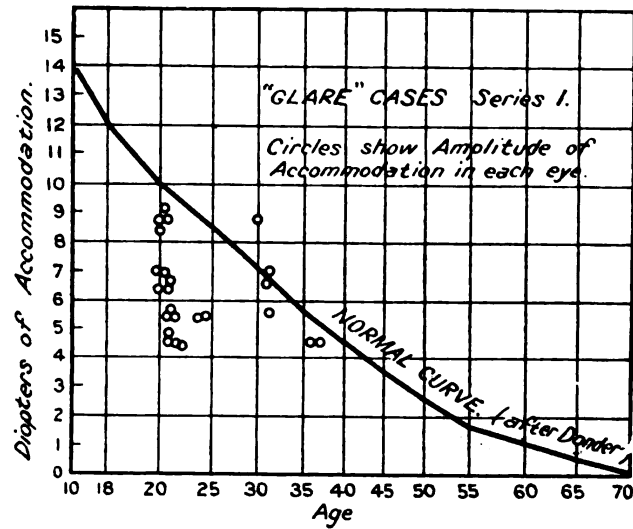


FIG. 1.

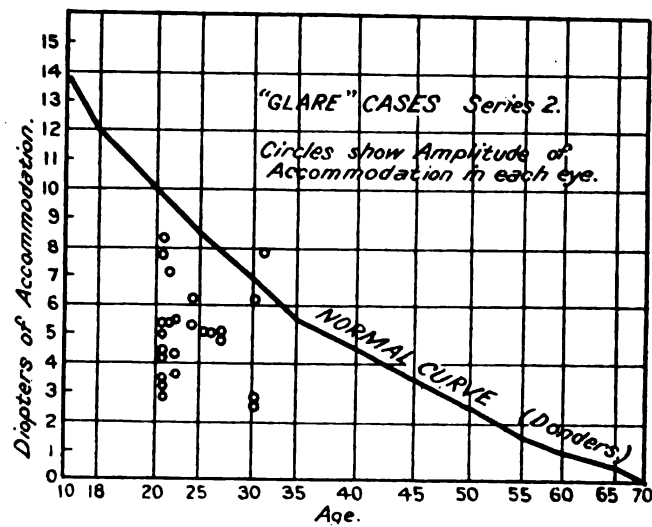


FIG. 2.

idea of the difference between the controls and the cases suffering from glare. It must be stated that these figures cannot be taken as indicating the relative proportion of fatigue due to glare with any amount of accuracy, on account of the small numbers examined.

ACCOMMODATIVE ASTHENOPIA IN EGYPT.

On arrival in Egypt I visited each station and carried out routine examinations of all available flying personnel. An attempt was made to find out whether there was any correlation between accommodative asthenopia (estimated by receding of the near point) and length of service in the East: 115 examinations were made and the results indicated that there is a constantly increasing asthenopia in most individuals during their residence in Egypt and the East. The subjects with marked defects of power of accommodation usually had a history of erratic landings.

The diagram exhibited shows the results of an examination of sixteen officers at Ramleh, and indicates that some pilots develop accommodative asthenopia which varies in degrees of severity according to the length of their stay in the East. Certain individuals, however, do not show any correlation. These cases were found to have a special predisposition to the effects of glare on account of errors of refraction and heterophoria.

These investigations suggest that the first indication of visual fatigue is shown by fatigue of accommodation. Although this condition causes discomfort, yet there is no correlation between mild accommodative asthenopia and inability to estimate distance on landing an aeroplane. It is only when there is a *marked* receding of both near points—about 30 per cent. defective, or spasm of accommodation, that we notice a tendency to make an error of judgment. The importance therefore lies in the fact that receding of the near point is the sentinel symptom of eye fatigue, and calls our attention to the necessity for further examination.

We now pass to the question of

GLARE AND CONVERGENCE.

Good convergence is necessary in order to maintain the highest degree of binocular stereoscopic vision. The association between glare as the exciting cause of a breakdown or weakness of convergence was, therefore, the next step in our investigations.

A few preliminary words in explanation of the mechanism of convergence are desirable.

Starting Point of Convergence.—We do not know exactly what position the eyes would take during life in the absence of any converging innervation, though the divergence which follows monocular amblyopia seems to show that it would be one of considerable divergence. Le Conte has shown that during sleep, and even in his case during drowsiness, the eyes diverge, as they also do in drunkenness, under chloroform and at death.

Tonic Convergence.—All muscles possess physiological tone. By this tonic contraction, the visual axes are brought into a condition of parallelism, so that on viewing a distant object, and occluding one eye, the vision either remains undeviated, or the object only moves slightly.

Exophoria in distant vision indicates a deficiency, and esophoria an excess of tonic convergence.

ACCOMMODATION AND CONVERGENCE.

When a person with a normal pair of emmetropic eyes looks at a near object, the eyes converge in order that both visual axes may be directed at the object. At the same time the eye "accommodates," in order that the rays of light may be accurately focussed on the retina.

These two functions, accommodation and convergence, are in ordinary life always employed together, so that they have become "associated." It is difficult, therefore, for a normal pair of eyes to accommodate without converging, or to converge without accommodating. As a rule, each diopter of accommodation is associated with about three-quarters of a metre—angle of associated convergence, so that in a typical emmetrope, the 4 diopters of accommodation in exercise for vision at a quarter of a metre, are accompanied by 3 ma. of convergence. The deficit of 1 ma. is made up by a visual reflex action known as the "fusion reflex," which protects us against seeing double. This fusion function is maintained in activity by the psychical level, and in the expression of the desire for single binocular vision.

Maddox holds that this "fusion convergence" involves greater wasting of co-ordinating nervous energy than "accommodative convergence" and suggests that it is the cause of the "muscular asthenopia" described by Von Graefe, which Maddox believes to be really a "central asthenopia."

INVESTIGATIONS INTO GLARE AND CONVERGENCE IN INDIA.

The optometer was used in measuring the convergence in the cases previously described. Both eyes were uncovered, and the carrier moved towards the eyes until the hair or a thin black line appeared doubled. This was the point we estimated as the near point.

Analysis of Results.—Since my return to England, I have submitted my figures to Wing-Commander Clements [7] for criticism, and find that he grades cases in a simple and satisfactory way as follows:—

Near point of convergence	{	Outside 4 in.	Condition
		From 3 to 4 in.	Poor
		From 2 to 3 in.	Fair
		Inside 2 in.	Good Very good

The optometer we used in testing the power of convergence in the cases described only registered accurately up to 2'8 in. so that I have classified our cases as

(A) Good	3 in. and under
(B) Fair	3 to 4 in.
(C) Poor	Outside 4 in.

Taking this standard we find that in Series 1 (cases of glare occurring in April)—

Three cases had good convergence
Two cases had fair convergence
Eight cases had poor convergence

In Series 2 (taken in May and June when the effects of glare were felt more severely) every case was graded "poor." The mean average being 7'5 in. (equal to 5'6 metre angles) compared with 4'3 in. (equal to 8'1 metre angles) in the April cases. Showing marked deterioration during the hotter months.

The controls show a mean average of 4'2 in. and 3'75 in. respectively, after an average of nineteen months exposure to tropical conditions.

In Series 1 there were six good cases, five fair cases and three with poor convergence. The poor cases all complained of symptoms of glare.

In Series 2 there were three cases of good, six of fair, and three of poor convergence.

EXPERIENCE.

In assessing the deleterious effect of eye fatigue in pilots, allowance must be made for the experience of the individual and his ability to fly.

A degree of eye fatigue, which, in a beginner, would certainly be associated with errors of judgment on landing, might not produce any apparent difference in an able and experienced pilot. Nevertheless, we find that an experienced pilot whose eyes show signs of "wear and tear," although he may still land well, will admit that he no longer lands "automatically," but does so with conscious care and deliberation, and frequently with apprehension that he may crash.

At Medical Boards, we often meet with cases of "anxiety neurosis" where the predisposing cause is eye fatigue and constant fear of crashing on landing. Hence the importance of preventing the onset of a psychoneurosis by careful observations of the visual apparatus. By stepping in at the right time and recommending a tired pilot for a rest, we may keep our personnel up to the high standard of fitness required.

CONCLUSIONS.

The most recently acquired faculties are always the first to be lost, and the "stereoscopic sense," belonging to the psychic level, is probably one of the most recent acquisitions of the human race.

These visual reflexes are the "semi-mechanized results of successive mental adaptations effected by the mental efforts of successive generations."

In some individuals there is an inherent power or "vital force" which enables them to retain their binocular stereoscopic vision under adverse conditions or stress, others under similar conditions will suffer, and in these cases we may consider that there is an inborn tendency to break down, in the same way that the investigations of Sir Frederick Mott [13] have shown that there is an inborn tendency (associated with definite changes in the endocrine organs and cortex) in certain individuals to develop dementia præcox at puberty, or mania during childbirth.

This conception of "resistance" explains why some complain of the glare and others do not.

We cannot always prophesy whether a candidate with normal vision will break down and develop eye trouble during training, because we cannot estimate his neuropathic and psychopathic predisposition with sufficient accuracy at present.

What we aim at achieving is by careful and repeated examinations during the entire period of a flying officer's service, to satisfy ourselves that no visual deterioration has occurred such as may result in an error of judgment on landing.

(II) TROPICAL CLIMATE AND MENTAL EFFICIENCY.

There appears to be no doubt that tropical climate impairs mental efficiency, and is a predisposing cause to psychoneurosis. Woodruffe [11] has shown that the blonde white race suffers from tropical light, and eventually tends to die out. Isaac Taylor [12] states that there is no third generation of unmixed European descent in the tropics, as they become sterile. Sterility suggests an endocrine basis for our tropical psychoneurosis, just as the war

neurosis was often associated with disturbed endocrine balance. Our psychoneurotic patients however, almost invariably had some mental conflict, and considerable relief was obtained by treatment with suggestion, persuasion or analysis.

FACTORS WHICH PREDOMINATE.

Let us briefly consider the various factors which we notice during residence in the plains of India:—

(1) *Fatigue*.—There is no doubt that the fatigue caused by the heat and glare of the sun, the irritation of the skin from prickly heat and insect bites, the loss of sleep at night, the lack of nourishing food, the periodical attacks of gastric disorder, &c., definitely diminishes the control normally exercised by the higher mental faculties over the lower mechanisms of the nervous system. Hence, irritability and attacks of anger over trivial incidents, a tendency to magnify small unpleasantnesses, together with lassitude, lack of ambition, and ability to concentrate for any length of time, are the symptoms which are readily noticed towards the end of the hot season.

The intelligent adaptation of one's habits to environment may discount a great deal of the disabilities caused by climate, but before the method is learned and acclimatization has occurred, there may be sufficient loss of control to predispose the subject to a psychoneurosis.

(2) *Self-criticism, Race-hostility, &c., as Factors associated with Psychoneurosis*.—India is an ideal place for the development of a psychoneurosis or psychosis in a potential psychopath. The European is living in the midst of potentially hostile races. The story of the Mutiny is still remembered, and the riots in the Punjab in 1919 are not forgotten. The recent Afghan War and the Waziristan Campaign were associated with stories of mutilation of prisoners and the prevalence of looting at night in frontier stations is disturbing to sleep. Evidence of this factor as a cause of psychoneurosis is found in the way Europeans congregate at the clubs in Indian stations. However small the station is, there is always a club, and in the evening practically every European in the station goes there.

Social functions, dances and entertaining have an importance in India that is out of proportion to the pleasure which is ordinarily obtained, and the explanation suggested is that the herding together of Europeans is a protective measure induced by the anxiety caused by their small numbers in the midst of an alien race. This anxiety shows itself in the exaggerated desire to maintain "prestige." The European feels that he is always under critical observation, and seldom relaxes his watch over his behaviour lest he should lower himself in the eyes of his servants. Inexperienced officers frequently pay absurdly high wages and allow themselves to be cheated rather than admit that they cannot afford to pay the prices asked, through fear of losing "caste." Thus, to the constant strain of self-criticism and observation is added that of financial difficulty, which in the case of married officers is certainly the most serious problem of life in India.

(3) *Financial Worry*.—Poverty is generally regarded as a crime. We have often noticed people whom we know to be "hard-up" giving absurdly high tips and trying to conceal the crime of poverty. Whilst I know of instances in which financial worry is unavoidable—such as stoppages of pay through clerical errors on the part of the pay agent, debts contracted by an extravagant wife, &c.—yet my observations have impressed me that entertaining and dinner parties, dances, and standing drinks at the club, are an important source of

financial worry. The lot of the married officer commanding a squadron, and others from whom entertaining is expected, is particularly hard, for he feels that public opinion obliges him to entertain so as to keep up the position of his rank.

(4) *Psychoneurosis in Flying Personnel*.—Added to the factors mentioned, the flying officer has the strain of adaptation to a new environment, that is, the air. The view taken by R.A.F. medical authorities is that the chief cause of psychoneurosis in flying officers is a failure to suppress the disturbing emotions produced by the instinct of self-preservation. This failure is due to crashes, forced landings, &c., or to weakness of the higher controlling forces—either innate or acquired, the latter being in many cases associated with toxæmic states.

(5) *Visual Disorder as a Cause of Psychoneurosis*.—We have shown that when the pilot has acquired the art of flying, various manoeuvres are performed without conscious thought or “automatically.” With the onset of eye-fatigue and lack of ability to estimate distance on landing, the pilot has to perform this act with conscious care, often flattening out too soon so as to avoid crashing, and this is accompanied by the emotion of anxiety. As one pilot of great skill and resolution (whose eyes were fatigued by glare) said to me in India, “Whenever I start to land, I wonder whether I am going to crash.” His landings, owing to his skill, were still safe, he knew the performance of his machine, and there was no obvious bad landing such as one would find in a novice with the same degree of defective vision, and his amour-propre would not allow him to ask for a rest. Another pilot, with marked hypermetropia, who, as a scout pilot, won the D.S.O., told me “My landings are erratic, but it is due to nervousness.” He confused cause and effect.

PHYSIOGENIC OR PSYCHOGENIC ORIGIN.

The question is often asked why certain individuals develop nervous symptoms, whilst others do not.

The physiogenic theory, according to Sir Frederick Mott [14], presupposes an inherent germinal narrow physiological margin of normal functional capacity of the brain; and stresses which may be physiological (e.g., adolescence or pregnancy), pathological (thyroid insufficiency; microbic toxins, &c.), or psychological (emotional shocks, conflicts, &c.), reveal, excite or accelerate a genetic inadequacy, causing a disintegration of the psychic unity.

The psychogenic school believes with Jung [15] that there is a certain predisposition from which arises a non-adaptable psychological function, which may develop into manifest mental disorder. In proof of this conception is the statement that we have no proof of the primary nature of the organic disorder, but overwhelming proofs exist of a primary psychological fault in function, the history of which can be traced back to the patient's childhood.

TROPICAL CLIMATE AND PSYCHONEUROSIS.

In conclusion, I think we may agree that tropical climate plays a primary part by undermining or lowering the bodily resistance, secondly, mental factors, such as conflicts come in during this debilitated state and determine the nature of the psychoneurotic symptoms. Imperfection of organic functions tends to produce an undue prominence in consciousness of the bodily self and, therefore, an introspective and brooding habit of mind. It is during this stage that problems seem so hard to face, decisions cannot be made and sleepless

nights or nightmares develop. In the worst cases, the family history and early life clearly indicate a psychopathic tendency—and it is suggested that in selecting individuals for service in the tropics, we should take this into consideration.

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INDEX

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Ab

Al

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INDEX

Note.—The Occasional Lectures in the Society's *Proceedings* are indicated by the abbreviation *Occ. Lect.* They are placed first in the bound volumes under the heading "General Reports." They are followed by the *Proceedings* of the Sections, which are arranged alphabetically, each Section being separately paged. The *Proceedings* of the Sub-section of Proctology are included in the Section of Surgery and indicated by the abbreviation *Proct.* preceding the numerals. The page references indicated by the numerals under the heading *Med.* and *Ophth.* refer to the combined discussion held by the Sections of Medicine and Ophthalmology, the report of which appears in the bound volumes immediately after the reports either of the Section of Medicine or the Section of Ophthalmology. The page-references indicated by the numerals under the heading *Obst.* and *Therap.* refer to the combined discussion held by the Sections of Obstetrics and Gynæcology and Therapeutics and Pharmacology, the report of which appears in the bound volumes immediately after the reports either of the Section of Obstetrics and Gynæcology or the Section of Therapeutics and Pharmacology.

N.B.—The Section of History of Medicine being again included in the whole volume of the *Proceedings*, the entries relating to that Section are, as before, contained in this Index.

- Abdomen**, acute suppurative conditions of, operations for, value of experience in, *Surg.* 5
- Abdominal glands**, calcified, relation of, to urinary surgery (Sir J. Thomson-Walker), *Urol.* 1-17
 see also *Lymphatic glands*, abdominal
- muscles, rhythmical stimulation by interrupted currents, *Baln.* 14, 15
- pain in case of acquired chronic hæmolytic jaundice, *Med.* 74
- Abdomino-anal operation** for cancer of rectum, operation June 1920, present condition of patient (H. Brown), *Proct.* 89
- Abductor paralysis**, bilateral, causing stenosis of larynx, operative methods in treatment, with special reference to new method by which airway may be permanently enlarged and patient decannulated (Irwin Moore), *Laryng.* 32-38
- cases of, unilateral and bilateral, proportion of, *Laryng.* 32
- double, case (A. A. Smalley), *Laryng.* 94
- ventriculo-chordectomy for, case (W. Howarth), *Laryng.* 47
- Abortion**, instruments used to procure, pushed through wall of uterus, *Obst.* 43
- Abscesses**, ischio rectal, new method of treating (J. P. Lockhart-Mummery), *Proct.* 65
- Acarus**, from case of mange in human being infected by dog (A. Whitfield), *Derm.* 75
- human, comparison with *Sarcoptes canis*, *Derm.* 76
- male and female, extracted from one burrow (A. M. H. Gray), *Derm.* 87
- Accidents**, mortality from, among coal-miners (ages 25-64), period 1890-1912, *Epid.* 88
- Accommodation**, amplitude of, definition, *War* 46
- convergence and, *War* 49
- estimation by optometer, *War* 47
- in cases with complaint of glare, investigation of, *War* 46
- insufficient range of, and asthenopia, association between, results of investigations into, *War* 47, 48
- Accommodative asthenopia** in Egypt, *War* 49
- Acetabulum**, operation for making upper lip to, in treatment of congenital dislocation of hips, *Orth.* 17-19
- Acetonuria** and diaceturia, persistent, in case of enlarged liver (C. Worster-Drought), *Child.* 56
- Achlorhydria**, association with dental sepsis, *Odont.* 30
- Acholic jaundice**, cases of (V. Coates), *Clin.* 28; (D. Paterson), *Child.* 41; (R. Hutchison), *Child.* 41
- Acidosis** theory of rickets, definition of term, *Child.* 2, 3
- Ackland**, W. R.—Discussion on dental sepsis, *Odont.* 28
- some considerations for preventive dentistry, *Odont.* 1-5
- Acne agminata**, cases of (Capt. Bruce), *Derm.* 16; (E. G. Graham-Little), *Derm.* 15
- scrofulosorum, case of (A. M. H. Gray), *Derm.* 101
- varioliformis, case (W. K. Sibley), *Derm.* 106
- Aconite**, sedative action on horses, *Therap.* 44

- Acromegaly** in girl aged 16, with congenital heart disease (aortic stenosis), case (E. Stolkind), *Clin.* 22
- Acromion** process, left, epiphysis of, injury to (P. B. Roth),
- Acusticus** tumour (right), case of; operation by Sir V. Horsley in 1912; removal of tumour; recovery (F. J. Cleminson and F. M. R. Walshe), *Otol.* 31, 32
- tumours (F. M. R. Walshe), *Otol.* 32
- symptoms due to compression of anterior part of lateral lobe of cerebellum, *Otol.* 35
- symptoms due to involvement of fifth, facial and sixth nerves, *Otol.* 33, 34
- symptoms of raised intracranial tension, *Otol.* 35
- see also *Eighth* nerve tumours
- Adams, J. E.**—Urgent need for education in the control of cancer, *Occ. Lect.* 29-33
- Adams, P.**—Discussion on significance of vascular and other changes in retina in arterio-sclerosis and renal disease, *Med. and Ophth.* 16
- Adamson, H. G.**—Case for diagnosis, *Derm.* 56
- case of multiple superficial rodent ulcer; possible embryonic sweat-duct origin, *Derm.* 24, 25
- of lupus vulgaris treated by liquid acid nitrate of mercury, *Derm.* 80
- of recurrent cellulitis, *Derm.* 79
- of scleroderma and leucoderma combined in girl, aged 16, *Derm.* 107
- discussion on case for diagnosis, *Derm.* 15
- of dermatitis repens, *Derm.* 99
- of lupus vulgaris treated with potassium iodide, *Derm.* 84
- of recurring erysipelas, *Derm.* 85
- of scleroderma, *Derm.* 47
- on leishmaniasis of skin, *Derm.* 10
- on manganese as chemotherapeutic agent, *Derm.* 67
- Adenoma** (?), cystic, containing cartilage: nasopharyngeal tumour (wet specimen and section) (A. A. Smalley), *Laryng.* 94
- of liver which had ruptured spontaneously, causing internal hæmorrhage, excision of, case (P. Turner), *Surg.* 60
- hepatectomy for, *Surg.* 62
- of prostate complicated by vesical calculi, suprapubic prostatectomy for, primary union after, three cases, *Urol.* 48, 49
- suprapubic prostatectomy for, primary union after, 14 cases, *Urol.* 47, 48
- of vaginal fornix simulating cancer of cervix (H. R. Spencer), *Obst.* 27
- microscopical appearances, *Obst.* 28-30
- sebaceous, case (H. C. Semon), *Derm.* 53
- weighing 2 lb. 3 oz., removed from liver, case of; with remarks on partial hepatectomy (G. G. Turner), *Surg.* 43-56
- pathological features, *Surg.* 46
- Adenomatosis** vaginæ, case, and treatment (B. Whitehouse), *Obst.* 46
- Adenomyomata** of female pelvic organs, clinical aspects of (A. Donald), *Obst.* 82-90
- age, social conditions and obstetric history of cases, *Obst.* 83
- association with infiltrating ovarian cysts with tarry contents, *Obst.* 82
- Adenomyomata**, conditions found at operation, *Obst.* 87
- details of sixteen cases, *Obst.* 83
- diagnosis of, *Obst.* 87
- operations for, *Obst.* 87
- pathological reports on cases, *Obst.* 88
- physical signs of, *Obst.* 83
- situations of, *Obst.* 82
- symptoms of, *Obst.* 83
- table of sixteen cases in which operation was performed, *Obst.* 84-86
- terminology, *Obst.* 82
- Adhesions**, cauterization of, in artificial pneumothorax treatment of pulmonary tuberculosis under thoracoscopic control (H. C. Jacobæus), *Electr.* 45-60
- bibliography, *Electr.* 61
- classification, *Electr.* 50
- critical survey of operations, *Electr.* 54-60
- indications for, *Electr.* 51
- technique, *Electr.* 47, 48
- under guidance of thoracoscope, *Electr.* 53, 54
- Adie, W. J.**—Case of dystrophia myotonica, *Neur.* 45
- dystrophia myotonica (myotonica atrophica), an heredito-familial disease with cataract, *Neur.* 36-43
- Adiposis** dolorosa, cases of (B. Myers), *Clin.* 11; (E. Stolkind), *Clin.* 44
- Adrenal** cortex, influence on nutrition, *Psych.* 24
- medication in organotherapy, *Therap.* 12
- medulla, part played by, in defence mechanism of body, *Psych.* 25
- Adrenalin**, reinforced by pituitrin, *Therap.* 4
- therapeutic value of, *Therap.* 3, 4
- Adrian, E. D.**—Disorders of function in the neurone, *Neur.* 55-60
- "Aerologia"** of Domenico Panarolo (G. Hinsdale), *Balm.* 19-21
- Afferent** impulses to nerve centres through vagus and sympathetic nerves, routes for, *Balm.* 8
- Age** and sex distribution in scarlet fever (F. M. Turner), *Epid.* 19-30
- Agglutination** reaction, high, in case of enteric carrier, *Epid.* 2
- tests in diagnosis of sand-fly fever in Malta, *War* 3, 5
- Agglutinins** in blood, factors in inheritance, *Path.* 37
- inheritance of groups, mode of, *Path.* 42
- tests of grouping of, rationale of, *Path.* 37
- Agincourt**, Battle of, casualties at, absence of medical arrangements for, *Hist.* 7
- Air** embolism occurring during urethroscopy, case (R. Ogier Ward), *Urol.* 54
- passages, teeth in, during extraction under anaesthesia, *Anæsth.* 18
- pilots, psychoneurosis in, *War* 53
- Aitken, D. M.**—Discussion on operative treatment of dislocated hips, *Orth.* 24
- osteo-chondritis of hip, *Orth.* 13
- traumatic osteo-arthritis of neck treated by bone-graft, *Orth.* 30
- Albumin**, absence of, from urine, no proof of renal integrity, *Urol.* 83, 84
- Albuminuria** absent in cases presenting cardinal signs of renal retinitis, *Med. & Ophth.* 15
- effect upon prognosis of eclampsia, *Obst.* 4

- Albuminuria**, not a constant symptom of chronic nephritis, *Urol.* 82
transient, in case of sudden onset of hemiplegia in pregnant woman at full term; Cæsarean section; gradual recovery (F. Cook), *Clin.* 43
- Alcohol**, abolition of nerve conductivity by, *Neur.* 56
- Alcoholic diseases**, mortality from, in various coalfields compared, *Epid.* 92, 93
habits in relation to venereal disease, *War* 20
- Alcoholism**, chronic, asymmetrical neuritis in, *Neur.* 15
mortality from, among coal-miners (ages 25-64), period 1890-1912, *Epid.* 88
- Alexin**, normal, properties of, *Path.* 4
- Alimentary canal**, axial anastomosis of, technique of (C. A. Pannett), *Surg.* 81-83
effects of exposure to radium upon (J. C. Mottram), *Electr.* 41-44
mucus in, normal production interfered with by small doses of radiation, *Electr.* 44
- Alopecia** of beard, due to dental sepsis, *Odont.* 28
outbreak of (2 cases) (H. C. Semon), *Derm.* 100
- Altitudes**, high, life at, medical aspects of, discussion on, *Med.* 58-62
- Alumina**, dust of, results of inhalation of, *Epid.* 98
- Alveolar process**, disease of, radiographic evidence of, in dental sepsis, *Odont.* 10, 11
- Amaurosis**, complete, dementia, and spastic paralysis in Hebrew boy aged 10 (G. Riddoch), *Neur.* 29
in uræmia, significance and causation of, *Urol.* 23
- Amaurotic family idiocy**, case (A. H. Levy), *Ophth.* 17
- Ambard's constant**, formula of, *Urol.* 81
- Amenorrhœa**, with (?) thymic asthma, *Therap.* 4
- American Civil War**, venereal disease in, *War* 16
Society for Control of Cancer, methods of, *Occ. Lect.*, 31
- Americans**, North, typical, physiognomy of, *Baln.* 13
- Amnesia** for names of objects in case of left temporo-sphenoidal abscess (S. Scott), *Otol.* 55
- Amyl nitrite**, tolerance by animals, idiosyncrasies of, *Therap.* 43
- Amyotonia congenita** (?), case of, for diagnosis (S. A. K. Wilson), *Neur.* 49
- Anæmia**, character of infective lesions in, *Med.* 8
forms of, causing polyneuritis, *Neur.* 18
glossitic, hæmolytic, *Med.* 9
individuality and identity shown by seasonal onset and relapses, *Med.* 26, 28
specificity of, *Med.* 27
see also *Anæmia*, pernicious
in relation to effects of exposure to radium, *Electr.* 44
nature of, with which combined sclerosis is associated, judged by seasonal incidence, glossitic and hæmolytic features, and blood changes, *Med.* 26
pernicious, a glossitic, hæmolytic and neuropathic disease, *Med.* 8
and other anæmic and cachectic conditions, nervous troubles associated with (foot-note), *Neur.* 73
beneficial effect of removal of oral sepsis on, *Odont.* 21
- Anæmia**, pernicious, blood changes in, compared with those presented in cases of combined sclerosis, *Med.* 33, 34
typical cases, *Med.* 4, 5, 37
case of acquired hæmolytic (acholuric) jaundice, seen fifteen years ago, with blood picture at that time resembling (F. Parkes Weber), *Med.* 73-77
central cerebral nervous system features in, *Med.* 42
cerebral and mental nervous features in, *Med.* 20, 31
clinical features, character of, classes illustrating, *Med.* 29-33
differential diagnosis from septic anæmia, *Med.* 10
glossitic, typical case of, *Med.* 14-18
glossitic and toxic nerve features, and their relation to one another, *Med.* 12
hæmolytic, idiopathic, specificity of, *Med.* 27
lesions in, *Med.* 8
toxin of, also neurotoxic, *Med.* 6
idiopathic, *Med.* 6
infective lesions underlying, *Med.* 6, 8
lesions in jejunum in case of, *Med.* 15
of mucosa and submucosa of stomach in case of, *Med.* 13
melancholia in, *Med.* 21
nervous and mental disorders associated with, cases illustrating, *Med.* 2
features in, character of, *Med.* 12
interpretation of, *Med.* 39
peripheral nervous features of, *Med.* 20, 41, 42
post-mortem character of case of, dying in state of jaundice, *Med.* 78
question of fragility of red corpuscles in, *Med.* 80
relation of lesions to nervous features, *Med.* 40
sclerosis of spinal cord in, features of cases, *Med.* 11, 22, 23
septic complication in case of, *Med.* 30
"sore tongue" of, *Med.* 6, 7
spinal cord lesions in, features of, *Med.* 42
tongue lesions in, intense neuritic and muscle changes in, *Med.* 6, 8, 9, 10
toxæmic attacks in, *Med.* 21
toxic nervous features in, case illustrating, *Med.* 7, 8
see also *Anæmia*, glossitic hæmolytic
secondary, "cotton wool" patches in retina in, origin of, *Med.* & *Ophth.* 4, 5
or "septic," in dental sepsis, *Odont.* 12
septic, cord lesions never found in, *Med.* 24, 25
dental sepsis causing, *Odont.* 20
differential diagnosis from pernicious anæmia, *Med.* 10
general nervous disturbance of cerebral system in, *Med.* 24
infective lesions underlying, *Med.* 6, 8
leucopenia in, *Odont.* 20
nervous and mental disorders associated with, cases illustrating, *Med.* 2
toxin of, non-specific in action on blood and blood-forming organs, *Med.* 6
types of blood changes in, *Med.* 5
severe, two types of, character of blood changes in, at different stages, *Med.* 35, 38

- Anæmia**, with enlargement of spleen accompanying congenital family cholæmia, *Med.* 80
with sclerosis, blood changes in cases of, *Med.* 36
- Anæmias**, severe, nervous and mental disorders of, in relation to their infective lesions and blood changes (W. Hunter), *Med.* 1-42
oral sepsis in relation to, *Med.* 3
types of, associated with nervous and mental disorders, *Med.* 2
- Anæsthesia**, general, in dental surgery (W. J. McCardie), *Anæsth.* 11-20
apparatus for, *Anæsth.* 15
case of swallowed sponge, *Anæsth.* 19
cases of teeth in air-passages under, *Anæsth.* 18
collapse after, *Anæsth.* 17, 18
difficult cases under, *Anæsth.* 17
dilatation of heart during, *Anæsth.* 13
nitrous oxide for, safety of, *Anæsth.* 12
pneumonia after, two cases, *Anæsth.* 19
posture of patients in, *Anæsth.* 20, 21
preliminary sedative treatment, *Anæsth.* 15
respiratory obstruction in patient in whom part of tongue and large mass of glands had been excised, *Anæsth.* 18
use of etherized sponges in, *Anæsth.* 15, 16
in Cæsarean section, danger of spinal methods, *Anæsth.* 1
inhalation methods preferable, *Anæsth.* 1
infiltration methods, *Anæsth.* 1
see also *Cæsarean* section
spinal, with tropacocaine, Cæsarean section under, two cases (B. Whitehouse and H. Featherstone), *Obst.* 55-58
- Anæsthetic**, cardiac arrest under, followed by heart massage, case (E. S. Rowbotham), *Anæsth.* 5
in Cæsarean section, choice of, factors influencing, *Obst.* 50
in dental surgery, choice of, factors influencing, *Anæsth.* 13
- Anæsthetics**, deaths under, classification as violent or unnatural, discussion on, *Anæsth.* 33-38
advantages, *Anæsth.* 39
disadvantages, *Anæsth.* 39
committee of investigation into, at St. Thomas' Hospital, *Anæsth.* 39
substitution of inquiry into, by expert committee instead of coroner advocated, *Anæsth.* 38
importance of careful routine examination of patients preceding administration of, cases illustrating (A. L. Flemming), *Anæsth.* 9, 10
used in dental surgery, statistics of, *Anæsth.* 12, 13
- Anæsthetization** of patients for classical Cæsarean section (H. R. Spencer), *Anæsth.* 1
- Anaphylaxis** and high blood-pressure, *Baln.* 3
skin and asthma, relations between, *Therap.* 4
- Anastomosis**, axial, of alimentary canal, technique of (C. A. Pannett), *Surg.* 81-83
of colon, technique, *Surg.* 73-76
end-to-end, operative results of, *Surg.* 81
lateral, of large intestine, disadvantages of, *Surg.* 72
methods of, after resection of colon, *Surg.* 72
- Anastomosis**, near lower end of pelvic colon, methods of, *Surg.* 80
of colon, axial, without exposing mucous membrane, methods of, *Surg.* 76
of ileum to large gut, method of, *Surg.* 79
and resection of colon, for tumour, technique of (J. P. Lockhart-Mummery), *Surg.* 69-81
- Anderson**, C. C.—Deep X-ray therapy in laryngological conditions, *Laryng.* 96
- Andrews**, H. Russell.—Carcinoma of prolapsed cervix in woman aged 77, *Obst.* 109
discussion on adenomyomata of female pelvic organs, *Obst.* 90
on case of carcinoma of vagina, *Obst.* 27
on instruments, &c., left in peritoneal cavity, *Obst.* 43
on sarcoma of uterus, *Obst.* 65
on treatment of dysmenorrhœa, *Obst.* 115
of uterine hæmorrhage by radium, *Obst.* 79
- Aneurysm** of arch of aorta and innominate artery in woman, case (B. Myers), *Clin.* 9
- Angeloma** of right upper eyelid (J. F. Cunningham), *Ophth.* 15
pedunculated (bleeding polypus) of inferior turbinal, case (S. Hastings), *Laryng.* 25
- Angina** pectoris, association of diseased coronary arteries with, first account, by Edward Jenner, *Occ. Lect.* 3
- Angiokeratoma**, case of (H. Davis), *Derm.* 29
discussion on (F. P. Weber, H. G. Adamson, A. Eddowes), *Derm.* 30
- Angiomatous** granuloma, see *Granuloma*, angiomatous
- Angioma** of vaginal wall (H. Briggs), *Obst.* 61
- Angiosarcoma**, hæmorrhagic, of upper jaw, case (H. J. Banks-Davis), *Laryng.* 49
- Animals** and man, eradication of glanders and anthrax in (Sir J. Moore), *Med.* 49-56
domesticated, odontalgia not experienced by, *Odont.* 64
drug tolerance of, as compared with man, idiosyncrasies to (W. H. Kirk), *Therap.* 43-47
- Aniridia**, total, following preliminary iridectomy example of (T. H. Butler), *Ophth.* 21
- Ante-natal** mortality from venereal disease, *War* 21
- Antero-tibial** artery, microscopical appearances in case of thrombo-angitis obliterans, *Clin.* 15, 16
- Anthrax**, as air-borne disease, cases showing, *Epid.* 40
analysis of cases showing sources of infection, *Med.* 53
and glanders, eradication of, in man and animals (Sir J. Moore), *Med.* 49-56
industrial (wool-sorter's disease), sources of, *Med.* 54
in animals, factors to be observed in dealing with, *Med.* 55
forms of, *Med.* 5
fulminant or apoplectic, *Med.* 5
infection, animals resistant to, *Path.* 6, 7
in man, forms of, *Med.* 52
malignant pustule (wool-sorter's disease), 51, 52
longevity of soil infection, *Med.* 53
natural resistance to, defence mechanisms and animal experiments, *Path.* 6, 7
passive transference of immune serum in, *Path.* 2

- Anthrax**, vaccination of sheep and cows against, Pasteur's discovery of, *Occ. Lect.* 13
virulent, doses of, how rendered tolerable to guinea-pig and rabbit, *Path.* 8
- Anti-dysenteric** serum, polyvalent, in treatment of ulcerative colitis, *Proct.* 107
- Antimony**, administration of, healing of ulcerating granuloma of pudenda commencing immediately after, case (P. Manson-Bahr), *Clin.* 25
- Antirabic** Institute in the Tropics, establishment of (A. E. Hamerton), *Trop.* 49-55
treatment in Bagdad, results of, *Trop.* 54
cost and limitations of, *Trop.* 54
vaccine, dosage of, *Trop.* 53, 54
manufacture of, *Trop.* 52
- Antrum**, carcinoma of, removal of upper jaw, case (A. A. Smalley), *Laryng.* 94
empyema of, chronic, Canfield's operation, recovery, case (Sir J. Dundas-Grant), *Laryng.* 20
exploring trocar and cannula, improved form (H. M. Wharry), *Laryng.* 53
maxillary, empyema of (Denker's operation), after-treatment of (D. L. Sewell) (abstract), *Laryng.* 85
left, and left frontal sinus, suppurative disease of, case (Sir W. Milligan and D. L. Sewell), *Laryng.* 90
sepsis in, acute, origin of, *Odont.* 41
chronic, origin of, *Odont.* 41
suppuration in, of dental origin, clinical distinction from that of nasal origin, *Odont.* 42
proportion of cases of dental origin, *Odont.* 41
of stomach, pressure upon, due to pathological gall-bladder, *Electr.* 79
see also *Sinusitis*, antral
- Aorta**, arch of, and innominate artery, aneurysm of, in woman, case (B. Myers), *Clin.* 9
- Aortic** stenosis, with acromegaly in girl aged 16 with, case (E. Stolkind), *Clin.* 22
- Apical** infections in dead teeth, prevention, *Odont.* 23
one form of dental sepsis, *Odont.* 22
- Apomorphine**, tolerance to, of animals, idiosyncrasies of, *Therap.* 43
- Appendicitis**, change in type of, *Med.* 45, 47
dental sepsis in relation to, *Odont.* 14
incidence of, and mortality from, increasing, *Med.* 45; *Surg.* 6, 7
statistics, 1880-1919, Guy's Hospital, *Med.* 45
1901-1920, *Surg.* 7, 8
operations for, too frequently performed, *Surg.* 5
- Appendicostomy** in treatment of gonorrhœal stricture of rectum, *Proct.* 14, 19
in treatment of ulcerative colitis, *Proct.* 95, 99
important points in connection with, *Proct.* 100
- Appendix**, conditions of, in cases of calcified abdominal glands, *Urol.* 4
- Archer**, G. E.—Case of laryngeal polyp, *Laryng.* 96
case of chronic cellulitis of face, *Laryng.* 94
of lupus of inferior turbinals, *Laryng.* 96
tertiary syphilitic lesions of tonsils, *Laryng.* 96
- Argyll-Robertson** pupils with mydriasis, case (F. Parkes Weber), *Child.* 68
- Argyria**, Gowers' case of, *Neur.* 13
- Arkwright**, J. A.—Discussion on the ultraviolet viruses, *Epid.* 77
- Armour**, Donald, C.M.G.—Case of syphilitic disease of anus and rectum in young woman, *Proct.* 90
- Army**, incidence of venereal disease in, at various periods, *War* 15
physical training in (Col. R. C. Campbell), *War* 31, 37
- Arsenic**, value, in asthma, *Therap.* 6
paste, rodent ulcer under treatment with, case (A. H. M. Gray), *Derm.* 78
- Arsenical** compounds in treatment of neurosyphilis, comparative value of, *Neur.* 76
toxic effects of excessive dosages of, *Neur.* 74, 75, 76
- Arseno-benzene**, action on *Leucocytozoon syphilitidis* indirect, *Derm.* 66
- Arterial** oxygen saturation at various altitudes, *Med.* 58
- Arterio-sclerosis** and renal disease, vascular and other changes in retina in, significance of, discussion on, *Med. & Ophth.* 1-36
general, retinal changes usually visible on ophthalmoscopic examination, *Med. & Ophth.* 6
not definitely correlated with cerebral hæmorrhage, *Med. & Ophth.* 28
retinitis due to local vascular disease in retina, in some cases of, *Med. & Ophth.* 5-10
stages of, as indicated by exudates in retina, *Med. & Ophth.* 8, 9
- Arthritic** changes associated with desquamative erythema, case (Sir J. Galloway and M. G. Hannay), *Derm.* 16-18
- Arthritis**, cause of pain in congenital dislocation of hips, *Orth.* 22
complicating ulcerative colitis, *Proct.* 93
dislocation of hips following, treatment of, *Orth.* 23
due to dental sepsis diagnosed and treated as tuberculous, case of (R. C. Elmslie), *Orth.* 28
hæmophilic, of knee, case (R. C. Elmslie), *Orth.* 27
infective, non-specific, dental sepsis common cause of, *Odont.* 15, 17, 18
- Artificial** respiration, operation on brain abscess under, cases of, *Otol.* 56, 58, 59
- Arytæno-epiglottidean** fold, large cyst of orifice of larynx arising from, post-mortem specimen of (E. D. D. Davis), *Laryng.* 54, 70
- Arytænoidectomy**, treatment of stenosis of larynx caused by bilateral abductor paralysis by, *Laryng.* 33
- Asclepiadæ**, Greek; medical guild, *Hist.* 12
- Ascoli**, comparison between uræmia and urinæmia (quoted), *Urol.* 19, 20
- Asphyxia** of infant when ether used as anæsthetic in Cæsarean section, *Anæsth.* 1, 2
- Asthenia**, blood pressure low in, *Baln.* 2
post-influenzal, low blood pressure in, *Baln.* 2
- Asthenopia**, accommodative, *War* 46
in Egypt, *War* 49
insufficient range of accommodation and, association between, results of investigation into, *War* 47, 48

- Asthma**, anaphylaxis and the skin in relation to, *Therap.* 4
and eczema, alternation in attacks of, *Therap.* 4
bronchial, case (O. C. M. Davis), *Child.* 81
causes of, formula for, *Therap.* 1
epilepsy and, points of resemblance between, *Therap.* 2
foreign proteins and, *Therap.* 2
Hurst's definition of (quoted), *Therap.* 1
influence of parasympathetic system on, *Therap.* 3
laryngismus stridulus, infantile form of, *Therap.* 5
nervous factor in, *Therap.* 5
peripheral stimuli and, *Therap.* 2
problems of (W. Langdon Brown), *Therap.* 1-6
psychical stimuli and, *Therap.* 2
sympathetic and endocrine balance in relation to, *Therap.* 3
thymic, (?) with amenorrhea, case of, treatment by ovarian extract, *Therap.* 4
treatment of, *Therap.* 5
uræmic, restriction of use of term, *Urol.* 24
- Astigmatism**, post-operative, excessive, two cases of (T. H. Butler), *Ophth.* 23
- Asylum** dysentery, *Proct.* 91, 97
- Athetosis**, double, progressive, case (A. Feiling), *Neur.* 79
- Athlete's reaction** (of heart), *Anæsth.* 29.
- Atkinson, J.**—Case of shrapnel wound of larynx, *Laryng.* 15
- Atmospheric** conditions in relation to epidemiology, *Epid.* 47
- Atonicity** heart murmur, significance of, *Anæsth.* 29
- Atropine**, effect on gastric secretion, *Therap.* 6
preliminary administration in anæsthetization for Cæsarean section, *Anæsth.* 3, 4
tolerance to, of animals, idiosyncrasies of, *Therap.* 43
- Auditory** fibroma, nature of, *Otol.* 37, 38
meatus, acquired atresia of, case (E. Lowry), *Otol.* 20
- Auenbrugger**, introduction of percussion of heart by, *Electr.* 1, 2
- Auricular** fibrillation and flutter, circulating wave in auricle present in, *Therap.* 31
case of reversion to normal rhythm under administration of quinidine (B.T. Parsons-Smith), *Clin.* 50
quinidine and digitalis in treatment of, relative merits of, *Therap.* 35
selection of cases for quinidine therapy, *Therap.* 37
treatment by administration of quinidine, results, *Therap.* 40
methods of, *Therap.* 32
- Auscultation** of heart sounds and their interpretation, *Anæsth.* 27, 28
- Autonomic** nervous system and endocrine function, relationship between, *Baln.* 16
effects of hydrotherapeutic measures on, *Baln.* 9
- Autotoxæmia** causing polyneuritis, *Neur.* 19
- Babington, G.**, F.R.S.—Opening address to Epidemiological Society of London (quoted), *Epid.* 36
- Babinski** on hysteria (quoted), *Psych.* 2
- Bacillus anthracis**, pathogenicity and life history, *Med.* 52
pyocyaneus, importance of, in connexion with ulcerative colitis, *Proct.* 102
typhosus in urine of enteric carrier, *Epid.* 6
- Bacteria**, pathogenic, variation in virulence, *Epid.* 77
- Bactericidal** body, thermostable, in rat serum, *Path.* 5, 6, 7
- Bacteriology**, changes in medicine due to, *Med.* 43
changes in surgery due to, *Med.* 44
of ulcerative colitis, *Proct.* 92, 98, 102
- Bacteriolysin**, in immune serum, *Path.* 4
- Bacteriophage**, thermostability and limitation of action to certain bacterial groups, *Path.* 5
- Bagdad**, antirabic treatment in, results of, *Trop.* 54
- Baldwin, Aslett.**—Discussion on gonorrhœal stricture of rectum, *Proct.* 20.
- Ball, W. Girling.**—Absent right kidney, deformity of left ureter, *Urol.* 35
necrosis of kidney, following ligature of abnormal renal vessels, *Urol.* 34
pyelogram, illustrating breaking of two shadows into multiple shadows, as result of injection of sodium bromide, *Urol.* 85
specimen showing transitional-celled growth of kidney, *Urol.* 35
- Ballance, Sir C.**, K.C.M.G.—Cases of brain abscesses and tumours operated on under artificial respiration, *Otol.* 59
discussion on case of brain abscess due to otitic infection, *Otol.* 55
of epileptiform seizures subsequent to operation for temporo-sphenoidal abscess, *Otol.* 52, 53
of left temporo-sphenoidal abscess, *Otol.* 58
on eighth nerve tumours, *Otol.* 39
on morbid anatomy and drainage of otitic meningitis, *Otol.* 46
on otosclerosis and osteitis deformans, *Otol.* 26
- Balzer's and Ménétrier's** case of epithelioma adenoides cysticum, *Derm.* 31, 32
- Bankart, A. S. Blundell.**—Discussion on operative treatment of dislocated hips, *Orth.* 25
on operative treatment of spastic paralysis, *Orth.* 33
- Banks-Davis, H. J.**—Discussion on case of otitis media with facial palsy, following scarlet fever, *Otol.* 18
discussion on case of tuberculous ulcer of dorsum of tongue, *Laryng.* 51
on operative treatment of middle-ear supuration, *Otol.* 6
on parotid fistula, following mastoid operations, *Otol.* 19
hæmorrhagic angiosarcoma of upper jaw, case, *Laryng.* 49
laceration of meatus and tympanic membrane, produced by celluloid knitting needle, *Otol.* 30
mounted specimen showing a threepenny-piece impacted in perforation between œsophagus and trachea of baby, aged 3 months, *Laryng.* 55
mounted specimen showing two foreign bodies, one movable and other fixed, in trachea of child aged 3, *Laryng.* 55

- Banks-Davis, H. J.**—Multiple papillomata of larynx, *Laryng.* 45
parotid fistula in scar of old mastoid wound, *Otol.* 30
specimen from case of multiple papillomata of nose, *Laryng.* 46
- Banting and Best**, preparation of insulin by, *Therap.* 17
- Baranoff**.—Cases illustrating eye injuries, *Ophth.* 33
- Barber, H. W.**—Case for diagnosis, *Derm.* 60
case of atrophic dermatitis of hands and feet, (?) lupus erythematosus, *Derm.* 99
of Darier's disease, *Derm.* 51
of dermatitis repens and infectious eczema-toid dermatitis, with involvement of mucous membranes, *Derm.* 98
of morphea associated with vitiligo, *Derm.* 106
of ? premycotic erythrodermia, *Derm.* 80
of urticaria pigmentosa, *Derm.* 94
discussion on case of gas-burn scarring, *Derm.* 95
- Barcroft, J., C.B.E.**—Discussion on medical aspects of life at high altitudes, *Med.* 58
- Bardsley, P.**—Case for diagnosis (? polycythæmia rubra), *Ophth.* 19
discussion on significance of vascular and other changes in retina in arterio-sclerosis and renal disease, *Med. & Ophth.* 15
- Barkas, Mary.**—Discussion on relationship between doctor and patient in psychotherapy, *Psych.* 20
- Barnes, Stanley.**—Discussion on case of muscular atrophy of peroneal type, *Neur.* 81
- Barrington, F. J. F., and Thomson-Walker, Sir J.**—Case of malakoplakia, *Urol.* 32-34
- Barris, J. D.**—Discussion on Cæsarean section, *Obst.* 59
discussion on case of inversion of uterus, *Obst.* 49, 50
two specimens of sarcoma of uterus, *Obst.* 65-67
- Basal metabolic rate**, normal, amount of active thyroxin required to maintain, *Therap.* 15
- Baskett, B. G. M.**, on economics and tuberculosis (quoted), *Epid.* 17
- Bassett-Smith, Sir P., Surg. Rear-Admiral, K.C.B.**—Discussion on venereal disease as war casualty, *War* 27
- Bath Vaccine Institution**, formation by Edward Jenner in 1800, *Occ. Lect.* 6
- Baths**, effects of, mainly a protein and vaccine therapy, *Baln.* 16
- Batten, Rayner.**—Calcareous degeneration of eye, with deposits on iris, *Ophth.* 1
discussion on cases of familial early maculo-cerebral degeneration, *Ophth.* 19
on progressive macular disease with tremors, *Ophth.* 39
on Tournay reaction, *Ophth.* 48
- Bayliss, Sir W., F.R.S.**—Discussion on vagus and sympathetic nerves and their relation to climate and hydrology, *Baln.* 11
"Nature of Enzyme Action" (quoted), *Epid.* 70, 71, 72
- Baynes, H. G.**—Discussion on relationship between doctor and patient in psychotherapy, *Psych.* 19
- Beard**, alopecia of, due to dental sepsis, *Odont.* 25
- Beattie, I. Hamilton.**—Discussion on coroners' inquests, *Anæsth.* 38
- Beef-bone graft** of humerus, late result of (M. Heath), *Orth.* 30
- Bell, K. Digby, Surg. Comm.**—Discussion on physical training, *War* 37
- Bell, Mary C.**—Use and abuse of relationship between doctor and patient in practice of psychotherapy, *Psych.* 12-19
- Belladonna group**, value in asthma, *Therap.* 6
tolerance to, of animals, idiosyncrasies of, *Therap.* 43
- Bennett, T. Izod.**—Modification of gastric function by means of drugs (abstract), *Therap.* 6, 7
- Beri-beri**, polyneuritis and, *Neur.* 19, 20
- Bernstein** "membrane theory" of nervous impulse, *Neur.* 55, 56
- Berry, J.**—Discussion on operative procedures for bilateral abductor paralysis, *Laryng.* 40
progress of surgery, and the rise and fall of surgical operations, *Surg.* 1-11
- Besredka**, researches on production of immunity: effect of site of inoculation of test organism, *Path.* 8
- Bestion**, views on yaws (quoted), *Trop.* 33
- Bett, W., Surgeon Rear-Admiral, M.V.O.**—Discussion on case of acquired chronic hæmolytic (acholuric) jaundice, *Med.* 79
venereal disease as a war casualty, *War* 15-29
- Bile**, change in character of, due to infection, influence upon X-ray diagnosis, *Electr.* 82, 83
- Bile-duct**, common, atresia of, and biliary cirrhosis in case of persistent jaundice in infant (B. Myers), *Child.* 17
- Bilharzia** disease in Portugal; endemic focus of, and specimens of intermediary host, *Planorbis dufourii* (Graells), (J. B. Christopherson), *Trop.* 47
- Biological relationship** of endocrine system, *Psych.* 21
- Biology**, connection of Pasteur's chemical studies, with, *Occ. Lect.* 19, 20
in relation to psychology, *Psych.* 34
- Birth**, blood at, constitution of, *Path.* 38
injuries, cases of, *Child.* 73-75
discussion on, *Child.* 73-78
treatment, surgical and orthopædic, *Child.* 74, 75, 77, 78
- Bismuth and glycerine gauze**, application to nasal cavities (Sir StC. Thomson), *Laryng.* 29
- Black, Dr.**—Views on caries of teeth (quoted), *Odont.* 74, 75
- Black Death** in England and Wales as exhibited in Manorial Documents (W. Rees), *Hist.* 27-45
See also under *Pestilence*, Great
- Blacker, Sir G. F.**—Discussion on case of leiomyosarcoma of fibromyoma removed by subtotal hysterectomy, *Obst.* 65
discussion on treatment of uterine hæmorrhage by radium, *Obst.* 79
- Bladder and prostate**, operations on, primary union after, advantages of, *Urol.* 50
propriety of attempting to secure primary union after (A. R. Thompson), *Urol.* 47-53
conditions contra-indicating, *Urol.* 51

- Bladder**, diverticula of, complicated by enlarged prostate, operative treatment in, *Urol.* 66
 complicated by stone, operative treatment in, *Urol.* 66
 diagnosis of, *Urol.* 68
 excision from without the bladder, *Urol.* 64
 intravesical operations, *Urol.* 65
 operative treatment of, details of 14 cases, *Urol.* 55-63
 relationship of ureter to, *Urol.* 67
 relative value of different procedures, *Urol.* 63
 residual urine in, *Urol.* 68
 splitting bladder wall down to orifice of diverticulum, *Urol.* 64
 diverticulum of, specimen (J. Everidge), *Urol.* 48
 exploration of, primary union after, case, *Urol.* 50
 growths of, subrapubic cystotomy for, primary suture after, 10 cases, *Urol.* 49
 interior of, six months after extensive resection for carcinoma with transplantation of right ureter, specimen (J. Everidge), *Urol.* 43
 of male, candle removed from (F. Kidd), *Urol.* 84
 vesicula of, operative treatment of (J. Swift Joly), *Urol.* 55-69
- Blake**, V. J.—Cases of skin diseases treated by salicylic ointment, *Derm.* 110
- Blenkinsop**, Sir A., Major-General.—Discussion on venereal disease as a war casualty, *War* 28
- Blepharitis**, age incidence of, *Epid.* 55, 56
 dental sepsis in relation to, *Odont.* 27
 measles in relation to, *Epid.* 56
- Blindness**, causes and prevention of, Departmental Committee's report quoted, *Epid.* 61
 due to infectious diseases, *Epid.* 61
 ophthalmia neonatorum, *Epid.* 60
- Blomfield**, J., O.B.E.—Discussion on anaesthetization in Cesarean section, *Anæsth.* 3
- Blood**, agglutinins in, factors in inheritance, *Path.* 37
 inheritance of groups, mode of, *Path.* 42
 tests of, rationale of grouping, *Path.* 37
 at birth, constitution of, *Path.* 38
 calcium in, diminished in active rickets, *Child.* 3
- Blood-cells**, red, human, agglutinable properties of, *Path.* 35
 number and condition of, at high altitudes, *Med.* 58
 specific isoagglutinable substances of, inheritance of, application of Mendel's law to, *Path.* 45
 possible existence of lethal factor, *Path.* 43-46
 question of fragility in pernicious anæmia, *Med.* 80
- Blood changes** in cases of anæmia with sclerosis, *Med.* 36
 in pernicious anæmia, typical cases, *Med.* 37
 compared with combined sclerosis, *Med.* 33, 34
 and septic anæmias, *Med.* 4, 5
 in two types of severe anæmia at different stages, character of, *Med.* 35, 38
 clot, method of obliteration of dental cyst after drainage, *Laryng.* 2, 3
 conditions in dental sepsis, *Odont.* 12, 13
 corpuscles. See *Blood-cells* (above)
- Blood count** in case for diagnosis (? leukæmia cutis), *Derm.* 12
 in case of acquired chronic hæmolytic (acholuric) jaundice; seen 15 years ago, at that time resembling one of pernicious anæmia (F. Parkes Weber), *Med.* 73, 77
 of chronic splenomegaly of uncertain origin, *Child.* 65
 of congenital hæmolytic jaundice, *Child.* 66
 of enlarged liver with persistent acetonuria and diaceturia, *Child.* 5, 8
 of erythræmia, *Clin.* 36, 37; *Med.* 84
 of lymphatic erythrodermia, *Derm.* 21
 of parakeratosis variegata, *Derm.* 20
 of purpura hæmorrhagica, *Clin.* 11
 of scorbutic infantilism, *Clin.* 21
 of septic anæmia associated with nervous and mental disorders, *Med.* 2
 in diagnosis of sandfly fever in Malta, *War* 8, 5
 cultures in cases of sandfly fever in Malta, *War* 2, 5
 examination in dental sepsis, *Odont.* 26
 platelets, bactericidal bodies in, *Path.* 6
 effects of exposure to radium on (J. C. Mottram), *Path.* 9-13
 pressure, diastolic and systolic estimation of, both important, *Baln.* 4
 discussion on, *Baln.* 1-6
 effects of dental sepsis on, *Odont.* 13
 exact measurements of, instruments for, *Urol.* 83
 high, anaphylaxis and, *Baln.* 3
 associated with cardio-vascular changes, *Baln.* 2
 with certain nervous disorders, *Baln.* 2
 with retinitis, compatible with good health, *Med. & Ophth.* 32
 effect on prognosis of eclampsia, *Obst.* 4
 functional type, *Baln.* 3
 in cases of retinitis with disturbance of renal function, *Med. & Ophth.* 17
 method of elimination of acid waste products, *Baln.* 2
 organic type, *Baln.* 3
 importance of pulse pressure in relation to, *Baln.* 4
 low, dependent upon nervous condition due to toxæmia, *Baln.* 3
 diseases in which found, *Baln.* 2
 indication of definite constitutional state, *Baln.* 2
 raising or lowering of, dangers of, *Baln.* 5
 systolic, high, cases of, *Baln.* 1
 in cases of arterio-sclerosis, with and without retinitis, *Med. & Ophth.* 8, 9
 studies of, effect on diagnosis of condition of kidneys, *Med. & Ophth.* 3, 5
 transfusion of, in treatment of case of purpura hæmorrhagica, *Clin.* 11
 vessels of enamel organ of *Felis domestica*, *Odont.* 48
- Blood-lævulose test** in case of enlarged liver with persistent acetonuria and diaceturia, *Child.* 58
- Bloods**, groups of, according to interaction of sera and corpuscles, *Path.* 35, 36

- "Blue bodies" in leishmaniasis of skin, *Derm.* 48, 50
- Board of Education**, connexion with physical training in the Army, *War* 36, 38, 41
- Body and mind**, endocrine system as intermediate zone between, *Psych.* 31
- Boeck's** sarcoid, case of (J. L. Bunch), *Derm.* 73
- Bond**, Hubert.—Discussion on endocrine factor in mental disease, *Psych.* 32
- Bone-conduction** usually reduced in otosclerosis, *Otol.* 22
- Bone-graft**, in treatment of traumatic osteoarthritis of neck (D. M. Aitken), *Orth.* 30
- Bone-grafting**, result of, in case of myeloma of outer condyle of femur (A. H. Todd), *Clin.* 3, 4
- Bone-necrosis**, peri-apical, term suggested in place of apical dental abscesses, *Odont.* 11
- Bone penholder**, removal of, from epigastric region, case, *Obst.* 37
- Bones**, extreme rarefaction of, in case of scorbutic infantilism, X-ray appearances, *Clin.* 18-21
- long, gunshot fractures of, disruptive phenomena, *Path.* 28
- Bonney**, Victor.—Discussion on treatment of dysmenorrhœa, *Obst.* 115
- diurnal incontinence in woman, *Obst.* 110
- scope and technique of myomectomy (abstract) *Obst.* 22
- Boock**, E., and **Trevar**, J. W.—Effect of light on response of frogs to drugs, *Therap.* 8
- Bougies** in treatment of gonorrhœal stricture of rectum, *Proct.* 18
- Bourne**, A. W.—Discussion on value of ergot, *Obst. & Therap.* 6
- Bowel**, sore, in pernicious anæmia, *Med.* 7
- Bowen's** disease and Paget's disease, differential diagnosis, *Derm.* 27
- Boyle**, H. E. G.—Discussion on anæsthesia in dental surgery, *Anæsth.* 21
- discussion on anæsthetization in Cæsarean section, *Anæsth.* 3
- on Coroners' Inquests, *Anæsth.* 41
- Boyle**, Robert.—Nature of ferments and fermentation (quoted), *Occ. Lect.* 14
- Brachial plexus**, birth injury to, all cords of plexus originally involved, recovery of function in outer and posterior cords, paresis now of infraclavicular or Klumpke type, case (C. Worster-Drought), *Child.* 73
- right, traction lesion of, involving 5th and 6th groups (A. E. M. Woolf), *Clin.* 1
- Bradwardyn**, William.—Arbitrator in case of alleged surgical malpraxis (1424), *Hist.* 8
- surgeon to Henry V.'s campaign of Somme in 1415, *Hist.* 3, 7-9
- Bradykinesia**, late effect of encephalitis lethargica, *Child.* 35
- Brain**, abscess of, due to otitic infection, right temporo-sphenoidal abscess without clinical signs (T. H. Just), *Otol.* 54
- and acoustic tumour, specimen of (F. M. R. Walshe and F. J. Cleminson), *Otol.* 32
- blood-vessels supplying, relation of arterio-sclerotic retinitis to condition of, *Med. & Ophth.* 27
- Brain**, localization of lesions in, in cases of post-encephalitic tremor, *Ophth.* 42
- mid-, lesion of, retraction of eyelids in case of (J. Collier), *Neur.* 46, 47
- sclerosis of, specimen of (C. B. Dansie), *Child.* 43
- tumour of, removed from child, aged 12 (H. S. Souttar), *Clin.* 27
- Brain**, W. Russell and **Riddoch**, G.—Case of right fronto-parietal tumour; cracked-pot percussion note over right frontal bone; left palmar reflex, *Neur.* 84
- Braine**, C. Carter.—Discussion on anæsthesia in dental surgery, *Anæsth.* 20
- Bramwell**, E.—Observations on myopathy, *Neur.* 1-12
- Branchial** cyst, case of (F. Holt Diggle), *Laryng.* 95
- Breast**, tumour of, with atrophy of skin, case of (S. E. Dore), *Derm.* 57; pathological report on (L. S. Dudgeon), *Derm.* 96
- uterus, ovary, cancer of, mortality compared (1900, 1915, 1920), *Occ. Lect.* 36
- Brewerton**, E.—Discussion on treatment of conical cornea, *Ophth.* 25
- Briggs**, H.—Angioma of vaginal wall, *Obst.* 61
- discussion on cases of sarcoma of uterus, *Obst.* 65
- on myomectomy for uterine fibroids, *Obst.* 23
- on value of ergot, *Obst. & Therap.* 6
- section of curettings, *Obst.* 61
- Bristow**, W. Rowley.—Discussion on operative treatment of spastic paralysis, *Orth.* 40
- transplantation of hamstrings, *Orth.* 13
- British Pharmacopœia**, present position of ergot in, and its value in obstetrical and gynecological practice (H. H. Dale, with discussion), *Obst. & Therap.* 1-7
- Red Cross Society, educational work in control of cancer, willingness to undertake, *Occ. Lect.* 36
- Broad** ligament, right, pin embedded in, *Obst.* 43
- Bronchial** asthma, case (O. C. M. Davis), *Child.* 81
- Bronchitis**, danger of, after Cæsarean section, *Obst.* 50, 51
- mortality from, among coal-miners (ages 25-64), period 1890-1912, *Epid.* 88
- Bronchus**, left, of child, paper fastener in, skiagram showing (H. Tilley), *Laryng.* 20
- Brooke's** disease (epithelioma adenoides cysticum), *Derm.* 30, 31
- Brooke**, H. G.—original description of cases of epithelioma adenoides cysticum (quoted), *Derm.* 31, 39, 40
- Broughton-Alcock**, W.—case of spirochætal dysentery, *Trop.* 46
- Brown**, Graham.—Discussion on after-treatment of empyema of maxillary antrum, *Laryng.* 86
- Brown**, H. H., O.B.E.—Patient upon whom an operation was performed in June 1920, for cancer of rectum, by abdomino-anal method, *Proct.* 89
- Brown**, W. Langdon.—Discussion on present position of organotherapy, *Therap.* 23
- factors in uræmia, *Urol.* 19-27
- problems of asthma, *Therap.* 1-6

- Brownlee, J.**—Relationship between rainfall and scarlet fever, *Epid.* 30, 34
- Bruce, N.**—Experiments with mustard oil (quoted), *Baln.* 12
- Bruce, R.**—Case of acne agminata, *Derm.* 16
- Brydone, J.**—Discussion on treatment of dysmenorrhœa, *Obst.* 116
- Bubonic plague.** See *Plague*, bubonic
- Buchanan, Sir G.**—Discussion on relationship between rainfall and scarlet fever, *Epid.* 34
- Budge, C. H. and Dyke, S. C.**—The inheritance of the specific isoagglutinable substances of human red cells, *Path.* 35, 46
- Bullets,** disruptive injuries due to, physics of; see under *Gunshot injuries and Rifle-bullet*
- Bullous eruption,** case of (J. H. Sequeira), *Derm.* 55
- Bunch, J. L.**—Case of adenoma sebaceum in girl aged 10, *Derm.* 80
of Boeck's sarcoid, *Derm.* 73
of trichorrhæxis nodosa, *Derm.* 74
of xantho-erythrodermia perstans, *Derm.* 81
- Burgess, A. H.**—Discussion on technique of resection and anastomosis of colon, *Surg.* 83
- Burnett, Sir Napier.**—Discussion on urgent need for education in control of cancer, *Occ. Lect.* 37
- Burns,** blistering effects of, prevented by suggestion, *Baln.* 15
keloid after, case of (E. G. Graham Little), *Derm.* 61
- Burrows, A.**—Methods of application of radium (cases shown), *Laryng.* 96
- Butler, T. Harrison.**—Discussion on case of ectopia lentis, *Ophth.* 13
discussion on standards of vision for scholars and teachers, *Ophth.* 10
some unusual results of operations for cataract, *Ophth.* 21-24
- Buxton, St. J. D.**—Case of chondroma of phalanx in hand, *Clin.* 27
thrombo-angitis obliterans, *Clin.* 14, 15
- Buzzard, E. F.**—Discussion on neuro-syphilis, *Neur.* 61, 77
- Cachectio causes of polyneuritis,** *Neur.* 18
- Cæcostomy** in treatment of gonorrhœal stricture of rectum, *Proct.* 19
of ulcerative colitis, *Proct.* 95
tube method of, *Surg.* 70, 76
- Cæcum,** changes in, due to exposure to radium, *Electr.* 42
drainage of, in axial anastomosis of colon, *Surg.* 75, 76
simple, without laparotomy, in treatment of obstruction of colon, *Surg.* 70
- Cæsarean section,** anæsthetization of patients for (H. R. Spencer), *Anæsth.* 1
method of administration of chloroform and ether, *Anæsth.* 2
preliminary administration of scopolamine, morphine or atropine, *Anæsth.* 3, 4
adhesions in, treatment of, *Obst.* 51, 52
anæsthetic in, choice of, *Obst.* 50
- Cæsarean section,** combined with myomectomy in treatment of fibroid, complicated by pregnancy, *Obst.* 22
dangers arising from sepsis, *Obst.* 51
in case of sudden onset of hemiplegia occurring in pregnant woman at full term, accompanied by transient albuminuria; gradual recovery, case (F. Cook), *Clin.* 43
line of incision in, *Obst.* 51
pulmonary complications after, *Obst.* 50, 51
suture materials after, *Obst.* 53
technique of (S. J. Cameron), *Obst.* 50-54
under spinal anæsthesia with tropacocaine, two cases (B. Whitehouse and H. Featherstone), *Obst.* 55-58
- Calcified abdominal glands.** relation of, to urinary surgery (Sir J. Thomson-Walker), *Urol.* 117
See also under *Lymphatic glands, abdominal*
- Calcinosis,** case of (abstract), (F. Langmead), *Clin.* 23
- Calcium,** all forms of, not equally available for metabolism, *Child.* 3
deficiency as index of absorption of toxin, *Therap.* 19, 20, 22
effect of parathyroid extract on, *Therap.* 19
in blood, diminished in active rickets, *Child.* 3
- Calculi,** large (two), removed from perineum of male aged 62 (W. G. Sutcliffe), *Urol.* 36
malignant growth of renal pelvis with, case (Sir J. Thomson-Walker), *Urol.* 85-87
renal, origin and treatment of, subcapsular pyelotomy in relation to (W. S. Handley), *Surg.* 21-37
cyst in cortical substance, first step in formation of, *Surg.* 21
extrusion of calyx calculi through renal cortex, *Surg.* 33
originating in cyst of cortex, cases illustrating, *Surg.* 21, 22, 23
removal of, best method of, *Surg.*, 23 *et seq.*
bidigital exploration of kidney in, *Surg.* 31-33
by bipolar nephrotomy (Legueu), *Surg.* 24
by nephrolithotomy, *Surg.* 24
by pyelotomy, *Surg.* 25
by restricted nephrolithotomy, *Surg.* 24
by subcapsular pyelotomy, case records, *Surg.* 34-37
by subcapsular pyelotomy, risk of, *Surg.* 33
by unipolar nephrolithotomy, *Surg.* 25
submaxillary gland with (H. B. Tawse), *Laryng.* 22
- vesical,** complicating adenoma of prostate, suprapubic prostatectomy for, primary union after, three cases, *Urol.* 48, 49
suprapubic cystotomy for, primary suture after, case, *Urol.* 49
- Calculous pyonephrosis ten years after double nephro-lithotomy,** case (P. Turner), *Clin.* 40
- Calculus formation,** massive, in kidney (J. Macalpine), *Urol.* 38
free in bladder, complicating vesical diverticula, operative treatment, *Urol.* 66
renal, large (R. Ogier Ward), *Urol.* 38
salivary, large, submaxillary gland containing (Dan McKenzie), *Laryng.* 7
vesico-urethral, case (Sir J. Thomson-Walker), *Urol.* 87

- Calomel**, susceptibility of cattle and horses to, *Therap.* 44
- Calorie** value of food intake, equality to physiological output, result of, *Child.* 4, 5
excess over physiological output, result of, *Child.* 5, 6
- Calyx** calculi, extrusion of, through renal cortex, *Surg.* 33
- Cameron, H. C.**—Cases of encephalitis lethargica, showing late results, *Child.* 30
discussion on late effects of encephalitis lethargica, *Child.* 38
- Cameron, S. J.**—Technique of Cæsarean section, *Obst.* 50-54
- Campbell, R. C., D.S.O.**—Physical training in the army, *War* 31-37
- Cancer**, conditions predisposing to, *Occ. Lect.* 30
control of, American Society for, methods of, *Occ. Lect.* 31
education in, co-operation of Ministry of Health and Insurance Societies, *Occ. Lect.* 33
methods of, *Occ. Lect.* 31, 35
resolutions proposed, *Occ. Lect.* 33
to whom should instruction be given, *Occ. Lect.* 30
urgent need for (J. E. Adams), *Occ. Lect.* 29-33
what can be done, *Occ. Lect.* 32
work already done, *Occ. Lect.* 32
diagnosis, education of students in, *Occ. Lect.* 30, 34, 36
mortality from, in 1880 and 1920 compared, *Med.* 47
reduction by means of propaganda, *Occ. Lect.* 38
of cervix uteri treated by radium before operation, *Obst.* 34, 35
two cases of (T. W. Eden and A. Goodwin), *Obst.* 32
of ovary, unilateral, solid, ruptured; ovariectomy, no recurrence six years later (H. R. Spencer), *Obst.* 105
of rectum, operated upon by abdomino-anal method in June, 1920, present condition of patient (H. Brown), *Proct.* 89
proportionate mortality from, among coal and metalliferous miners (1910-1912) according to parts affected, *Epid.* 93
ulcerating and inoperable, parathyroid medication in, *Therap.* 21
see also *Carcinoma*
- "Cancerophobia,"** *Occ. Lect.* 32, 34, 36
- Canfield's** operation in case of chronic empyema of antrum, recovery (Sir J. Dundas-Grant), *Laryng.* 20
- Canines** and incisors, unerupted, in male aged 59, case (G. Harborow), *Odont.* 73, 74
- Cannula** and trocar, antrum-exploring, improved form of (H. M. Wharry), *Laryng.* 53
- Canti, R. G.**—Observations on urea retention, *Urol.* 25
- Cantrell, B. W. and Gunewardene, H. O.**—Simple instrument for withdrawing serous effusions, *Clin.* 38, 39
- Carbohydrates**, in diet, in production of dental caries, *Odont.* 76
separation from proteins during sterilization, new fermentation tube for (C. Dukes), *Path.* 18-16
- Carbon** bisulphide causing polyneuritis, *Neur.* 14
monoxide causing polyneuritis, *Neur.* 14
- Carcinoma** faciei apud puellam, case of (W. J. O'Donovan), *Derm.* 87
multiple, two cases of (J. H. Sequeira), *Derm.* 23, 24
of adrenal "rest" in liver, case (C. A. R. Nitch), *Surg.* 64
of antrum, removal of upper jaw, case (A. A. Smalley), *Laryng.* 94
of cervix, uterus removed for, after treatment by radium (A. H. Richardson), *Obst.* 31
of face, squamous, in woman, aged 24 (W. J. O'Donovan), *Derm.* 52
of liver, primary, excised by operation, case (G. Wright), *Surg.* 56
primary, hepatectomy for, *Surg.* 62
of prolapsed cervix in woman aged 77 (H. R. Andrews), *Obst.* 109
of rectum, inoperable, case appearing clinically as, treated by colostomy and subsequent injections of cuprase-collosal selenium and colossal cuprum for over 2 years, with disappearance of growth (L. E. C. Norbury), *Proct.* 67
of vagina, primary, specimen of (E. Holland), *Obst.* 25
see also *Cancer*
- Cardiac** arrest under anæsthetic followed by heart massage, case (E. S. Rowbotham), *Anæsth.* 5
conditions arising from dental sepsis, *Odont.* 13
diagnosis, survey of development of physical methods (R. Knox), *Electr.* 1-30
electrocardiograph in, *Electr.* 4
X-rays in, *Electr.* 6
disease, see *Heart disease*
- Cardio-vascular** changes combined with renal disease, fatality of, *Med. & Ophth.* 33
high blood-pressure associated with, *Bahn.* 2
in retinitis, psycho-pathology with reference to, *Med. & Ophth.* 33
conditions contra-indicating antisypilitic treatment, *Neur.* 64, 72
- Carles**, dental, effect of diet on resistance to (May Mellanby), *Odont.* 74-82
experimental work to determine, *Odont.* 75, 76
summary, *Odont.* 79-82
effect of diet before and after eruption of teeth in relation to, *Odont.* 77
formation of adventitious dentine in, *Odont.* 65
in teeth of normal and hypoplastic structure compared, *Odont.* 77
progress influenced by defective deciduous teeth, *Odont.* 76, 77
- Carmichael, E. A.**—Case of chronic mercurial poisoning, *Neur.* 80
- Carnwath, T.**—Discussion on age and sex distribution in scarlet fever, *Epid.* 30
- Carrel's** tubes, control of sepsis in uterus by, *Obst.* 50
- "Carriers,"** administrative treatment of, Scottish Board of Health regulations, *Epid.* 9
enteric fever due to (F. Dittmar) (abstract), *Epid.* 1-10
in relation to progress of epidemiology, *Epid.* 42
- Cartilage** formation in tonsils, summary of observations recorded, *Laryng.* 16

- Cassidy, M. A.**—Case of patent interventricular septum, *Clin.* 4
of scorbutic infantilism, *Clin.* 16
- Castellani, A., C.M.G.**—Case of trichomycosis axillaris rubra, *Derm.* 97
peculiar folliculitis of scalp, *Derm.* 97
views on yaws quoted, *Trop.* 38
- Castle, W. F. R.**—Case of epidermolysis bullosa, *Derm.* 53
- Cataract**, diabetic, prognosis, *Ophth.* 24
dystrophia myotonica, an heredito-familial disease with (W. J. Adie), *Neur.* 36, 43
in dystrophia myotonica, importance of, *Neur.* 39
operations for, some unusual results of (T. H. Butler), *Ophth.* 21, 24
senile, origin of, theories of, in relation to dystrophia myotonica, *Neur.* 43
- Cataractous lens**, total absorption of, case (T. H. Butler), *Ophth.* 22
- Catgut** as suture material in Cæsarean section, *Obst.* 53
- Cathcart, G. C. and Patterson, N.**—Tuberculoma of pharynx, *Laryng.* 51
- Catheter**, indwelling, use of, in urinary fistula, after suprapubic prostatectomy, *Surg.* 121
- Catheterization**, retrograde, suprapubic cystotomy for, primary union after, *Urol.* 50
- Cattle**, effects of cocaine upon, *Therap.* 47
of strychnine upon, *Therap.* 47
large doses of medicinal agents required for, *Therap.* 44
- Cauterization** of adhesions under guidance of thoracoscope, critical survey of operation, *Electr.* 54-60
indications for, *Elect.* 51, 53, 54
technique, *Elect.* 47, 48
- Cautley, E.**—Duodenal stenosis (quoted), *Child.* 10
- Cavenagh, J. B.**—Discussion on case of outgrowth from ventricle in subject of pulmonary tuberculosis, *Laryng.* 55
discussion on improved antrum-exploring trocar and cannula, *Laryng.* 53
on operative procedures for bilateral abductor paralysis, *Laryng.* 39
- Cavernous sinus thrombosis**, non-suppurative, exophthalmos probably caused by, case (F. Parkes Weber), *Clin.* 41
- Cellulitis** of face, chronic, case (G. E. Archer), *Laryng.* 94
orbital, invasion of frontal sinus, osteo-myelitis of frontal bone, case (Sir W. Milligan and F. Wrigley), *Laryng.* 90
recurrent, case of (H. G. Adamson), *Derm.* 79
- Cerebellum**, abscess of, ependymal glioma growing from floor of fourth ventricle simulating, in case of bilateral chronic suppurative otitis media, section of (T. H. Just), *Otol.* 62
five weeks after onset of acute otitis media, right side, case (S. Scott), *Otol.* 57
sudden coma and apnoea, recovery after operation during artificial respiration, case (S. Scott), *Otol.* 56
lateral lobe of, compression of anterior part of, symptoms due to, *Otol.* 35
- Cerebral** and mental features in case of pernicious anæmia, *Med.* 31
degeneration, case of (H. Ingleby), *Child.* 15
hemorrhage, arterio-sclerosis not definitely correlated with, *Med. & Ophth.* 28
in case of erythræmia (J. A. Ryle), *Med.* 83
nervous system, central, disturbances of, with pernicious anæmia, features of, *Med.* 42
system, general nervous disturbances of, in septic anæmia, *Med.* 24
- Cerebro-macular** degeneration, macula in case of, illustrations, *Ophth.* 40, 41
disease, adult type suggested, *Ophth.* 42
- Cerebro-spinal fluid** in neuro-syphilis, alterations in, *Neur.* 67
pituitary secretion in, *Therap.* 23
Wassermann reaction of, in series of cases of syphilis before and after treatment, *Neur.* 67
- Cervical region**, injuries in, in traumatic spondylitis, *Orth.* 2
- Cervix.** See under *Uterus*
- Chaldecott, J. H.**—Discussion on Coroners' Inquests. The classification of deaths under anæsthetics as violent or unnatural, *Anæsth.* 33-38
- Chalmers, A. K.**—Discussion on economics and tuberculosis, *Epid.* 17
on mortality of coal and metalliferous miners, *Epid.* 100
- Charcot's knee (?)** case of (H. A. T. Fairbank), *Orth.* 47
- Charlouis' views** on yaws, quoted, *Trop.* 31, 32
- Cheate, Sir G. Lenthal, K.C.B.**—The sites of origin and methods of growth of fibroadenomata of breast, *Surg.* 85
- Chemist**, Muhammadan, Jābir ibn Hayyān most celebrated, *Hist.* 47
- Chest** measurements in those living at high altitudes, *Med.* 60
- Chicken** cholera, Pasteur's investigations into, *Occ. Lect.* 13
- Chicken-pox**, abnormal scarring after, case (R. T. Smith), *Derm.* 82
epidemics of, association of herpes with, *Ophth.* 29
- Chilblains**, treatment by dry thyroid, *Therap.* 15
- Childe, C. P.**—Discussion on urgent need for education in control of cancer, *Occ. Lect.* 34-36
- Childhood**, dental sepsis in, results of, *Odont.* 24
- Children**, anæsthetic for, in dental surgery, *Anæsth.* 13, 20
teeth of, importance of sufficient sleep in regard to, *Odont.* 5
young, acute nasal sinus disease in (E. Watson-Williams), *Child.* 81-84
- Chill**, intense, causing multiple neuritis, *Neur.* 14
- China**, origin of great pestilence in, in 1333, *Hist.* 27
- Chloretone** causing polyneuritis, *Neur.* 14
- Chloroform**, action of, on heart (A. G. Levy), *Anæsth.* 30, 31
as anæsthetic in Cæsarean section, method of administration, *Anæsth.* 1, 2
syncope, *Anæsth.* 30, 31
- Cholæmia**, congenital family, accompanied by anæmia with enlargement of spleen, *Med.* 80

- Cholæmia**, accompanied by jaundice with enlargement of liver, *Med.* 80
- Cholesterolæmia**, present in both xanthoma tuberosum and xanthoma diabeticorum, *Derm.* 93-94
- Chondroma** of phalanx in hand, case (St. J. D. Buxton), *Clin.* 27
- Chorea**, Huntington's, case of (C. Worster-Drought), *Neur.* 82
- Choroid**, retinitis circinata originating in, *Ophthalm.* 11, 12
- Choroiditis**, tubercular, *Ophthalm.* 2
- Choroido-retinitis**, cerebro-macular, *Ophthalm.* 39
forms of, classified, *Ophthalm.* 39
- Christopherson**, J. B., O.B.E.—Case of leishmaniasis of skin, *Derm.* 8
leishmaniasis of skin resembling lupus vulgaris, *Derm.* 48
remarks on photograph of endemic focus of bilharzia disease in Portugal; specimens of intermediary host, *Planorbis dufourii* (Graella), *Trop.* 47
specimens of *Schistosoma bovis* and its snail carrier; intermediate host of *Schistosoma mansoni*, *Trop.* 56
- Chronaxie** of nerve fibre, *Neur.* 59
- Circulatory** system in dental pulp, *Odont.* 62
- Cirrhosis**, biliary, and atresia of common bile-duct in case of persistent jaundice in infant (B. Myers), *Child.* 17
- Cisterna magna**, injection of salvarsanized serum into, in treatment of general paralysis, *Neur.* 69, 72
tumour of (J. P. Martin and J. G. Greenfield), *Neur.* 32-35
- Cisternal** puncture, technique of, *Neur.* 70, 72
- Claremont**, H. E.—Case of mycosis fungoides, *Clin.* 34
- Clark-Kennedy**, A. E.—Discussion on action of quinidine in cases of cardiac disease, *Therap.* 32
- Clarke**, E.—Discussion on significance of vascular and other changes in retina in arterio-sclerosis and renal disease, *Med. & Ophthalm.* 22
discussion on standards of vision for scholars and teachers, *Ophthalm.* 9
- Clarke**, J. J.—Note on molluscum contagiosum, *Derm.* 3
- Clavicle**, right, subluxation of inner end of (P. M. Heath), *Clin.* 12
- Cleminson**, F. J.—Case of cerebellar abscess operated on under artificial respiration, *Otol.* 59
of otitis media with facial palsy, following scarlet fever; specimen (malleus and incus) shown, *Otol.* 17
discussion on cases of deafness due to falls, *Otol.* 49
on cases of otosclerosis with unusual symptom, *Otol.* 11
and **Walsh**, F. M. R.—Case of acoustic tumour (right): operation by Sir V. Horsley in 1912; removal of tumour; recovery, *Otol.* 31, 32
- Climate** and hydrology, sympathetic and vagus nerves and their relation to, discussion on, *Baln.* 7-17
- Climate**, influence of, upon mortality rate of coal-miners, *Epid.* 89, 101
- Clonic** spasm of palate, case (D. McKenzie), *Laryng.* 57
- Club** fingers, unassociated with pulmonary or cardiac disease in those living at high altitudes, *Med.* 60
- Coal** dust, inhalation of, influence in lowering incidence of phthisis among coal-miners, *Epid.* 92, 98
- Coalfields**, different, mortality from phthisis per 1,000 living at various age periods, *Epid.* 91
geological formation of, influence upon phthisis mortality of miners, *Epid.* 90
various, mortality from alcoholic diseases in, compared, *Epid.* 92, 93
- Coal-** and metalliferous-miners in England and Wales, mortality of (E. L. Collis), *Epid.* 85-99
summary and references, *Epid.* 99
- Coal-miners**, ages 25-64 inclusive, comparative mortality from certain causes, 1890-1912, *Epid.* 88
mortality from cancer among, *Epid.* 93
see also under *Mines and Miners*
- Coates**, V.—Case of acholuric jaundice, *Clin.* 28
case of cysticercus cellulose, *Clin.* 28
discussion on case of patent ductus arteriosus and mitral disease, *Child.* 49
- Cocaine**, application to skin, neutralizing effect of application of mustard oil, *Baln.* 12
effects of, upon animals and man compared, *Therap.* 46
- Cockayne**, E. A.—Case of encephalitis lethargica, showing late results, *Child.* 32
- Coeliac** infantilism in convalescent (non-diarrhoeic) stage, two cases (R. Miller), *Child.* 21-23
- Cole**, P.—Specimen of colon, showing multiple perforations resulting from dysentery, *Surg.* 67
- Colectomy**, total, *Surg.* 80
- Coleman**, F.—Discussion on anaesthesia in dental surgery, *Anæsth.* 21
- Colitis**, dental sepsis in relation to, *Odont.* 14, 18
septic, underlying septic anæmia, *Med.* 6
etiology, *Proct.* 92, 96
and true dysentery, not necessarily caused by same specific organism, *Proct.* 105
bacteriology of, *Proct.* 92, 98, 102
chronic, due to secondary infection, case illustrating, *Proct.* 97
clinical features, *Proct.* 94, 106
commencement as solitary follicle, *Proct.* 97
complications of, *Proct.* 93
devitaminized diet as factor favouring infection, *Proct.* 92
diagnosis of, methods for, *Proct.* 105
differential diagnosis of chronic cases from other conditions, *Proct.* 94
discussion on, *Proct.* 91-110
etiology of, *Proct.* 92, 96
mortality of, high, *Proct.* 94
recently reduced, reasons for, *Proct.* 97
natural healing of ulcers in, *Proct.* 99
of asylums, *Proct.* 91, 92
origin of infection considered, *Proct.* 103
prognosis of, with and without operation, *Proct.* 99
- ulcerative, agglutination and other immunity tests of little value, *Proct.* 103

- Colitis**, ulcerative, references, *Proct.* 96
 scope of term, *Proct.* 91
 syndrome of clinical manifestations and anatomical changes, *Proct.* 92
 treatment of, *Proct.* 94, 96, 101, 103
 by anti-dysenteric serum, *Proct.* 107
 by drugs, *Proct.* 95
 by vaccines, *Proct.* 101, 103
 dietetic, *Proct.* 94
 operative forms of, *Proct.* 99
 typical dysenteric stools exceptional in, *Proct.* 105
- Colley, R.**—Cases illustrating various congenital defects, *Ophth.* 33
- Collier, J.**—Case of scleroderma, *Neur.* 30
 case of spondylose rhizomélisque, *Neur.* 47
 discussion on case of unilateral affection of cranial nerves 9 to 12 associated with chronic otitis media, *Neur.* 53
 three cases showing retraction of eyelids, *Neur.* 46
- Collingwood, F. W.**—Case of death immediately after discontinuance of anæsthetic, *Anæsth.* 32
- Collins, E. Treacher.**—Discussion on case of amaurotic family idiocy, *Ophth.* 18
 discussion on cases of endothelioma of orbit, *Ophth.* 37
 on case of hole in hyaloid, *Ophth.* 21
 on cases of tumours of optic nerve, *Ophth.* 34
 on unusual results of operations for cataract, *Ophth.* 24
- Collis, E. L.**—Inquiry into mortality of coal- and metalliferous-miners in England and Wales, *Epid.* 85-99
- Colnet, Nichol.**—Physician to Henry V, *Hist.* 3
- Colon**, anastomosis of, axial technique, *Surg.* 73-76
 without exposing mucous membrane, methods of, *Surg.* 76
 Gudin's and Martel's methods, *Surg.* 77, 78
 changes in, due to exposure to radium, *Electr.* 42
 dilatation, congenital. See *Hirschsprung's disease*
 ends of, methods of closing, *Surg.* 78, 79
 lavage of, by Plombières douche, *Baln.* 22
 with antiseptics in treatment of ulcerative colitis, *Proct.* 95
 multiple perforations of, resulting from dysentery, specimens showing, *Proct.* 67
 obstruction of, treatment by simple drainage of cæcum without laparotomy, *Surg.* 70
 pelvic, lower end of, anastomosis near, *Surg.* 80
 resection and anastomosis of, for tumour, technique of (J. P. Lockhart-Mummery), *Surg.* 69-81
 careful preliminary preparation of patient important, *Surg.* 70
 developments during last 20 years, *Surg.* 69
 incision for, *Surg.* 71
 methods of joining after, *Surg.* 72
 streptococcal infection of dental origin, *Odont.* 11
 transverse, change in position of proximal portion of, diagnostic sign of pathological gall-bladder, *Electr.* 79
- Colon**, wall of, blood supply of, effect upon anastomosis, *Surg.* 72
- Celostomy** in treatment of gonorrhœal stricture of rectum, *Proct.* 19
- Colyer, Sir Frank.**—Discussion on dental sepsis, *Odont.* 31
- Coma**, effect upon prognosis of eclampsia, *Obst.* 3
 in uræmia, significance and causation of, *Urol.* 24
- Conduct**, human, endocrine system in relation to, *Psych.* 23
 mechanism of, disorder of, preceding hysterical syndrome, *Psych.* 1
- Conjunctivitis**, age incidence showing decline of liability to, in children, *Epid.* 55
 chronic dental sepsis in relation to, *Odont.* 27
 due to Morax-Axenfeld bacillus, prevalence among adults, *Epid.* 57
 rarity among infants and school children, *Epid.* 57
 prevalence, seasonal influence in relation to, *Epid.* 59
 purulent, commonest in new-born infants, *Epid.* 60
 tubercular, *Ophth.* 4
 treatment by peritomy, *Ophth.* 5, 6
 various forms of, sex incidence, *Epid.* 58
- Consanguinity**, case of (E. C. Williams), *Child.* 79
- Contagious Disease Acts**, various, history of and observations on, *War* 22-25
- Convalescents**, physical training of, *War* 34, 38, 41
- Convergence** and glare, *War* 49
 in India, investigation into, *War* 50
 accommodation and, *War* 49
- Convulsions** in uræmia, significance and causation of, *Urol.* 23
- Cook, F.**—Hemiplegia occurring in pregnant woman at full term; sudden onset accompanied by transient albuminuria; Cæsarean section; gradual recovery, *Chin.* 43
- Copeman, S. M., F.R.S.**—Discussion on the ultra-visible viruses, *Epid.* 78
 discussion on urgent need for education in control of cancer, *Occ. Lect.* 37
- Cordectomy**, in treatment of stenosis of larynx caused by bilateral abductor paralysis, *Laryng.* 33
- Cordopexy**, glottic space obtained by, if crico-arytenoid joint is motile, and if fixed, *Laryng.* 37
 in treatment of stenosis of larynx caused by bilateral abductor paralysis, *Laryng.* 35
 showing thyro-fissure followed by antero-lateral transplantation of vocal cord, *Laryng.* 37
- Cornea**, conical, treatment of (abstract), (C. Killick), *Ophth.* 24, 25
- Corneæ**, both, opacity of, band-shaped, primary, two cases of (A. C. Hudson), *Ophth.* 31
- Coronary arteries**, diseased, associated with angina pectoris, first account of, by Edward Jenner, *Occ. Lect.* 3
- Coroners' Inquests.**—The classification of deaths under anæsthetics as violent or unnatural, discussion on, *Anæsth.* 33-38
 substitution of inquiry by expert committee instead of coroner advocated, *Anæsth.* 38

- Cotton, T. F.**—Discussion on action of quinidine in cases of cardiac disease, *Therap.* 38, 40
- Coué system, rationale of, Psych. 18**
- Coutts, F. J. H.**—Discussion on economics and tuberculosis, *Epid.* 18
- Cow, Graves' disease and thyroid instability in, relation of ovarian disease to, Obst. 92, 99
 "nymphomania" in, nature of, *Obst.* 92, 93
 see also *Hendon cow disease***
- Cows, gynaecological disorders in, frequency of, Obst. 97**
- Crampton, H. P.**—Discussion on Coroners' Inquests, *Anesth.* 40
- Cranial nerve paralysis, following pharyngeal diphtheria, Neur. 21
 nerves 7, 9, 10, 11, and 12, unilateral affection of, case (C. P. Symonds), *Neur.* 52
 9, 12, unilateral affection of (Tapia's syndrome), associated with chronic otitis media, case (C. P. Symonds), *Neur.* 53**
- Cretinism, case of (F. J. Poynton), Child. 43**
- Cribb, H. E.**—Discussion on dental sepsis, *Odont.* 30
- Crico-arytænoid muscle, left, implication in case of myotonia atrophica (H. Tilley), Laryng. 18**
- Cridland, B.**—Discussion on cases of endo-thelioma of orbit, *Ophth.* 37
- Crimean War, venereal disease in, War 16**
- Crossen, summary of records of instruments left in peritoneal cavity (quoted), Obst. 38**
- Crying and laughing, pathological (S. A. K. Wilson), Psych. 39**
- Culpin, M.**—Discussion on organic basis of hysterical syndrome, *Psych.* 10
- Cumston, C. G.**—Discussion on incidence of malignant disease in the apparently benign enlargement of prostate, *Urol.* 77
 two points in connexion with chronic nephritis, *Urol.* 81-84
- Cunningham, J. F.**—Tumour of right upper lid (angioma) *Ophth.* 15
- Curettings of uterus, section of (H. Briggs), Obst. 61**
- Cutis verticis gyrata, case (M. G. Hannay), Derm. 88**
- Cutlers, mortality from phthisis per 1,000 living at various age periods, Epid. 90**
- Cyanosis resembling that of pneumonia in those living at high altitudes, Med. 60**
- Cyst, bronchial, case of (F. Holt Diggle), Laryng. 95
 of uterine cornu due to dilatation of interstitial portion of tube (J. S. Fairbairn), *Obst.* 45
 of uvula (T. J. Faulder), *Laryng.* 25**
- Cysts, dentigerous, multiple, cases of (B. Grellier), Odont. 43, 44; (J. Howard Mummery), *Odont.* 44-47**
- Cysticercus cellulose, case of (V. Coates), Clin. 28**
- Cystinuria, congenital, two cases of (D. Paterson), Child. 27**
- Cystograms in diagnosis of vesical diverticula, Urol. 68**
- Cystoscopic diagnosis of pelvic tumour, Surg. 88**
- Cystoscopy in diagnosis of vesical diverticula, Urol. 68**
- Cystotomy, preliminary, in vesical diverticula, dangers of, Urol. 63**
- Cystotomy, suprapubic, advisability of suturing bladder in certain cases of, Urol. 47
 line of incision in, *Urol.* 52
 primary union after, table showing cases of, *Urol.* 48
 primary union in, reasons for, *Urol.* 53
 technique of, *Urol.* 51**
- Dale, H. H., C.B.E.**—Value of ergot in obstetrical and gynaecological practice; with special reference to its present position in the British Pharmacopœia, *Obst. & Therap.* 1-7
- Dally, J. Halls.**—Discussion on blood pressure, *Baln.* 3
- Dansie, C. B.**—Specimens of (1) Still's disease; (2) sclerosis of brain; (3) congenital honeycomb lung; (4) tuberculous spleen; (5) hip-joint from case of lymphatic leukæmia, *Child.* 43
- Darier's disease, case of (H. W. Barber), Derm. 51**
- Davies, A. T.**—Note on Thomas Davies, introducer of the exploring needle, *Hist.* 19
- Davies, D. Leighton.**—Discussion on case of ectopia lentis, *Ophth.* 13
 discussion on significance of vascular and other changes in retina in arterio-sclerosis and renal disease, *Med. & Ophth.* 26
- Davies, Thomas, introducer of exploring needle, note on (A. T. Davies), Hist. 19**
- Davis, A.**—Case of multiple exostoses, *Orth.* 26
- Davis, E. D. D.**—Discussion on case of chronic laryngitis of long standing, *Laryng.* 24
 discussion on case of myasthenia gravis in which throat symptoms were an early sign, *Laryng.* 17
 of ventriculo-chordeotomy for double abductor paralysis, *Laryng.* 47
 on cases of deafness due to falls, *Otol.* 50
 on infections of teeth and jaws in their relationship to nose, throat and ear, *Odont.* 41
 on laryngeal case, apparently of epithelioma, completely healed and arrested under X-rays without operation, *Laryng.* 62
 morbid anatomy and drainage of otitic meningitis, *Otol.* 43
 specimen, post mortem, of large cyst of orifice of larynx arising from aryteno-epiglottidean fold, *Laryng.* 54, 70
 temporal bone from case of tuberculous lateral sinus thrombosis and extra-cerebellar abscess, *Otol.* 5
- Davis, Haldin.**—Case of angiokeratoma, *Derm.* 29
 of epidermolysis bullosa, *Derm.* 2
 of pityriasis lichenoides chronica, *Derm.* 92
 of scleroderma, *Derm.* 29
 discussion on cases for diagnosis, *Derm.* 62
 on manganese as chemotherapeutic agent, *Derm.* 68
 on outbreak of alopecia, *Derm.* 100
 psoriasis of anomalous type, *Derm.* 72
- Davis, O.C.M.**—Case of bronchial asthma, *Child.* 81
 clubbing of fingers and toes, *Child.* 81
 congenital absence of iris in both eyes, *Child.* 81

- Davis, O.C.M.**—Case of encephalitis lethargica, followed by symptoms of paralysis agitans, *Child*. 81
 of ichthyosis and congenital heart lesion associated with small pulse, *Child*. 80
 clinical significance of certain urinary conditions in childhood, *Child*. 84
 three cases exhibiting mongolism, *Child*. 80
- Dawson, G. W.**—A woman whose larynx can be examined by direct method with aid of tongue depressor only, *Laryng*. 20
 congenital webbing of larynx, case, *Laryng*. 20
 discussion on case of multiple papillomata of nose, *Laryng*. 46
 infiltration and ulceration of epiglottis and right aryepiglottic fold, *Laryng*. 58
- Dawson of Penn, Lord, G.C.V.O.**—Discussion on urgent need for education in control of cancer, *Occ. Lect.* 35
- Deaf (The)**, new instrument for assisting (Marconi otophone), (W. M. Mollison), *Otol.* 51
- Deafness**, associated with osteitis deformans, nine cases described, *Otol.* 23
 becoming worse after each successive pregnancy, *Otol.* 11
 bilateral, absolute, with almost complete loss of vestibular activity, case (A. Ryland), *Otol.* 7
 complete, dating from a fall, case of (Sir J. Dundas-Grant), *Otol.* 47
 of left ear, case of acute suppuration in opposite ear, subjected to early operation on account of (Sir J. Dundas-Grant), *Otol.* 6
 decrease, of immediately before parturition, *Otol.* 11
 functional and organic, malingering and, *Otol.* 12, 13
 greatly increased after a fall, case of, (Sir J. Dundas-Grant), *Otol.* 48
 long-standing, attributable to falls on the head, case of, improvement (Sir J. Dundas-Grant), *Otol.* 49
 malingering in, stages of, *Otol.* 12, 13
 "The disease of not listening, the malady of not marking." (T. B. Layton), *Otol.* 12
- Deer (Napu mouse)**, *Gastrodiscoides hominis* from, description of (M. Khalil), *Trop.* 9-14
- Defence** mechanisms, application to natural resistance to anthrax, animal experiments, *Path.* 6, 7
 normal study of, natural resistance and (J. C. G. Ledingham), *Path.* 1-8
- Deficiency** disease, pyorrhoea as, *Odont.* 4
- Deformity**, congenital, of upper limbs and feet, (R. C. Elmslie), *Orth.* 13
- Dementia præcox**, mental symptoms, *Psych.* 29
 spastic paralysis and complete amaurosis in Hebrew boy aged 10 (G. Riddoch), *Neur.* 29
- Demodex** impetigo, photographs of (A. Whitfield), *Derm.* 28
- Denker's** operation for empyema of maxillary antrum, after-treatment of (D. L. Sewell) (abstract), *Laryng.* 85
- Dental** abscesses, apical, alternative description as peri-apical bone, necrosis suggested, *Odont.* 11
 most serious lesion in dental sepsis, *Odont.* 11
 buds, nasal infection and, *Odont.* 37
- Dental** cyst, suppurating, drained, subsequently obliterated by blood-clot method (Dan McKenzie), *Laryng.* 2
 cysts, formation of, *Odont.* 39
 in maxilla, treatment, *Odont.* 41, 42
 treatment of, discussion on, *Laryng.* 3
 pulp, anatomical peculiarities of, *Odont.* 63
 cells of, *Odont.* 59
 circulatory system of, *Odont.* 62
 diseases of, exciting causes, *Odont.* 68
 predisposing causes, *Odont.* 67
 reactions due to, *Odont.* 68
 fibrilloblasts, *Odont.* 59
 dentinal fibrils formed by, *Odont.* 60
 fibroid degeneration of, in adult life, *Odont.* 69
 formative and nutritional functions of, *Odont.* 63
 "hastate" cells of, *Odont.* 62
 histology of (A. Hopewell-Smith), *Odont.* 58-62
 infection of, relatively rare, *Odont.* 70
 nervous system of, *Odont.* 63
 nodules of, extremely common, *Odont.* 70
 pathology of (A. Hopewell-Smith), *Odont.* 65-71
 physiology of (A. Hopewell-Smith), *Odont.* 63
 reactions due to non-operative injuries, *Odont.* 65
 references, *Odont.* 71
- sepsis**, arthritis due to, diagnosed and treated as tuberculous, case (R. C. Elmslie), *Orth.* 28
 as aetiological factor in diseases of other organs, discussion on, *Odont.* 7-34
 bacteriological considerations, *Odont.* 7
 cardiovascular complications of, *Odont.* 13
 cause of septic anaemia, *Odont.* 20
 diathesis predisposing to, *Odont.* 9
 diabetes and, *Odont.* 16
 disease of alveolar process in, radiographic evidence, *Odont.* 10, 11
 effects on blood-pressure, *Odont.* 13
 gastro-intestinal complications of, *Odont.* 13
 general diseases caused by, *Odont.* 12, 17
 general factors influencing effects of, *Odont.* 8
 amount of toxic absorption, *Odont.* 9
 family predisposition, *Odont.* 9
 local injury, *Odont.* 9
 potential health, *Odont.* 10
 resistance of patient, *Odont.* 9
 symbiosis, *Odont.* 10
 virulence of organism, *Odont.* 8
 gout and, *Odont.* 16
 hyperthyroidism and, *Odont.* 16
 ill-effect of, widespread, *Odont.* 25
 importance of blood examination in, *Odont.* 26
 in childhood, results of, *Odont.* 24
 indicated by clinical macroscopical signs of unhealthy conditions of teeth and gums, *Odont.* 10
 in mothers nursing infants, later effects on children, *Odont.* 32
 investigation of, need for co-operation between physician and dentist in, *Odont.* 32
 liver complications of, *Odont.* 14
 local infective conditions resulting from, *Odont.* 12
 mental disease and, *Odont.* 20, 21

- Dental sepsis, nephritis and, *Odont.* 20**
 nervous diseases and, *Odont.* 15
 ocular complications of, *Odont.* 14-26
 prophylaxis, importance of, *Odont.* 17
 renal complications of, *Odont.* 14
 respiratory complications of, *Odont.* 13
 rheumatic conditions and, *Odont.* 15, 17
 scurvy and, *Odont.* 18
 secondary infections in, *Odont.* 11
 secondary to some other disease or toxæmia, *Odont.* 12
 skin complications of, *Odont.* 14
 staphylococcal infection in, *Odont.* 8, 26
 streptococci found in, varieties of, *Odont.* 8, 17, 25
 treatment of, *Odont.* 17
 by extraction of teeth, *Odont.* 7, 17, 23, 26-32
 surgery, general anaesthesia in (W. J. McCardie), *Anæsth.* 11-20
- Dentigerous cyst, case of (M. Vlasto), *Laryng.* 43**
 multiple, cases of (B. Grellier, J. Howard Mummery), *Odont.* 43, 44-47
 symmetrical formation of, *Odont.* 47
- Dentine, adventitious, formation of, in dental caries, *Odont.* 65**
 of pathological origin, *Odont.* 66
 not innervated, *Odont.* 60, 61
 secondary, of pathological origin, *Odont.* 66
 structure of, related to diet during period of formation, *Odont.* 78
 type of, in 302 human deciduous teeth, *Odont.* 78
- Dentistry, preventive, some considerations for, (W. R. Ackland), *Odont.* 1-5**
 pre-natal and post-natal considerations, *Odont.* 4, 5
- Dentures, ill-fitting, causing dental sepsis, *Odont.* 28**
- Dercum's disease (adiposis dolorosa), case (E. Stolkind), *Clin.* 45**
- Dermatitis, arsenical, due to excessive doses of arsenical compound, *Neur.* 74-77**
 artefacts, case, (H. MacCormac), *Derm.* 106
 atrophic, of hands and feet, ? lupus erythematosus, case (H. W. Barber), *Derm.* 99
 repens and infectious eczematoid dermatitis, with involvement of mucous membrane, case (H. W. Barber), *Derm.* 98
 treatment by salicylic ointment, *Derm.* 109
- Dermoid tumour, ovarian, sarcoma in, case (H. R. Spencer), *Obst.* 101-105**
 microscopic structure, *Obst.* 104
- Dewar, T. F., C.B.—Incidence of venereal disease in Scotland (abstract), *Epid.* 81-84**
- Dextrocardia without transposition of other viscera, case (H. T. Gray), *Child.* 44**
- Diabetes, asymmetrical neuritis in, *Neur.* 15**
 dental sepsis in relation to, *Odont.* 16
 serious and fatal cases in young subjects, retinitis rare in, *Med. & Ophth.* 31
- Diaceturia and acetonuria, persistent, in case of enlarged liver (C. Worster-Drought), *Child.* 56**
- Diagnosis, cases for: (H. G. Adamson), *Derm.* 56; (H. W. Barber), *Derm.* 90; (? polycythæmia rubra) (P. Bardsley), *Ophth.* 19; (S. E. Dore), *Derm.* 57, 96;**
- Diagnosis, cases for: (C. M. H. Howell), *Neur.* 51; (R. C. Jewesbury), *Child.* 54; laryngeal (H. B. Jones), *Laryng.* 51; ? rena dwarfism (G. M. Kendall), *Child.* 20; ? diphtheria of skin (E. G. Graham Little), *Derm.* 86; (H. MacCormac), *Derm.* 62, 84; (B. Myers), *Child.* 19; (W. J. Oliver), *Derm.* 47; (H. C. Semon), *Derm.* 15; pigmented lesion (H. C. Semon), *Derm.* 59; ? leukæmia cutis (W. K. Sibley), *Derm.* 12; (? leukæmia cutis) discussion on, *Derm.* 14; (H. Smurthwaite), *Laryng.* 31; (H. B. Tawse), *Laryng.* 21; ulceration of left tonsil (H. Bell Tawse), *Laryng.* 70-72; spinal compression or disseminated sclerosis (F. M. R. Walshe), *Neur.* 49; (A. Whitfield) *Derm.* 75; (A. W. Williams), *Derm.* 71; possibly amyotonia congenita (S. A. K. Wilson), *Neur.* 49; ? papulonecrotic tuberculides (E. G. Graham Little), *Derm.* 103 (J. E. M. Wigley), *Derm.* 108**
- Diascope for opaque meal examinations of stomach, *Electr.* 39**
- Diathermy, in treatment of epithelioma of right half of fauces (Sir J. Dundas-Grant), *Laryng.* 8**
 in treatment of lymphangioma circumscriptum of tongue, *Derm.* 58, 59
- Diathesis in dental sepsis, *Odont.* 9**
- Dickson, W. E. Carnegie.—Discussion in ulcerative colitis, *Proct.* 100**
- Dientameba fragilis*, Jepps and Dobell, 1917, infection with, specimens from human case of (A. Robertson), *Trop.* 48**
- Diet, absence of vitamins from, favouring infection of colon, *Proct.* 92**
 before and after eruption of teeth, effects of, in relation to caries, *Odont.* 77
 carbohydrates in, as causal factor of dental caries, *Odont.* 76
 effect of, on resistance of teeth to caries (May Mellanby), *Odont.* 74-82
 summary, *Odont.* 79-82
 factor in pellagra, *Child.* 63, 64
 in treatment of ulcerative colitis, *Proct.* 94
 of asthma, *Therap.* 5
 sufficient in amount but defective in quality, effects on teeth and jaws in puppies, experiments showing, *Odont.* 75, 76
- Digestion, movements of gastro-intestinal mucosa in, rôle of, *Electr.* 92**
- Digestive system, diseases of, mortality in 1880 and 1920 compared, *Med.* 47**
 mortality in 1901-1920, *Surg.* 8
- Diggle, F. Holt.—Case of branchial cyst, *Laryng.* 95**
 of ? clinically malignant disease of left pyriform sinus, *Laryng.* 95
 laryngeal papillomata, *Laryng.* 96
 papilloma of left vocal cord, *Laryng.* 96
 tuberculosis of larynx, *Laryng.* 96
 discussion on case of pharyngeal pouch, *Laryng.* 41
 diseases of thyroid gland in their relation to laryngology (abstract), *Laryng.* 81
 epithelioma of right vocal cord, *Laryng.* 95

- Digitalis** and quinidine in treatment of auricular fibrillation, relative merits of, *Therap.* 35
- Diphtheria**, measles, and scarlet fever, triple infection, necrosis of left temporal bone involving facial nerve and labyrinth in case of, *Otol.* 29
- mortality in 1880 and 1920 compared, *Med.* 47
- of skin, case of, for diagnosis (E. G. Graham Little), *Derm.* 86
- pharyngeal, cranial nerve paralysis following, *Neur.* 21
- special incidence in young girls, *Epid.* 62
- Diplegia**, facial, accompanying acute febrile poliomyelitis, *Neur.* 17
- differential diagnosis from that due to poliomyelitis, *Child.* 78
- spastic, due to birth injury, *Baln.* 77
- Disease**, epidemicity of, *Epid.* 45
- living cause of (contagium vivum), antiquity of belief in, *Epid.* 46
- Dislocated** hips, see under *Hip-joint*, dislocation of
- Dittmar**, F.—Outbreaks of enteric fever due to carriers of infection (abstract), *Epid.* 1-10
- Diverticulum**, vesical (specimen) (J. Everidge), *Urol.* 43
- neck of, relationship of ureter to, *Urol.* 67
- Diverticula**, vesical, operative treatment of (J. Swift Joly), *Urol.* 55-69
- Dixon**, W. E., F.R.S.—Discussion on present position of organotherapy, *Therap.* 3
- discussion on value of ergot, *Obst. & Therap.* 5
- Doctor** and patient, relationship between, in practice of psychotherapy, use and abuse of (Mary C. Bell), *Psych.* 12-19
- Dog**, acarus from, case of mange in human being infected by (A. Whitfield), *Derm.* 75
- Dogs**, action of medicine upon, *Therap.* 44
- effects of cocaine upon, *Therap.* 46, 47
- of morphine upon, *Therap.* 45
- of strychnine upon, *Therap.* 47
- Donald**, A.—Clinical aspect of adenomyomata of female pelvic organs, *Obst.* 82-90
- Donaldson**, M.—Discussion on radium treatment before operation for cancer of cervix, *Obst.* 34
- Dore**, S. E.—Case for diagnosis, *Derm.* 57, 96
- of atrophic lichen planus in woman, aged 40, *Derm.* 20
- of breast tumour with atrophy of skin, pathological report on, *Derm.* 57, 96
- of lichen planus and syphilis, *Derm.* 18
- of parakeratosis variegata in man, aged 60, *Derm.* 19
- discussion on case for diagnosis, *Derm.* 90
- of acne scrofulosorum, *Derm.* 102
- of psoriasis, *Derm.* 72
- extensive linear naevus in man, aged 54, *Derm.* 105
- parakeratosis variegata in man, aged 40, case, *Derm.* 104
- two cases of neurofibromatosis, *Derm.* 104
- Douche**, Plombières, use of (A. G. Gibson), *Baln.* 22, 23
- diagnostic value, *Baln.* 22
- in infections from dental sepsis, *Odont.* 17
- Doyne**, P. G.—Case of additional phalanx in right thumb, *Child.* 46
- of hernial protrusions of orbital fat, *Child.* 46
- Tournay reaction (abstract), *Ophthalm.* 47
- Drake**, J. A.—Case of urticaria pigmentosa, *Derm.* 73
- Drew**, D.—Discussion on ulcerative colitis, *Proct.* 109
- Drowsiness** in uræmia, significance and causation of, *Urol.* 24
- Drug** tolerance of animals as compared with man, idiosyncrasies to (W. H. Kirk), *Therap.* 43-47
- Drugs** affecting gastric motility, *Therap.* 7
- influence of, on men of genius, *Psych.* 36
- in treatment of ulcerative colitis, *Proct.* 95
- modification of gastric function by (abstract) (T. I. Bennett), *Therap.* 6, 7
- response of frogs to, effect of light on (E. Boock and J. W. Trevan), *Therap.* 8
- Drury**, A. N.—Discussion on action of quinidine in cases of cardiac disease, *Therap.* 30
- Ductless** glands, dystrophia myotonia in relation to, *Neur.* 42
- importance of, in speeding up nervous system, *Psych.* 36
- influence on calcification of teeth, *Odont.* 3, 4
- Ductus arteriosus**, patent, and mitral disease, case of (G. M. Kendall), *Child.* 48
- Dudfield**, R.—Discussion on incidence of venereal disease in Scotland, *Epid.* 84
- discussion on surface diseases of eyes, *Epid.* 62
- on the ultraviolet viruses, *Epid.* 78
- Dudgeon**, L. S., C.M.G.—Discussion on ulcerative colitis, *Proct.* 104
- report on case of breast tumour with atrophy of skin, *Derm.* 96
- Dukes**, C., O.B.E.—New fermentation tube, in which carbohydrate may be separated from proteins during sterilization, *Path.* 13-16
- Duncan**, A. G.—Case of syringomyelia showing pain of central origin, *Neur.* 83
- Dundas-Grant**, Sir J.—Case illustrating valvular action of ventricular bands, *Laryng.* 43
- case of acute suppuration in one ear subjected to early operation on account of complete deafness in opposite ear, *Otol.* 6
- of chronic empyema of antrum; Canfield's operation, recovery, *Laryng.* 20
- of complete deafness dating from a fall, *Otol.* 47
- of complete nerve-deafness due to syphilis of internal ears; caloric and rotation tests negative, galvanic positive, *Otol.* 16
- of deafness greatly increased after a fall, *Otol.* 48
- of epithelioma of right half of fauces treated by diathermy, *Laryng.* 8
- of epithelioma of vestibule of nose after treatment by radium, *Laryng.* 65
- of hoarseness due to singer's nodes, *Laryng.* 44
- of infiltration of ventricular band (intraventricular tuberculosis), *Laryng.* 43
- of long-standing deafness attributable to falls on the head, improvement, *Otol.* 49
- of outgrowth from ventricle in subject of pulmonary tuberculosis, *Laryng.* 55
- of swelling of right ventricular band, *Laryng.* 43

- Dundas-Grant, Sir J.**—Case of tuberculosis of larynx, with demonstration of instrument for sunlight treatment, *Laryng.* 12
 of vertigo (simulating Ménière's disease) with anomalous nystagmus reactions, *Otol.* 20
 of vertigo, with fixation of ossicles, cured by ossiculectomy, *Otol.* 18
 discussion on case of brain abscess due to otitic infection, *Otol.* 55
 on case of otitis media with facial paralysis, following scarlet fever, *Otol.* 18
 of otosclerosis with unusual symptoms, *Otol.* 10
 of tinnitus, associated with facial spasm, *Otol.* 8
 of vertigo cured by opening external semi-circular canal, *Otol.* 60
 on eighth nerve tumours, *Otol.* 42
 on infection of teeth and gums in their relationship to nose, throat and ear, *Odont.* 39
 on otosclerosis and osteitis deformans, *Otol.* 27
 on treatment in cases of tuberculosis of larynx, *Laryng.* 65
 outgrowth from anterior half of left vocal cord (?) fibroma or prolapse, *Laryng.* 69
 two cases of paralysis of left vocal cord and left half of palate, *Laryng.* 68
 and **McKenzie, Dan.**—Case of sarcoma of tonsillar region treated by X-Rays after partial removal, *Laryng.* 69
 and **Perkins, J. J.**—Case of papillomata of trachea, *Laryng.* 7
 and **Worster-Drought, C. C.**—Nasal stenosis, mainly subjective, in case of Parkinson's disease, *Laryng.* 23
Dunn, Naughton.—Discussion on operative treatment of spastic paralysis, *Orth.* 41
Duodenal and gastric ulcers, mortality greater now than 40 years ago, *Med.* 44, 45
 bulb, pressure deformity of, due to pathological gall-bladder, *Electr.* 83
 ulcer, cases of, to illustrate certain points in diagnosis (H. S. Souttar), *Clin.* 5, 6
 associated with ptosis: high acidity: relief with gastro-enterostomy, case (H. S. Souttar), *Clin.* 6
 involving pylorus, change in epoch of pain, relief by gastro-enterostomy case (H. S. Souttar), *Clin.* 6
 mortality statistics, 1880-1919, Guy's Hospital, *Med.* 45
 with visceroptosis and low acidity, case (H. S. Souttar), *Clin.* 5, 6
Duodeno-jejunal junction, congenital obstruction of bowel at, case (R. C. Jewesbury), *Child.* 11, 12
Duodenum, atresia of, *Child.* 13
 first portion of, constant pressure of pathological gall-bladder on, X-ray appearances, *Electr.* 86, 87, 90
 pressure deformity of pathological gall-bladder on, *Electr.* 78
 obstruction of, in infants, two cases (R. C. Jewesbury), *Child.* 10-12
 treatment by operation (R. C. Jewesbury and M. Page), *Child.* 50
 X-ray appearances, *Child.* 51-53
Duodenum, second portion of, fixation of, due to pathological gall-bladder, X-ray appearance, *Electr.* 86, 87
 involvement of, in pathological gall-bladder, *Electr.* 78, 79
 pressure from pathological gall-bladder on, X-ray appearances, *Electr.* 85
 stenosis of, congenital, case (R. C. Jewesbury), *Child.* 10, 11
 in infants, rarity, *Child.* 13
Dyke, S. C. B. and Bridge, C. H.—The inheritance of the specific isoagglutinable substances of human red cells, *Path.* 35-46
Dysenteric stools, typical, exceptional in ulcerative colitis, *Proct.* 105
Dysentery, amœbic and bacillary, differences as seen by sigmoidoscope, *Proct.* 107
 casualties from, in Henry V's campaign of the Somme in 1415, *Hist.* 3, 4
 institutional, *Proct.* 91, 97
 multiple perforations of colon resulting from, specimen showing (P. Cole), *Proct.* 67
 periodontitis complicating, *Odont.* 4
 spirochaetal, case of (W. Broughton-Alcock), *Trop.* 46
 true, and ulcerative colitis not necessarily caused by same specific organism, *Proct.* 105
Dysmenorrhœa, analysis of 100 cases (L. Phillips), *Obst.* 110-115
 cases of, classification, *Obst.* 110
 chief symptom of adenomyomata of female pelvic organs, *Obst.* 83
 clinical types, due to arrested development of genital organs, *Obst.* 114
 due to faulty hygiene, *Obst.* 112
 due to functional disturbance, *Obst.* 113
 due to obstruction, *Obst.* 113
 treatment, appropriate, *Obst.* 112-115
 general, *Obst.* 111
 by electricity, *Obst.* 114
 by organotherapy, *Obst.* 111, 114
 and menorrhagia, frequent association of pelvic hypoplasia with, *Obst.* 111
Dyson, W.—Case of lymphoblastic erythrodermia, *Derm.* 21
 discussion on, leishmaniasis of skin, *Derm.* 10
Dyspepsia, gastric and intestinal, due to dental sepsis, *Odont.* 13
Dyspnœa, paroxysmal, in uræmia, significance and causation of, *Urol.* 23
Dystrophia myotonica, case of (W. J. Adie), *Neur.* 45
 (myotonia atrophica), heredito-familial disease with cataract (W. J. Adie), *Neur.* 36-43
 distinction from myopathies, *Neur.* 42
 from Thomsen's disease, *Neur.* 41
 history of, *Neur.* 36, 37
 in relation to ductless glands, *Neur.* 42
 symptoms of, *Neur.* 38, 39, 41
Ear, internal, syphilis of, complete nerve-deafness due to, case (Sir J. Dundas-Grant), *Otol.* 16
 middle, acute suppuration of, epidemic cerebro-spinal meningitis associated with, case (F. Sydenham and Dan McKenzie), *Otol.* 51

- Ear**, suppurative of, labyrinthitis as complication of (abstract), (A. Logan Turner and J. S. Fraser), *Otol.* 15
treatment, operative, *Otol.* 6
nose and throat, infections of teeth and gums in their relationship to, discussion on, *Odont.* 35-42
right, acute suppuration in, subjected to early operation on account of complete deafness in left ear (Sir J. Dundas-Grant), *Otol.* 6
- Earache**, deep-seated, dental origin of, *Odont.* 35, 40
- Eclampsia**, ætiological factors, *Obst.* 2
causes of death in fatal cases, *Obst.* 9, 10
changes in retina in, due to blood-conditions, *Med. & Ophth.* 5
classification of cases of, *Obst.* 4, 5
fœtal and neo-natal mortalities, *Obst.* 10
mortality in, effects of methods of delivery upon, *Obst.* 11
relation to number of fits, *Obst.* 10
relation to time of onset of fits, *Obst.* 10
influence of parity on, *Obst.* 2, 5
maternal mortality in, in relation to incidence of fits before, during or after labour, *Obst.* 5
in relation to number of fits previous to treatment, *Obst.* 5
to parity, *Obst.* 2, 5
to persistence of fits after delivery, *Obst.* 6
to period of gestation at which eclamptic convulsions supervened, *Obst.* 5
to sudden onset of fits without preceding symptoms, *Obst.* 5
statistics, *Obst.* 5
prognosis and treatment of, Report of Committee of Section of Obstetrics and Gynæcology, on, *Obst.* 1-11
prognosis of, effect upon, of albuminuria, *Obst.* 4
effect upon, of coma, *Obst.* 3
of high blood-pressure, *Obst.* 4
of œdema, *Obst.* 4
in relation to grouping of cases, *Obst.* 3
number of fits, *Obst.* 4
pulse-rate, *Obst.* 3
temperature, *Obst.* 3
proportion of twin pregnancies complicated by, *Obst.* 2
seasonal incidence of, *Obst.* 2
treatment of, by morphia and other sedatives, *Obst.* 9
by venesection, results, *Obst.* 9
medical, *Obst.* 8
obstetric, *Obst.* 7, 3
results, *Obst.* 6
yearly incidence of, *Obst.* 2
- Economics** and tuberculosis (R. J. Ewart), *Epid.* 11-16
in relation to free meals provided to school children, 1912-1922, *Epid.* 15
"inherited diathesis," *Epid.* 15, 16
wage movements and cost of living, table showing, *Epid.* 14, 15
- Ectopia lentis** (both eyes), case (M. L. Hine), *Ophth.* 12
methods of operating for, *Ophth.* 12, 13
testis, case of (A. C. Morson), *Urol.* 43-45
- Eczema** and asthma, alternation in attacks of, *Therap.* 4
- Eden**, T. W.—Discussion on case of carcinoma of prolapsed cervix in woman aged 77, *Obst.* 110
on myomectomy for uterine fibroids, *Obst.* 22
on treatment of dysmenorrhœa, *Obst.* 115
on value of ergot, *Obst. & Therap.* 6
Report of Committee of Section of Obstetrics and Gynæcology in Prognosis and Treatment of Eclampsia, *Obst.* 1-11
and Goodwin, A.—Two cases of cancer of cervix treated by radium before operation, *Obst.* 32
- Edwards**, F. Swinford.—Discussion on gonorrhœal stricture of rectum, *Proct.* 19
- Egypt**, accommodative asthenopia in, *War* 49
surface diseases of eyes in, seasonal influence in relation to, *Epid.* 60
- Eighth** nerve tumours, surgical treatment of (abstract) (W. Trotter), *Otol.* 37, 38
See also *Acusticus* tumours
- Einhoven's** string galvanometer, introduction of, *Electr.* 3, 4
- Elbow-joint**, syphilitic osteomyelitis involving, case (C. M. Page), *Clin.* 32
- Electric** responses of phrenic nerve during respiration, *Neur.* 57
- Electrical** currents, interrupted, rhythmical stimulation of abdominal muscles by, *Baln.* 14, 15
stimuli, changes in nervous conduction revealed by, *Neur.* 55
- Electricity** in treatment of dysmenorrhœa, *Obst.* 114
- Electrocardiograph** in diagnosis, history of, *Electr.* 4
- Elliot**, Hugh.—Definition of genius by, *Psych.* 35
- Ellis**, A.—Discussion on significance of vascular and other changes in retina in arteriosclerosis and renal disease, *Med. & Ophth.* 17
- Elmalie**, R. C., O.B.E.—Case of arthritis due to dental sepsis diagnosed and treated as tuberculous, *Orth.* 28
of hæmophilic arthritis of knee, *Orth.* 27
of intracapsular fracture of neck of femur, *Orth.* 49
congenital deformity of upper limbs and feet, *Orth.* 13
discussion on operative treatment of dislocated hips, *Orth.* 24
on operative treatment of spastic paralysis, *Orth.* 40
- Embolism** (air) occurring during urethroscopy, case (R. Ogier Ward), *Urol.* 54
consequent on restoration of auricular function, after quinidine administration, *Therap.* 33
- Emetics**, action of, insusceptibility of horse to, *Therap.* 43
- Emotion**, relationship of internal secretions to, *Psych.* 25
- Empyema** of maxillary antrum, chronic case of, Canfield's operation, recovery (Sir J. Dundas Grant), *Laryng.* 20
(Denker's operation for) after-treatment of (abstract) (D. L. Sewell), *Laryng.* 85
- Enamel** organ of *Felis domestica*, vascular supply of (E. Sprawson), *Odont.* 47-54

- Enamel organ**, cells of, *Odont.* 49, 50
microscopical appearance of, *Odont.* 48-53
tooth-follicle in relation to, *Odont.* 51
- Encephalitis** lethargica, case with unusual sequelæ (Parkinsonian syndrome associated with right hemiplegia, showing peculiar disturbances of tone and posture in limbs on hemiplegic side) (D. McAlpine), *Neur.* 27
discussion on, *Child.* 33-40
followed by symptoms of paralysis agitans, case (O. C. M. Davis), *Child.* 81
followed by tremor, localization of lesions in brain, *Ophth.* 42
late results of, cases showing (E. A. Cockayne), *Child.* 32; (H. C. Cameron), *Child.* 30; (R. C. Jewesbury), *Child.* 29; (F. J. Poynton), *Child.* 31; (C. P. Symonds), *Child.* 32; (C. Worster-Drought), *Child.* 32, 33
benefit from residential school treatment, *Child.* 37
manifestations in school children, *Child.* 35, 38
Parkinsonian syndrome and, *Child.* 35, 38, 39
seat of lesion in brain, *Child.* 39
- Endocarditis**, ulcerative, due to dental sepsis, *Odont.* 13
- Endocrine** "balance," *Psych.* 22
in relation to psycho-physical reaction, *Psych.* 31
factor in mental disease (J. L. Wilson), *Psych.* 21-30
bibliography and references, *Psych.* 30
function, and autonomic nervous system, relationship between, *Baln.* 16
pigmentation of skin in relation to, *Baln.* 13
glands concerned in self-preservation, *Psych.* 25
directing sexual development, *Psych.* 24
foetal development of, *Psych.* 22
governing nutrition, *Psych.* 23
system and sympathetic nerve, balance between, in relation to asthma, *Therap.* 3
as basis of feeling-tone or psychæsthesia, *Psych.* 26
as intermediate zone between mind and body, *Psych.* 31
biological relationship of, *Psych.* 21
in relation to human conduct, *Psych.* 23
to instincts and feelings, *Psych.* 27
- Endolysins**, constitution of, uncertain, *Path.* 5
- Endometrium**, microscopical appearances of, before and after exposure to radium, *Obst.* 74-76
- Endothelioma** of ethmoid, lateral rhinotomy, case (A. A. Smalley), *Laryng.* 94
of orbit (F. A. Williamson-Noble), *Ophth.* 35
- England**, pellagra in, *Child.* 62
and Wales, Black Death in, as exhibited in Manorial Documents (W. Rees) *Hist.* 27-45
coal and metalliferous miners in, mortality of (E. L. Collis), *Epid.* 85-99
entry of Great Pestilence into, in 1348, *Hist.* 28
- Enteric fever**, see *Typhoid fever*
- Enteritis**, dental sepsis in relation to, *Odont.* 14
mortality from, 1907-1920, statistics, *Surg.* 8
septic, underlying septic anæmia, *Med.* 6
- Environment** in relation to causation of caries of teeth and pyorrhœa, *Odont.* 3
- Enzyme** action, nature of, Sir W. M. Bayliss quoted, *Epid.* 70, 71, 72
- Enzymes**, modern views on (quoted), *Epid.* 69
- Eosinophilia** in asthma and laryngismus stidulus, *Therap.* 5
- Epidemic disease**, persistency of type of, *Epid.* 74
- Epidemicity** of disease, *Epid.* 45
- Epidemics** of Fourteenth Century (W. Rees), *Hist.* 27-45
- Epidemiological Society**, of London, history and aims of, *Epid.* 35
- Epidemiology** of surface diseases of the eyes (N. B. Harman), *Epid.* 49-62
progress and problems in (R. J. Reece), *Epid.* 35-48
in relation to atmospheric conditions, *Epid.* 47
to "carriers" of disease, *Epid.* 42
to diseases communicated to man by animals, *Epid.* 39
to diseases communicated to man by insects, *Epid.* 38, 39
to seasonal incidence of disease, *Epid.* 43
knowledge of the ancients as to, *Epid.* 44, 45
ultravisible viruses considered from point of view of, (Sir W. Hamer), *Epid.* 65-76
- Epidermolysis** bullosa, cases (W. F. R. Castle), *Derm.* 53 and (H. Davis) *Derm.* 2
- Epidermophyton rubrum**, infection by, in cases of ringworm of nails, *Derm.* 1
- Epididymis**, myosarcoma of, case (Sir J. W. Thomson-Walker), *Urol.* 31
- Epididymitis**, gonococcal, acute, treated with manganese butyrate, case, *Derm.* 69
- Epigastric** region, removal of bone penholder from, case, *Obst.* 37
- Epilepsy** and asthma, points of resemblance between, *Therap.* 2
differential diagnosis from hysterical fits difficult, *Neur.* 93
- Epileptics**, subject to paroxysms of neuroses, *Neur.* 93
- Epileptiform** seizures subsequent to operation for temporo-sphenoidal abscess, case (D. McKenzie), *Otol.* 52
- Epiphysis** of left acromion process, injury to (P. B. Roth), *Orth.* 14
- Epispadias**, glandular, two cases of (J. Swift Joly), *Urol.* 39-41
- Epithelioma** adenoides cysticum (L. Savatard), *Derm.* 30-46
Balzer's and Menetrier's case, *Derm.* 31, 32
bibliography, *Derm.* 46
Brooke's original description of cases of, *Derm.* 31, 32
cases of solitary lesions, *Derm.* 36, 37
reported, 1894-1918, *Derm.* 31
clinical aspects, summary of, *Derm.* 39
cyst formation, *Derm.* 41, 42
description of cases, *Derm.* 34 et seq.
differential diagnosis, *Derm.* 43, 44, 45
Fordyce's description of cases, *Derm.* 30, 32
hereditary factor in, *Derm.* 35
histology of, Brooke's views, *Derm.* 39, 40
microscopical appearances of tumours, solitary lesion, *Derm.* 38, 40-44
Perry's case, *Derm.* 31, 32
treatment of, *Derm.* 45
- Epithelioma** of nasal septum, floor of both nostrils, alveolar surface upper jaw, and left side lower jaw, case (A. Wylie), *Laryng.* 30

- Epithelioma** of penis (?), case (A. E. M. Woolf), *Clin.* 1
 of right half of fauces treated by diathermy (Sir J. Dundas-Grant), *Laryng.* 8
 of right vocal cord, case (F. Holt Diggle), *Laryng.* 95
 laryngo-fissure for, case (Sir W. Milligan), *Laryng.* 89
 of soft palate and left anterior faucial pillar, case (Sir W. Milligan), *Laryng.* 88
 of tubal rugæ, stretching of, by blood effused into them in torsion of pedicle of ovarian tumour (H. R. Spencer), *Obst.* 106-109
 of vestibule of nose after treatment by radium, case (Sir J. Dundas-Grant), *Laryng.* 65
 (possibly syphilis), laryngeal, case of, completely healed and arrested under X-ray treatment without operation (Sir StClair Thomson), *Laryng.* 60
 squamous, of vagina, specimen of (T. G. Stevens), *Obst.* 26
Erb's paralysis, case of (G. Perkins), *Child.* 74
Ergot, activities, bases to which due, *Obst. & Therap.* 3
 alkaloids of, *Obst. & Therap.* 3
 and pituitary extract, action compared, *Obst. & Therap.* 6
 extracts of, therapeutic value measurement, *Obst. & Therap.* 2, 3
 fluid extract of (U. S. Pharmacopœia), *Obst. & Therap.* 2
 preparations of, in British Pharmacopœia, *Obst. & Therap.* 1
 use of, in obstetrical practice replaced by pituitary extract, *Obst. & Therap.* 4
 value of, in obstetrical and gynecological practice, with special reference to its present position in British Pharmacopœia (H. H. Dale and discussion), *Obst. & Therap.* 1-7
"Ergotamine" therapeutic properties of. *Obst. & Therap.* 3
"Ergotinine," amorphous, *Obst. & Therap.* 2
 citrate, *Obst. & Therap.* 2, 4
"Ergotoxine," *Obst. & Therap.* 2
"Ernutin," *Obst. & Therap.* 4
Erysipelas, recurring, case of (J. E. R. McDonagh), *Derm.* 85
Erythema, chronic, of legs, two cases (H. MacCormac), *Derm.* 11
 desquamative, associated with arthritic changes, case (Sir J. Galloway and M. G. Hannay), *Derm.* 16-18
 multiforme, cases of, satisfactorily treated by extraction of teeth, *Odont.* 28
 of face, case (A. H. M. Gray), *Derm.* 79
Erythraemia (polycythæmia vera, Vaquez-Osler's disease) with cerebral hæmorrhage, case of (J. A. Ryle), *Med.* 83
 treated by Röntgen-therapy, two cases of (E. Stolkind), *Clin.* 35-38
Erythrodermia, lymphoblastic, case of (W. Dyson), *Derm.* 21
Erythrodermie pityriasique en plaques disséminées, (Brocq) (H. C. Semon), *Derm.* 103
Ether, administration during gas anaesthesia in dental surgery, *Anæsth.* 15, 16
 instead of gas in dental surgery, *Anæsth.* 16
Ether, as anæsthetic in Cæsarean section, danger of asphyxia of infant, *Anæsth.* 1, 2
 in dental surgery, *Anæsth.* 21
Ethmoid, endothelioma of; lateral rhinotomy, case, (A. A. Smalley), *Laryng.* 94
Ethyl chloride anæsthesia in dental surgery, *Anæsth.* 16, 20
Eustachian tube, region of, sequestra removed from, during radical mastoid operation (T. H. Just), *Otol.* 61
Evans, E. Laming, C.B.E. — Discussion on operative treatment of spastic paralysis, *Orth.* 38
 tendon transplantation for talipes, *Orth.* 14
Evans, G. — Case of thrombo-angeitis obliterans, *Clin.* 12-14
 summary of conception of nephritis (quoted), *Urol.* 24
Everest expedition, experiences with (abstract), (T. G. Longstaff), *Med.* 57
Everidge, J., O.B.E. — Specimen of diverticulum of bladder, *Urol.* 43
 specimen showing interior of bladder six months after extensive resection for carcinoma, with transplantation of right ureter, *Urol.* 43
Εὐσχημοσύνη, definition of, *Hist.* 13
Ewart, R. J. — Economics and tuberculosis, *Epid.* 11-16
Exanthemata, association of onset of phlyctenular keratitis with, *Epid.* 56
Exercise and exercises in treatment of dysmenorrhœa, *Obst.* 112
 effects of, on heart, *Anæsth.* 29
Exophthalmos probably caused by non-suppurative cavernous sinus thrombosis, case (F. Parkes Weber), *Clin.* 41
Exostoses, multiple, case (A. Davis), *Orth.* 26
 and hip disease, case (C. E. Shattock), *Clin.* 2
Extra-cerebellar abscess and tuberculous lateral sinus thrombosis, temporal bone from, case of (E. D. D. Davis), *Otol.* 5
Eye, calcareous degeneration of, with deposits on iris, (R. Batten), *Ophth.* 1
 complications of dental sepsis, *Odont.* 14
 congenital crescents in, embryology of (abstract) (Ida C. Mann), *Ophth.* 45
 limitation of movement in one, in case of bilateral proptosis (R. A. Greeves), *Ophth.* 15
Eyelid, right upper, tumour of (angioma) (J. F. Cunningham), *Ophth.* 15
Eyelids, retraction of, disseminated sclerosis with, case (J. Collier), *Neur.* 47
 case of mid-brain lesion (J. Collier), *Neur.* 47
 of tumour affecting mid-brain (J. Collier), *Neur.* 46
Eyes, affection of, associated with dental sepsis, *Odont.* 27, 29
 contracted socket of, plastic operation for (N. W. B. Oliver), *Ophth.* 15
 surface diseases of, epidemiology of (N. B. Harman), *Epid.* 49-62
 age incidence, *Epid.* 54
 prevalence of, influence of other epidemics on, *Epid.* 60
 results of examination of school children, *Epid.* 53, 54
 seasonal influence, *Epid.* 59

- Eyes**, surfaces diseases of, sex influence, *Epid.* 57
social conditions and, *Epid.* 51, 52
tuberculous diseases of (A. L. Whitehead),
Ophth. 2-7
- Face**, carcinoma of, squamous, in woman, aged 24
(W. J. O'Donovan), *Derm.* 52
cellulitis of, chronic case (G. E. Archer), *Laryng.*
94
erythema of (A. H. M. Gray), *Derm.* 79
skin of, multiple early squamous-celled carci-
noma of, case (W. J. O'Donovan), *Derm.*
87
swelling of, case (D. Hare), *Clin.* 33
- Facial nerve** and labyrinth, necrosis of left tem-
poral bone involving, following triple
infection of scarlet fever, measles and
diphtheria, in child aged 7, *Otol.* 29
spasm, tinnitus associated with, case (G. J.
Jenkins), *Otol.* 8
- Fæces**, analyses of, in cases of coeliac infantilism
in non-diarrhoeic stage, *Child.* 22
fat analysis of, twenty-two months after hepatec-
tomy, *Surg.* 48
transmission of leishmania parasites by, dis-
cussed, *Trop.* 5-7
- Fairbairn, J. S.**—Cyst of uterine cornu, due to
dilatation of interstitial portion of tube,
Obst. 45
necrotic fibro-adenoma in patient, aged 74,
simulating cancer of corpus uteri, *Obst.*
45
- Fairbank, H. A. T., D.S.O.**—Case of (?) Charcot's
knee, *Orth.* 47
of dislocation of patella outwards, secondary
to osteomyelitis of femur, *Orth.* 47
discussion on operative treatment of dislocated
hips, congenital and pathological, *Orth.*
15-23
on operative treatment of spastic paralysis,
Orth. 38
ischæmic paralysis, case, *Orth.* 11
unusual form of syndactyly, *Orth.* 29
- Fallopian tube**, dilatation of interstitial portion
of, cyst of uterine cornu due to, *Obst.*
45
condition of, in case of ovarian cyst with twisted
pedicle, of, *Obst.* 106, 107
rugæ of, stretching of epithelium of, by blood
effused into rugæ in torsion of pedicle of
ovarian tumour (H. R. Spencer), *Obst.*
106-109
- Farrington, W. B., D.S.O., Flight-Lt.**—Dis-
cussion on medical aspects of life at high
altitudes, *Med.* 60
- Fatigue**, effect of, on mental efficiency in tropical
climates, *War* 52
- Fauces** and palate, ulceration of, cases (T. J.
Faulder), *Laryng.* 53 (W. H. Kelson and
W. H. Thornhill), *Laryng.* 13
posterior pillar of, myeloid sarcoma of, case
(N. Patterson), *Laryng.* 13
right half of, epithelioma of, treated by dia-
thermy, case (Sir J. Dundas-Grant),
Laryng. 8
- Faulder, T. J.**—Cyst of uvula, *Laryng.* 25
case of ulceration of palate and fauces, *Laryng.*
53
- Favus** of smooth skin, two cases of (E. G. G.
Little), *Derm.* 51
- Featherstone, H. W.**—Discussion on anæstheti-
zation in Cæsarean section, *Anæsth.* 3
and Whitehouse, B.—Two cases of Cæsarean
section under spinal anæsthesia with
tropacocaine, *Obst.* 55-58
- Feeling**, morbid states of, as cause of mental
symptoms, *Psych.* 28
in dementia præcox, *Psych.* 29
in mania, *Psych.* 28
in melancholia, *Psych.* 28
- Feelings** and instincts, relationship to endocrine
system, *Psych.* 27
- Fees**, question of, under Greek medical etiquette,
Hist. 15
- Feet and hands**, atrophic dermatitis of, (?) lupus
erythematosus, case (H. W. Barber),
Derm. 99
and upper limbs, congenital deformity of (R. C.
Elmslie), *Orth.* 13
tuberculous neuritis of, *Neur.* 18
- Felling A.**—Case of progressive double athetosis,
Neur. 79
of tremor for diagnosis, *Neur.* 27
discussion on operative treatment of spastic
paralysis, *Orth.* 40
on significance of vascular and other changes
in retina in arterio-sclerosis and renal
disease, *Med. & Ophth.* 29
on treatment of neuro-syphilis, *Neur.* 75
- Felis domestica**, enamel organ of, vascular supply
of (E. Sprawson), *Odont.* 47-54
- Femur**, neck of, fracture of, intracapsular, case
(R. C. Elmslie), *Orth.* 49
two cases in training-ship boys (W. T. G.
Pugh), *Orth.* 31
osteomyelitis of, outward dislocation of patella
secondary to, case (H. A. T. Fairbank),
Orth. 47
outer condyle of, myeloma of, showing result of
bone-grafting, case (A. H. Todd), *Clin.* 3, 4
use of weight-relieving calliper after opera-
tion, *Clin.* 3, 4
- Fermentation**, doctrine of evolution of, stages of,
Epid. 73
nature of, Lord Lister quoted, *Epid.* 67
Pasteur's studies on, described, *Occ. Lect.* 12, 13
tube, new, in which carbohydrates may be
separated from proteins during steriliza-
tion (C. Dukes), *Path.* 13-16
- "Fermergin"** (solution of ergotamine salts),
Obst. & Therap. 4
- Fibrillation** and flutter, auricular, circulating
wave in auricle present in, *Therap.* 31
- "Fibrilloblast,"** term suggested in place of
"odontoblast," *Odont.* 59
- Fibrilloblasts**, adult and young cells, differences
between, *Odont.* 62
in dental pulp forming dentinal fibril, *Odont.* 60
number of, *Odont.* 59, 60
- Fibro-adenoma**, necrotic, in patient aged 74,
simulating cancer of corpus uteri (J. S.
Fairbairn), *Obst.* 45
- Fibroid** degeneration of dental pulp in adult life,
extremely common, *Odont.* 69
of cervix, large, developing after subtotal
hysterectomy, case (A. E. Giles), *Obst.*
12

- Fibroids** complicated by pregnancy, treatment by myomectomy combined with Cesarean section, *Obst.* 22
uterine, treatment by myomectomy, indications for and results of, *Obst.* 13-21
- Fibroma** of nose, case (L. Powell), *Laryng.* 66
- Fibromyoma**, leiomyosarcoma of, removed by subtotal hysterectomy (E. Holland), *Obst.* 64
- Fibrositis**, dental sepsis common cause of, *Odont.* 15, 17
- Field hospitals**, first mention of use of, *Hist.* 4
- Filariasis**, early studies of Manson on, results of, *Epid.* 39
- Finger**, numb, in cases of chronic nephritis, importance of symptom, *Urol.* 82
- Fingers** and toes, clubbing of, case (O. C. M. Davis), *Child.* 81
erosion of, by muriatic acid (W. J. O'Donovan), *Derm.* 87
- Finklestone-Sayliss**, H.—Results of treatment by manganese butyrate of cases at London Lock Hospital, *Derm.* 68, 69
- Finucane**, M. I.—Discussion on Coroners' Inquests, *Anæsth.* 40
- Fisher**, J. Herbert—Discussion on significance of vascular and other changes in retina in arterio-sclerosis and renal disease, *Med. & Ophth.* 30
discussion on Tournay reaction, *Ophth.* 48
- Flatula**, urinary, suprapubic, closure of, following suprapubic prostatectomy, observations on 68 cases (H. P. W. White), *Surg.* 119-125
cases in relation to use of indwelling catheter, *Surg.* 120, 125
malignant cases, *Surg.* 124
onset of micturition, *Surg.* 123-125
operative procedures, *Surg.* 120
post-operative treatment, *Surg.* 119
references, *Surg.* 125
secondary hæmorrhage, *Surg.* 123
time of removal of suprapubic drain, *Surg.* 123
use of indwelling catheter, *Surg.* 121
- Fits**, hysterical, with some reference to their treatment (L. R. Yealland), *Neur.* 85-94
clinical observations on, *Neur.* 86, 87
differential diagnosis from epilepsy difficult, *Neur.* 93
types of seizures, *Neur.* 88
cases illustrating, *Neur.* 88-93
number of, effects on prognosis of eclampsia, *Obst.* 4
- Flack**, Martin, Wing-Commander—Discussion on medical aspects of life at high altitudes, *Med.* 58
- Flavine**, value of, for irrigation, in ulcerated colitis, *Proct.* 109
- Flemming**, A. L.—Cases of difficulties due to important points having been missed at preliminary examination, *Anæsth.* 9, 10
discussion on anesthetization for Cesarean section, *Anæsth.* 2
on systematic examination of heart, *Anæsth.* 31
- Fletcher**, Dr.—Table showing results of examination of prostates removed by operation, *Urol.* 75
- Flexion** contracture of forearm, treated by muscle-sliding operation, four cases of (C. Max Page), *Orth.* 43, 44
- Flukes** (lung), of genus *Paragonimus* (G. M. Vevers), *Trop.* 43, 44
- Fluoroscopes** in conjunction with orthodiagraphy, Gösta Forssell's device for, *Electr.* 8-11
- Flutter** and fibrillation, auricular, circulating wave in auricle present in, *Therap.* 31
- Flying** at high altitudes, immediate and remote effects of, *Med.* 59
- Foerster's** operation, disappointing results of, *Orth.* 34, 37, 39, 40
in spastic paralysis, principle of, *Orth.* 34
- Fœtal** and neo-natal mortalities in eclampsia, *Obst.* 10
- Folliculitis** decalvans, association of tuberculides with, unique case of, *Derm.* 102
case, (E. G. G. Little), *Derm.* 50
of scalp, peculiar case of (A. Castellani) *Derm.* 97
ulerythematosæ reticulata, case (E. G. Graham Little), *Derm.* 81
- Food-consumption**, real wages and death-rate from, phthisis, comparative table, *Epid.* 13
supply in relation to death-rate from phthisis, *Epid.* 15
- Forceps**, hæmostatic, left in peritoneal cavity, spontaneous partial extrusion through cervix, case, *Obst.* 36
- Ford**, Rosa.—Intracranial tumour causing quadrantic hemiopia, *Ophth.* 30
- Fordyce**, description of cases of epithelioma adenoides cysticum by, *Derm.* 30, 32
- Forearm**, flexion contracture of, treated by a muscle-sliding operation, four cases of (C. Max Page), *Orth.* 43, 44
- Foreign** bodies, large, impacted in gullet, treatment of (D. R. Paterson), *Laryng.* 77
(candle) removed from bladder of male (F. Kidd), *Urol.* 84
(paper fastener) in left bronchus of child (H. Tilley), *Laryng.* 20
(threepenny piece) impacted in perforation between œsophagus and trachea in baby (H. J. Banks-Davis), *Laryng.* 55
left in peritoneal cavity, see under *Instruments*
removed from trachea of child aged 6 months (H. Smurthwaite), *Laryng.* 66
two, one movable and the other fixed, in trachea of child aged 3, mounted specimen showing (H. J. Banks-Davis), *Laryng.* 55
- Forsdike**, S.—Discussion on radium treatment before operation for cancer of cervix, *Obst.* 34
treatment of severe and persistent uterine hæmorrhage by radium, with report upon 45 cases, *Obst.* 69-78
- Forssell**, Gösta.—Device for orthodiagraphy in conjunction with fluoroscopy, *Electr.* 8-11
movements of the gastro-intestinal mucosa (abstract), *Electr.* 91-94
- Fournier**.—Observations on neurosyphilis (quoted), *Neur.* 67
- Fox**, F.—Medical hydrology (quoted), *Bain.* 15
- Fox**, Wilfrid.—Case of syphilis in a man, *Derm.* 16
discussion on mange in human being infected by dog, *Derm.* 76
parapsoriasis — type xantho-erythrodermia perstans, *Derm.* 91

- Fracture** dislocation of spine, *Orth.* 2, see also *Spondylitis*, traumatic
- Fractures** of patella, treatment of (R. H. A. Whitelocke), *Surg.* 111-119
- Framboesia**: history of its introduction into India, with personal observations in over 200 initial lesions (A. Powell), *Trop.* 15-42
see also under *Yaws*
- Franco-Prussian War**, venereal disease in, *War.* 16
- Frank**.—Method of opening uterus in lower segment, advantages and disadvantages, *Obst.* 54
- Frankau**, C., C.B.E., D.S.O.—Case of resection of liver for malignant disease spreading from gall-bladder, *Surg.* 59
- Franklin**, P.—Two cases of pulmonary tuberculosis with laryngeal symptoms, *Laryng.* 25
- Fraser**, A. Mearns.—Discussion on urgent need for education in control of cancer, *Occ. Lect.* 38-40
- Fraser**, F. R.—Discussion on action of quinidine in cases of cardiac disease, *Therap.* 25-30
- Fraser**, J. S.—Discussion on case of otitis media, with facial palsy following scarlet fever, *Otol.* 17
and **Turner**, A. Logan.—Labyrinthitis as a complication of middle-ear suppuration (abstract), *Otol.* 15
- Freud** and **Jung**.—Views on principles of psycho-analysis contrasted, *Psych.* 20
- Frogs**, response of, to drugs, effect of light on (E. Boock and J. W. Trevan), *Therap.* 8
- Frontal bone**, osteomyelitis of, extensive, case (F. H. Westmacott), *Laryng.* 93
bone, malar bone and maxilla, sarcoma of, case (F. H. Westmacott), *Laryng.* 92
sinus, invasion of, in case of orbital cellulitis, by osteo-myelitis of frontal bone (Sir W. Milligan and F. Wrigley), *Laryng.* 90
left, and left maxillary antrum, suppurative disease of, case (Sir W. Milligan and D. L. Sewell), *Laryng.* 90
- Fronto-parietal tumour**, right, with cracked-pot percussion note over right frontal bone (C. Riddoch and W. R. Brain), *Neur.* 84
- Frost**, A. T., Major.—Discussion on venereal disease as a war casualty, *War* 27
- Fry**, Dr.—Pathological report on case of multiple perforations of colon, resulting from dysentery, *Proct.* 68
- Gall-bladder**, carcinoma of liver secondary to, hepatectomy for, *Surg.* 63
containing gall-stone, outline of, X-ray appearance of, *Urol.* 9
healthy, not usually visible by radiology, *Electr.* 76
malignant disease spreading from, resection of liver for, case (C. Frankau), *Surg.* 59
normal and pathological, shadows of, as seen in radiogram, compared, *Urol.* 8
pathological (Mackenzie Davidson Memorial Lecture) (A. W. George), *Electr.* 75-90
pathological change in position of proximal portion of transverse colon diagnostic sign of, *Electr.* 79
- Gall-bladder**, diagnosis of, negative value of X-rays in, *Electr.* 81
filling of ampulla of Vater with barium in cases of, *Electr.* 79
investigation of cases, necessity for perseverance in, *Elect.* 84
negative value of X-rays in diagnosis of, *Elect.* 81
pressure deformity due to, causation of, *Electr.* 77
nature of, *Electr.* 76, 77
pressure effects upon antrum of stomach, *Electr.* 79
on duodenum, *Electr.* 77-79
spasm of antrum of stomach in diagnosis of, *Electr.* 81
visible, X-ray appearances of, *Electr.* 88
visible, without stones, X-ray appearance of, *Electr.* 84
- Gall-stone** superimposed on large branching renal calculus, X-ray appearances, *Urol.* 14
with pyelography, X-ray appearances of, *Urol.* 10
- Gall-stones**, X-ray appearances of, *Electr.* 82, 83, 84, 89, 90
see also under *Gall-bladder*, pathological
- Galloway**, Sir J., K.B.E., and **Hannay**, M. G.—Desquamative erythema associated with arthritic changes, case, *Derm.* 16-18
- Galvanic** response to stimulus, differentiation of hysterical syndrome from other forms of neuroses by, *Psych.* 6
- Galvanometer**, string, Einthoven's, introduction of, *Electr.* 3, 4
- Games**, encouragement of, in the Services, as counteracting venereal tendencies, *War* 20
- Gangosa**, *Trop.* 41
- Ganister** mining, dust from, nearly pure silica, leading to excessive mortality from phthisis, *Epid.* 98
- Gape-worm**, life history of (abstract) (R. J. Ortlepp), *Trop.* 44
- Gardiner**, F.—Discussion on case of parakeratosis variegata, *Derm.* 105
- Gardner**, H. Bellamy.—Discussion on anaesthesia in dental surgery, *Anæsth.* 20
- Gas-burn** scarring, case of (H. C. Semon), *Derm.* 95
- Gask**, G. E., C.M.G., D.S.O.—Medical services of Henry V's campaign of the Somme in 1415, *Hist.* 1-10
- Gaskell**, A., Surg.-Capt.—Discussion on physical training, *War* 37
- Gaskell**, J. F.—Discussion on significance of vascular and other changes in retina in arterio-sclerosis and renal disease, *Med. & Ophth.* 20
- Gas-oxygen** as anæsthetic in Cæsarean section, *Anæsth.* 3, 4
and C.E. combination as anæsthetic in dental surgery, *Anæsth.* 21
and ether as anæsthetic in dental surgery, *Anæsth.* 21
- Gas-poisoning** (war), tachycardia and gastric atony persisting after acute toxæmia, with low blood pressure, *Baln.* 2
- Gastrectomy**, partial, for cancer of stomach, case (R. P. Rowlands), *Clin.* 2

- Gastric and duodenal ulcer, dental sepsis in relation to, *Odont.* 14**
 mortality greater now than 40 years ago, *Med.* 45
 function, modification of, by means of drugs (abstract) (T. I. Bennett), *Therap.* 6, 7
 motility, substances affecting, *Therap.* 7
 secretion, diminished by atropine, *Therap.* 6
 drugs affecting, *Therap.* 6
 increased by pilocarpine, *Therap.* 6
 substances affecting, after its evolution, *Therap.* 7
 substances neutralizing acid secretion, *Therap.* 7
 substances replacing deficient acid secretions, *Therap.* 7
 ulcer, mortality from, 1880-1919, Guy's Hospital statistics, *Med.* 45
 mortality from 1901-1920, statistics, *Surg.* 7, 8
- Gastritis, phlegmonous, dental sepsis and, *Odont.* 13**
 septic, underlying septic anæmia, *Med.* 6
Gastrodiscoides, genus, remarks on, *Trop.* 13
Gastrodiscoides hominis, camera lucida tracing of, *Trop.* 10; digestive tract of, *Trop.* 10
- Gastro-enteritis, mortality from, 1901-1920, statistics, *Surg.* 8**
- Gastro-intestinal complications of dental sepsis, *Odont.* 13**
 mucosa, movements of (abstract), (G. Forssell), *Electr.* 91-94
 mechanism of, *Electr.* 91
 rôle of, in digestion, *Electr.* 92
 excretory system of, *Trop.* 12
 expulsion from body, means for, *Trop.* 13
 external characters, *Trop.* 9
 genital system of, male and female, *Trop.* 11-12
 geographical distribution, *Trop.* 13
 historical review, *Trop.* 9
 hosts of, *Trop.* 12
 references, *Trop.* 14
 synonyms of, *Trop.* 8
- Gauze, bismuth and glycerine application to nasal cavity (Sir StC. Thomson), *Laryng.* 29**
- "Geber," *Summa perfectionis*, Jābir ibn Hayyān, possible author of, *Hist.* 47**
- Genital organs, arrested development of, causing dysmenorrhœa, *Obst.* 114**
- Genius and insanity (H. J. Norman), *Psych.* 33-37**
 historical references to, *Psych.* 33, 34
 toxic factors in relation to, *Psych.* 36
 definitions of, *Psych.* 34, 35
 men of, influence of drugs on, *Psych.* 36
 mental breakdown in, *Psych.* 35
 mental stability associated with, instances of, *Psych.* 37
- Genu valgum, operation for, renal dwarfism after, case (Paul B. Roth.), *Orth.* 45**
- Geological formation of coalfields, influence upon mortality of miners, *Epid.* 90**
- George, Arial W.—The pathological gall-bladder (Mackenzie Davidson Memorial Lecture), *Electr.* 75-90**
- Germ theory, evolution of, *Epid.* 65**
- Gibson, A. G.—Use of Plombières douche, *Baln.* 22, 23**
- Gilchrist, T. C. (Baltimore).—Some problems in dermatology, *Derm.* 22**
- Giles, A. E.—Discussion on instruments left in peritoneal cavity, *Obst.* 43**
 indications for, and results of myomectomy for uterine fibroids, *Obst.* 13-21
 large fibroid of cervix developing after subtotal hysterectomy, *Obst.* 12
- Gill-Carey, C.—Case showing results following accidental swallowing of sulphuric acid in patient with syphilitic laryngitis, *Laryng.* 67**
- Gillies, H. D., C.B.E.—Case of depressed bony ridge of nose, *Laryng.* 4**
 of depressed fracture of nasal arch, *Laryng.* 6
 depressed fracture of nasal and associated bones, *Laryng.* 4
- Gimblett, C. L.—Discussion on case of hole in hyaloid, *Ophth.* 21**
 discussion on dental sepsis, *Odont.* 27
- Gingivitis, complete infection of teeth removed in cases of, *Odont.* 32**
 prophylaxis of, *Odont.* 23
 septic, purulent, less injurious than latent pyorrhœa, *Odont.* 39
- "Girdle-pain" in fracture-dislocation in dorsal or lumbar regions of vertebral column, *Orth.* 3**
- Glanders and anthrax, eradication of, in man and animals (Sir J. Moore), *Med.* 49-56**
 eradication of, mallein in, *Med.* 49
 methods of testing for, *Med.* 50, 51
 procedure of control in late war, *Med.* 50
- Glands, disease of, possible underlying ætiological factor in myotonia atrophica, *Neur.* 11**
- Glandular epispadias, two cases of (J. Swift Joly), *Urol.* 39-41**
 fever and infective mononucleosis (abstract) (H. L. Tidy), *Med.* 70-72
 identical, *Med.* 71, 72
 differential diagnosis of, *Med.* 71
 question whether a clinical entity, *Med.* 71
 relation to other diseases, *Med.* 71
- Glare, convergence and, *War* 49**
 in India, investigations into, *War* 50
 discomfort due to, in India, investigation into, *War* 46
 see also under *Visual fatigue in India*
- Glasgow, deaths from scarlet fever and amount of rainfall in (1856-1877), contrasted, *Epid.* 32, 33**
- Glioma, ependymal, growing from floor of fourth ventricle, simulating cerebellar abscess, in case of bilateral chronic suppurative otitis media, section of (T. H. Just), *Otol.* 62**
- Glossitic anæmia, *Med.* 9**
 and toxic nerve features, and their relation to one another, in pernicious anæmia, *Med.* 12
 hæmolytic anæmia, *Med.* 9
 see also under *Anæmia*
- Glossitis, Hunterian, in pernicious anæmia, *Med.* 7**
- Glosso-epiglottic furrows, submucous lipoma in (T. B. Layton), *Laryng.* 11**
- Glycerine and bismuth gauze, application to nasal cavity (Sir StC. Thomson), *Laryng.* 29**

- Glycosuria**, resulting in birth of dead child, treated with success in subsequent pregnancy (R. Wise), *Obst.* 35
- Goadby**, Sir K.—Discussion on dental sepsis, *Odont.* 17-19
- Goitre**, case of Graves' (Parry-Graves-Basedow) disease in woman without (E. Stolkind), *Clin.* 44
- Goldschmidt**, W. N.—Case of parenchymatous nephritis described, *Med. & Ophth.* 23-26
discussion on significance of vascular and other changes in retina in arterio-sclerosis and renal disease, *Med. & Ophth.* 23-26
- Golla**, F. L.—Organic basis of the hysterical syndrome, *Psych.* 1-11
- Gonads**, internal secretions of, preparations, *Therap.* 17
- Gonorrhoea**, antiquity of, *War* 15
complications of, treated with manganese butyrate, case, *Derm.* 69
in Scotland, incidence of, *Epid.* 81, 82
- Gonorrhoeal proctitis**, *Proct.* 16
stricture of rectum (Sir C. Symonds), *Proct.* 12-19
- Goodall**, J. Strickland.—Systematic examination of the heart, *Anæsth.* 25-30
- Goodwin**, A., and **Eden**, T. W.—Two cases of cancer of cervix treated by radium before operation, *Obst.* 32
- Gordon-Watson**, Sir C., K.C.M.G.—Discussion on ulcerative colitis, *Proct.* 109
- Gott and Rosenthal** on röntgenkymography, *Electr.* 19
- Gouldesbrough**, C.—Osteo-arthritis of the spine, *Med.* 63-70
- Gout**, dental sepsis in relation to, *Odont.* 16
- Granuloma** about apices and roots of teeth, *Odont.* 25, 31
angiomatous (multiple idiopathic pigment sarcoma of Kaposi), two cases of (J. H. Sequeira), *Derm.* 76
of pudenda, ulcerating, healing commencing immediately after administration of antimony, case (P. Manson-Bahr), *Clin.* 25
- Granulomata**, removal from liver, *Surg.* 51
- Graves'** disease and thyroid instability in the cow, and its relation to ovarian disease (L. P. Pugh), *Obst.* 92-99
and nymphomania in cows, breed and heredity in, *Obst.* 95
cardio-vascular symptoms, *Obst.* 96
comparison of, *Obst.* 95
exciting causes, *Obst.* 95
myxoedema as sequel in, *Obst.* 96
nervous symptoms, *Obst.* 96
thyroid enlargement in, *Obst.* 96
clinical picture of, *Obst.* 93
pelvic lesions in, *Obst.* 98
(Parry-Graves-Basedow), in woman, aged 69, without goitre, case (E. Stolkind), *Clin.* 44
- Gray**, A. A.—Cases of otosclerosis with unusual symptom (otosclerosis paradoxica) (abstract), *Otol.* 9, 10
discussion on labyrinthitis as complication of middle-ear suppuration, *Otol.* 15
on otosclerosis and osteitis deformans, *Otol.* 26
- Gray**, A. H. M., C.B.E.—Case of acne scrofulosorum, *Derm.* 101
of rodent ulcer under treatment with arsenic paste, *Derm.* 78
discussion on case of Boeck's sarcoid, *Derm.* 74
on case of mange in human being infected by dog, *Derm.* 76
on case of xanthoma (? diabetorum), *Derm.* 94
on cases of multiple superficial rodent ulcer, *Derm.* 26
on manganese as chemotherapeutic agent, *Derm.* 67
erythema of face, *Derm.* 79
generalized scleroderma with subcutaneous nodules, *Derm.* 107
male and female acarus extracted from one burrow, *Derm.* 87
so-called Kaposi's multiple idiopathic pigment sarcoma, case, *Derm.* 78
- Gray**, H. Tyrrell—Cases of: (1) Congenital absence of shoulder muscles; (2) congenital absence of radius with extreme eversion of hand; (3, 4) congenital hydrocephalus (two cases); (5) acute osteomyelitis of right tibia; (6) congenital absence of pectoral muscles in male infant; (7) dextrocardia without transplantation of other viscera; *Child.* 43, 44
- Greek medical etiquette** (W. H. S. Jones), *Hist.* 11-17
comparison with that of present time, *Hist.* 17
enumeration of acts debarred and conduct to be pursued, *Hist.* 13
physicians not forbidden to advertise, *Hist.* 14
question of fees under, *Hist.* 15
severance of surgery and medicine under, *Hist.* 14, 15
- Greeks**, medical guild among (Asclepiadæ), *Hist.* 12
- Greenfield**, J. G.—Discussion on case of "spondylose rhizo-mélique," *Neur.* 47
and **Martin**, J. P., tumour of cisterna magna, *Neur.* 32-35
- Greenwood**, M.—Discussion on age and sex distribution in scarlet fever, *Epid.* 30
on economics and tuberculosis, *Epid.* 16
on incidence of syphilis in Scotland, *Epid.* 84
on mortality of coal- and metalliferous-miners, *Epid.* 100
on surface diseases of the eyes, *Epid.* 63
- Greeves**, R. A.—Case of bilateral proptosis, with limitation of movement in one eye, *Ophth.* 15
discussion on case of ectopia lentis, *Ophth.* 13
on cases of endothelioma of orbit, *Ophth.* 37
on cases of primary band-shaped opacity of both corneæ, *Ophth.* 32
- Grellier**, B., M.C.—Case of multiple dentigerous cysts, *Odont.* 43, 44
- Griffith**, H. K. and **White-Cooper**, W. R.—Case of obstructed labour, *Obst.* 50
inversion of uterus occurring in third week of puerperium, *Obst.* 48, 49
- Gripper**, W.—Case of congenital subluxation of humeri, *Orth.* 30
- Grove**, W. R.—Discussion on present position of organotherapy, *Therap.* 18

- Gudin's** method of anastomosis of colon, *Surg.* 77
- Gullet**, large foreign bodies in, impacted in, treatment of (D. R. Paterson) (abstract), *Laryng.* 77
cases described, *Laryng.* 78
see also *Œsophagus*
- Gum** infections, one form of dental sepsis, *Odont.* 22
see also *Gingivitis*.
- Gums** and teeth, clinical macroscopical signs of unhealthy conditions of, *Odont.* 10
infections of, in their relationship to nose, throat and ear, discussion on, *Odont.* 35-42
- Gumma**, facial, parathyroid medication in, *Therap.* 20
- Gummata**, removal from liver, *Surg.* 57
- Gunewardene**, H. O. and **Cantrell**, B. W.—Simple instrument for withdrawing serous effusions, *Clin.* 38, 39
- Gunn**, Marcus.—Ophthalmoscopic evidences of arterio-sclerosis (quoted), *Med. & Ophth.* 7
- Gunshot** fracture of thigh, comminuted, showing displacement of fragments, *Path.* 34
injuries, disruptive phenomena in, physics of (S. G. Shattock), *Path.* 17-34
condition of leaden case of bullet when fired considered, *Path.* 31, 32
cone of compressed air accompanying bullet, negligible, *Path.* 17, 18
experiment with British rifle bullet, *Path.* 29
in long bones, *Path.* 28
in skull, *Path.* 21, 22
in punctured fractures of skull, *Path.* 25
in relation to spin of bullet, *Path.* 19
to velocity or force of impact, *Path.* 21
- Guthrie**, D.—Chronic hyperplasia of upper jaw: its relationship to other osseous tumours and to otosclerosis (abstract), *Laryng.* 73
- Guy's** Hospital, mortality statistics 1880-1919 for peritonitis, gastric ulcer, duodenal ulcer, perityphlitis and appendicitis, *Med.* 45
- Gynæcological** and obstetric practice, value of ergot in, with special reference to its present position in British Pharmacopœia (H. H. Dale, with discussion), *Obst. & Therap.* 1-7
- "Gynergen"** (solution of ergotamine salts), *Obst. & Therap.* 4
- Habits** in relation to causation of caries of teeth and pyorrhœa, *Odont.* 3
- Hadfield**, C. F.—Discussion on anæsthetization in Cæsarean section, *Anæsth.* 4
- Hæmangioma** of orbit (L. Paton), *Ophth.* 13
- Hæmangiomata** of liver, hepatectomy for, *Surg.* 64
- Hæmatoma** of ovary, ruptured, with extensive intraperitoneal hæmorrhage, case (L. C. Rivett), *Obst.* 81
- Hæmatoporphyrinuria**, toxic, acute, chronic poisoning causing, *Neur.* 25
- Hæmatoporphyrinuric** polyneuritis, cases and clinical description of, *Neur.* 23-25
- Hæmaturia**, complication of calcified abdominal glands, *Urol.* 7
- Hæmoclastic** test, Widal's, in case of enlarged liver with persistent acetoneuria and diaceturia, *Child.* 58
- Hæmolytic** anæmic disease, see *Anæmia*, pernicious
- jaundice, congenital, case (F. Parkes Weber), *Child.* 66
- Hæmorrhage**, cerebral, at birth, fatal case, *Child.* 76
control of, in resection of liver, *Surg.* 52
in case of erythræmia (J. A. Ryle), *Med.* 83
danger of, in cauterization of adhesions under guidance of thoracoscope, *Electr.* 48, 49, 50
intraperitoneal, extensive, ruptured hæmatoma of ovary with (L. C. Rivett), *Obst.* 81
risk of, in splitting kidney for removal of calculi, *Surg.* 38
secondary, following suprapubic prostatectomy, *Surg.* 123
tremendous, after extraction of one tooth, (cirroid aneurysm), *Anæsth.* 19
uterine, severe and persistent, treatment by radium, with report on 45 cases (S. Forsdike), *Obst.* 69-78
- Halberstaedter**, views on yaws quoted, *Trop.* 32, 33
- Haldane**, J. S., F.R.S.—Discussion on medical aspects of life at high altitudes, *Med.* 62
- Hale-White**, Sir W., K.B.E.—Edward Jenner (Centenary Celebration), *Occ. Lect.* 1-10
Pasteur in relation to medicine (Pasteur Celebration), *Occ. Lect.* 11-16
- Halisteresis** and absorption of bone in antral sinusitis, *Odont.* 39
- Hall**, A.—Discussion on late effects of encephalitis lethargica, *Child.* 34
- Hamer**, Sir W.—Discussion on age and sex distribution in scarlet fever, *Epid.* 28
on economics and tuberculosis, *Epid.* 18
on surface disease of the eyes, *Epid.* 62
the ultraviolet viruses considered from an epidemiological point of view, *Epid.* 65-76
- Hamerton**, A. E.—Establishment of an Antirabic Institute in the Tropics, *Trop.* 49-55
- Hammer-toes**, double, involving both great toes (A. E. M. Woolf), *Clin.* 1
- Hamstrings**, transplantation of (W. R. Bristow), *Orth.* 13
- Hand**, phalanx in, chondroma of, case (St. J. D. Buxton), *Clin.* 27
right, muscular atrophy of "peroneal" type commencing in, and for some time confined to; case (C. P. Symonds), *Neur.* 80
- Hands** and feet, atrophic dermatitis of, ? lupus erythematosus, case (H. W. Barber), *Derm.* 99
- Handley**, W. Sampson.—Subcapsular pyelotomy, with remarks on origin and treatment of renal calculi, *Surg.* 21-37
- Hannay**, M. G.—Case of cutis verticis gyrata, *Derm.* 88
case of scleroderma, *Derm.* 60
and **Galloway**, Sir J., K.B.E.—Desquamative erythema associated with arthritic changes, case, *Derm.* 16-18
- Harborow**, G.—Case of unerupted incisors and canines in male aged 59, *Odont.* 73, 74

- Hare, D.**—Case of swelling of face, *Clin.* 33
- Harford, C. F.**—Discussion on endocrine factor in mental disease, *Psych.* 31
on significance of vascular and other changes in retina, in arterio-sclerosis and renal disease, *Med. & Ophth.* 32
- Harman, N. Bishop**—Epidemiology of surface diseases of eyes, *Epid.* 49-62
standards of vision for scholars and teachers in Council Schools (abstract), *Ophth.* 7
- Harris, W.**—Discussion on case for diagnosis, *Neur.* 51
on treatment of neuro-syphilis, *Neur.* 76
multiple peripheral neuritis, *Neur.* 13-26
- Harrison, G. A.**—Discussion on factors in uræmia, *Urol.* 28
- Harrison, L. W., D.S.O.**—Discussion on treatment of neuro-syphilis, *Neur.* 72
- Hart, M. D. and Smith, W. Whately**—New apparatus for measuring sensori-motor reaction times, *Electr.* 63-69
- Hastate cells of dental pulp**, *Odont.* 62
- Hastings, Somerville**—Case of pedunculated angioma (bleeding polypus) of inferior turbinal, *Laryng.* 25
discussion on case of œdema of septum in association with nasal polypi, *Laryng.* 28
on operative procedure for bilateral abductor paralysis, *Laryng.* 39
- and **Tucker, W. S., Major, R.E.**—An attempt to standardize tests for hearing, *Otol.* 1-5
- Hawthorne, C. O.**—Discussion on significance of vascular and other changes in retina in arterio-sclerosis and renal disease, *Med. & Ophth.* 20
- Haynes, Lt.-Col.**—Discussion on endocrine factor in mental disease, *Psych.* 31
- Head, Henry, F.R.S.**—Discussion on treatment of neuro-syphilis, *Neur.* 73
- Headache** in uræmia, significance and causation, *Urol.* 24
- Hearing**, testing of, standard apparatus for demonstration of, *Otol.* 2, 3
standard apparatus for, results obtained by, *Otol.* 4
tests for, attempt to standardize (S. Hastings and Major W. S. Tucker), *Otol.* 1-5
times of improvement in, during course of otosclerosis, *Otol.* 10, 11
- Heart**, action of chloroform on (A. G. Levy), *Anæsth.* 30, 31
and great vessels, in abnormal conditions, oblique lateral views of chest showing, *Electr.* 14
position of, in relation to anatomical structure of thorax, *Electr.* 13
block, case of (O. Leyton), *Clin.* 35
chambers of, schematic drawing showing position of, in skiagram, *Electr.* 13
dilatation of, during anæsthesia, *Anæsth.* 13
disease, action of quinidine in cases of, discussion on, *Therap.* 25-42; prognosis and tendency to relapse, *Therap.* 36
case of (G. M. Kendall), *Child.* 69
diagnosis, physical methods of, *Electr.* 1-30
quinidine administration resulting in dangerous symptoms, four cases of, *Therap.* 26
- Heart disease**, selection of cases for quinidine therapy, *Therap.* 37
restoration to normal rhythm as therapeutic procedure, *Therap.* 35
see also *Quinidine*
lesion, congenital, associated with small pulse and ichthyosis, case (O. C. M. Davis), *Child.* 80
massage in case of cardiac arrest under anæsthetics (E. S. Rowbotham), *Anæsth.* 5
measuring apparatus, diagram of, *Electr.* 17
musculature of, effects of quinidine on, *Therap.* 30-32, 42
rate and force of, effect of hydrotherapeutic measures on, *Baln.* 9
rate of, effects of exercise on, *Anæsth.* 29
rhythm, effects of exercise on, *Anæsth.* 29
irregularities of, classification of, *Anæsth.* 27
sounds at apex, interpretations of, *Anæsth.* 27
at base, interpretations of, *Anæsth.* 28
effects of exercise on, *Anæsth.* 29
sympathetic innervation of, *Baln.* 10
systematic examination of (J. S. Goodall), *Anæsth.* 25-30
clinical signs and their interpretation, *Anæsth.* 26, 27, 28
value of electro-cardiographic records in, *Anæsth.* 30
of X-ray examinations in, *Anæsth.* 30
thrills, effects of exercise on, *Anæsth.* 29
X-ray examination of, essentials of, *Electr.* 21
see also under *Cardiac*
- Heath, D.**—Discussion on case of folliculitis decalvans, *Derm.* 50
- Heath, P. Maynard.**—Late result of beef-bone graft of humerus, *Orth.* 30
subluxation of inner end of right clavicle, *Orth.* 12
traction fracture of small trochanter, *Orth.* 12
- Hemianopia**, quadrant, intracranial tumour causing (R. Ford), *Ophth.* 30.
- Hemiplegia** in pregnant woman at full term, sudden onset accompanied by transient albuminuria, Cæsarean section, gradual recovery. case (F. Cook), *Clin.* 43
in uræmia, significance and causation of, *Urol.* 24
mortality in 1880 and 1920 compared, *Med.* 47
right, Parkinsonian syndrome associated with, showing peculiar disturbances of tone and posture in limbs on hemiplegic side, sequelæ of lethargic encephalitis, case (D. McAlpine), *Neur.* 27
spastic, case of (G. Perkins), *Child.* 75
due to birth injury, *Child.* 77
differential diagnosis from that due to polio-encephalitis, *Child.* 78
- Henderson, P. H., Lieut.-Col.**—Discussion on physical training, *War* 38
on venereal disease as a war casualty, *War* 25, 26, 29
- Hendon** cow disease, *Epid.* 40
controversy as to, *Epid.* 40
- Henry V's** campaign of Somme in 1415, medical services of (G. E. Gask), *Hist.* 1-10
- Hepatectomy**, partial, remarks on (G. G. Turner), *Surg.* 43-56
analysis of urine and fæces twenty-two months after, *Surg.* 48

- Hepatectomy**, hæmorrhage in, methods of controlling, *Surg.* 52, 53
 in case of tumour of liver in woman: recovery, *Surg.* 48
 indications for, *Surg.* 51, 62-64
 suture of liver after, difficulty of, *Surg.* 53
 methods of, *Surg.* 54
 technique of, *Surg.* 55
 see also *Liver*, resection of
- Hepburn, M. L.**—Discussion on case of retinitis circinata, *Ophth.* 11
 discussion on medical aspects of life at high altitudes, *Med.* 61
- Hereditation** in causation of caries of teeth and pyorrhœa, *Odont.* 3
- Heredity** in myopia, *Ophth.* 9
 in reference to yaws, *Trop.* 41
- Hern, W.**—Discussion on dental sepsis, *Odont.* 22
- Hernaman-Johnson, F.**—Discussion on vagus and sympathetic nerves and their relation to climate and hydrology, *Baln.* 14
- Hernia**, mortality from, 1901-1920, statistics, *Surg.* 8
- Herpes**, epidemics of, associated with chicken-pox, *Ophth.* 29
 ophthalmicus, optic atrophy after (L. Paton), *Ophth.* 27-30
 optic atrophy after, cases reported, *Ophth.* 28
 Hutchinson's law in, *Ophth.* 28
 zoster of conjunctiva with iritis, parathyroid medication in, *Therap.* 21
- Hewer, C. L.**—Effects of vagal trauma on anesthetized patient, *Anæsth.* 7-9
- High altitudes**, arterial oxygen saturation at, *Med.* 58
 change in oxygen dissociation curve at, *Med.* 58
 flying at, immediate and remote effects of, *Med.* 59
 number and condition of red blood corpuscles at, *Med.* 58
- Hill, W.**—Discussion on case of pharyngeal pouch, *Laryng.* 41
 discussion on case of unusual tonsillar appendage, *Laryng.* 16
 on lingual tonsil, *Laryng.* 75
 on operative procedures for bilateral abductor paralysis, *Laryng.* 38
 on operative treatment of middle-ear suppuration, *Otol.* 6
 on treatment of large foreign bodies impacted in gullet, *Laryng.* 80
- Hine, M. L.**—Case of ectopia lentis (both eyes), *Ophth.* 12
 familial nodular and reticular keratitis (cases), *Ophth.* 43-45
 two cases of early familial maculo-cerebral degeneration, *Ophth.* 18, 19
- Hinsdale, G.**—The "Aerologia" of Domenico Panarolo, *Baln.* 19-21
- Hip-joint**, disease of, multiple exostosis and, case (C. E. Shattock), *Clin.* 2
 dislocation of, congenital and pathological, operative treatment of, discussion on, *Orth.* 15-25
 congenital, treatment by making upper lip to acetabulum, *Orth.* 17
 by making upper lip to acetabulum, indications for and technique of, *Orth.* 18, 19
- Hip-joint**, dislocation, congenital, treatment by open reduction, *Orth.* 15
 anatomical points, *Orth.* 16, 17
 indications for and technique of, *Orth.* 16
 by operation for relief of pain, *Orth.* 22
 operative, types of, *Orth.* 15
 results of operations, *Orth.* 20, 21
 following arthritis, treatment of, *Orth.* 23
 osteo-chondritis of (D. M. Aitken), *Orth.* 13
 paralytic, treatment of, *Orth.* 23
 snapping, case of (B. W. Howell), *Orth.* 46
- Hippocratic** Collection, earlier treatises of, lack of references to etiquette in, *Hist.* 13
 Tracts referring to medical etiquette, *Hist.* 12
 decorum, *Hist.* 12
 Law, *Hist.* 11, 12
 penalties under, *Hist.* 11
 Oath, *Hist.* 11
 professional secrecy enjoined by, *Hist.* 16
 Precepts, *Hist.* 12, 13
 in relation to advertisement (quoted), *Hist.* 14
 in relation to question of fees (quoted), *Hist.* 15
- Hirschfeld, L. and H.**—Significance of blood-groups (quoted), *Path.* 42, 43
- Hirschsprung's disease**, partial relief following plication of sigmoid flexure, case (W. G. Spencer), *Clin.* 31
- Histamine** in ergot preparations, *Obst. & Therap.* 8
- Hoarseness** due to singer's nodes, case (Sir J. Dundas-Grant), *Laryng.* 44
- Hobday, F. G. T.**—Calculi in urinary track in animals, *Surg.* 41
 discussion on case of mange in human being infected by dog, *Derm.* 76
 on operative procedures for bilateral abductor paralysis, *Laryng.* 39
- Holland, E.**—Discussion on Cæsarean section, *Obst.* 58
 discussion on instances of instruments left in peritoneal cavity, *Obst.* 44
 leiomyosarcoma of a fibromyoma removed by subtotal hysterectomy, *Obst.* 64
 primary carcinoma of vagina, specimen of, *Obst.* 25
- Holmes, Gordon**—Discussion on case of complete amaurosis, dementia and spastic paralysis in Hebrew boy aged 10, *Neur.* 30
 discussion on case of progressive macular degeneration with tremors, *Ophth.* 42
 on eighth nerve tumours, *Otol.* 40
- Holmyard, E. J.**—Jābir ibn Hayyān, *Hist.* 47-53
- Holzknicht's screening stand**, *Electr.* 8, 9
- Hopewell-Smith, A.**—Observations on histology, physiology, and pathology of dental pulp, *Odont.* 58-71
 two odontomes, *Odont.* 55-58
- Horde, Sir T.**—Discussion on ulcerative colitis, *Proct.* 96
- Hormones**, thyroïdal, use of, *Therap.* 15
- Hormotone** in treatment of dysmenorrhœa, *Obst.* 111, 114
- Horne, W. Jobson.**—Specimens of tumours of interarytænoid space of larynx; demonstration, *Laryng.* 58
 discussion on case of chronic laryngitis of long standing, *Laryng.* 24

- Horses**, effects of cocaine upon, *Therap.* 46
 of morphine upon, *Therap.* 45
 of strychnine upon, *Therap.* 47
 roaring in, operation for, *Laryng.* 32, 40
 sacculus and ventricle in, contrasted with human structure, *Laryng.* 34
- Housing** problem, hygienic aspect of, *Odont.* 5
- Hovell**, T. Mark.—Discussion on case of multiple papillomata of larynx, *Laryng.* 45
 discussion on infection of teeth and gums in their relationship to nose, throat, and ear, *Odont.* 40
- Howarth**, W. G.—Complete laryngectomy for malignant disease, *Laryng.* 49
 discussion on treatment of large foreign bodies impacted in gullet, *Laryng.* 80
 extensive lupus of palate, pharynx and larynx, *Laryng.* 50
 laryngostomy for complete subglottic stenosis, *Laryng.* 48
 tuberculous ulcer of dorsum of tongue, *Laryng.* 50
 ventriculo-chordectomy for double abductor paralysis, case, *Laryng.* 47
- Howell**, B. Whitchurch.—Case of snapping hip, *Orth.* 46
 of tendon transplantation, *Orth.* 50
- Howell**, C. M. Hinds.—Case for diagnosis, *Neur.* 51
 of syringomyelia, with much sensory and motor impairment and little wasting, *Neur.* 50
- Hudson**, A. C.—Two cases of primary band-shaped opacity of both corneæ, *Ophth.* 31
- Hughes**, C.—Discussion on anæsthesia in dental surgery, *Anæsth.* 21
- Hughes**, P. T.—Discussion on endocrine factor in mental disease, *Psych.* 31
- Humeri**, subluxation of, congenital, case (W. Gripper), *Orth.* 30
- Humerus**, beef-bone graft of, late results (M. Heath), *Orth.* 30
- Hunter**, John.—Correspondence with Edward Jenner (quoted), *Occ. Lect.* 2
- Hunter**, W., C.B.—Discussion on case of acquired chronic hæmolytic (acholuric) jaundice, *Med.* 77
 discussion on dental sepsis, *Odont.* 19-21
 nervous and mental disorders of severe anæmias in relation to their infective lesions and blood changes, *Med.* 1-42
- “**Hunterian glossitis**,” *Med.* 7
- Huntington's** chorea, case of (C. Worster-Drought), *Neur.* 82
- Hurst**, A. F.—Achlorhydria in relation to other diseases, quoted in reference to dental sepsis, *Odont.* 30
 definition of asthma (quoted), *Therap.* 1
 discussion on ulcerative colitis, *Proct.* 106
- Hutchinson**, Sir J.—Original description of cases of retinitis circinata, *Ophth.* 12
- “**Hutchinson's law**” in optic atrophy, after herpes ophthalmicus, *Ophth.* 28
- Hutchinsonian** teeth, case of (A. T. Pitts), *Child.* 45
- Hutchison**, A. J.—Discussion on cases of intrinsic cancer of larynx treated by laryngofissure, *Laryng.* 61
- Hutchison**, R.—Case of pellagra, *Child.* 61
 congenital stricture of œsophagus, specimen of, *Child.* 42
 discussion on case for diagnosis, *Child.* 55
- Hutchison**, R.—Discussion on case of enlarged liver with persistent acetonuria and diaceturia, *Child.* 59
 discussion on cases of duodenal obstruction in infants, *Child.* 53
 of osteogenesis imperfecta, *Child.* 72
 of splenomegaly and congenital hæmolytic jaundice, *Child.* 67
 two cases of acholuric jaundice, *Child.* 41
- Huxley**, definition of genius by, *Psych.* 34
- Hyaloid**, hole in, case of (R. L. Rea), *Ophth.* 20
- Hydrocephalus**, congenital, two cases of (H. T. Gray), *Child.* 43
- Hydrocyanic acid gas**, use of, for rat destruction on ships, *Epid.* 42
- Hydrology**, climate and: sympathetic and vagus nerves and their relation to, discussion on, *Baln.* 7-17
- Hydronephrosis** of single kidney; spontaneous rupture into peritoneal cavity, case (P. Turner), *Clin.* 24
- Hydrophobia**, cases of, in Irak, *Trop.* 50
- Hydrotherapeutic measures**, effects on autonomic nervous system, *Baln.* 9
 effects on force and rate of heart, *Baln.* 9
 localized applications inducing changes in internal organs reflexly connected with area of skin treated, *Baln.* 9
 reflex character of induced changes, *Baln.* 9
- Hygienic** aspect of housing problem, *Odont.* 5
- Hyperæsthesia** in fracture-dislocations in dorsal and lumbar regions of vertebral column, *Orth.* 3
- Hyperkeratosis**, congenital or developmental, *Derm.* 95
 plantar, as sequel of yaws, *Trop.* 41
- Hypermetropia**, standard for scholars and teachers in Council schools, *Ophth.* 8
- Hyperpiësis**, variability of, *Med. & Ophth.* 2, 34
- Hyperplasia**, chronic, of upper jaw; relationship to other osseous tumours and to otosclerosis (D. Guthrie), *Laryng.* 73
- Hyperthyroidism**, dental sepsis in relation to, *Odont.* 16
- Hysterectomy** compared with myomectomy in treatment of uterine fibroids, mortality statistics, *Obst.* 14
 operations, proportion of myomectomy operations to, at different ages, table, *Obst.* 20
 subtotal, large fibroid of cervix developing after (A. E. Giles), *Obst.* 12
 leiomyosarcoma of a fibromyoma removed by (E. Holland), *Obst.* 64
 mass of secondary leiomyosarcoma following (A. C. Palmer), *Obst.* 62
- Hysteria**, abnormal suggestibility of subject of, *Psych.* 3, 4, 7
 age-incidence of commencement of symptoms, *Psych.* 9
 Babinski's views on (quoted), *Psych.* 3
 behaviour in, purely imitative, *Psych.* 7
 definitions of, *Psych.* 2, 3
 diminution of response effective in, central or peripheral origin considered, *Psych.* 8, 9
 enfeeblement or suppression of affective response in, *Psych.* 6
 excessive motor reflex response in subjects of, *Psych.* 7
 Lasègue's dictum on, *Psych.* 2

- Hysteria**, muscular hypertonicity in subjects of, *Psych.* 4, 5, 78
 psychology of, difficulty in evaluation of, *Psych.* 3
 symptoms of, *Psych.* 3
Hysterical fits. See under *Fits*, hysterical
 syndrome, organic basis of (F. L. Golla), *Psych.* 1-11
 disorder of mechanism of conduct must precede, *Psych.* 1
- Ichthyosis** and congenital heart lesion associated with small pulse, case (O. C. M. Davis), *Child.* 80
 unusual localization of, case of (H. C. Semon), *Derm.* 94
- Identification**, definition of, *Psych.* 13
 process in psychotherapy, *Psych.* 13
- Ileocolic glands**, calcified, with pyelography, X-ray appearances of, *Urol.* 11.
- Ileo-sigmoidostomy**, *Surg.* 79
- Ileum**, anastomosis of, to large gut, method of, *Surg.* 79
 mesentery of coil of large spindle-celled sarcoma arising in, successfully removed by operation, case (F. Kidd), *Surg.* 86
 sarcoma of, case (C. Rowntree), *Surg.* 85
- Immunity** in relation to site of inoculation of test organism, *Path.* 8
 natural, or natural resistance, *Path.* 1
 problem of, *Path.* 2
 rationale of, *Path.* 3, 4
- Impetigo**, demodex, photographs of (A. Whitfield), *Derm.* 28
- Incisors** and canines, unerupted, in male aged 59, case (G. Harborow), *Odont.* 73, 74
- Incus**, ossification of, to tegmen (S. Scott), *Otol.* 20
- India**, contagious animal disease in civil districts in, *Med.* 54
 glare and convergence among air-pilots in, investigations into, *War* 50
 history of introduction of frambœsia into, with personal observations on over 200 initial lesions (A. Powell), *Trop.* 15-42
 syphilis in, incidence, *Trop.* 15
 visual fatigue in, investigations into, *War* 46
 widespread prevalence of pyorrhœa in, *Odont.* 26
 yaws in, cases reported by various authorities, *Trop.* 17, 18
- Indian kala-azar**, occurrence of *Leishmania* in intestinal tissues in, pathological changes occasioned by their presence and their possible significance in this position (Lt.-Col. H. M. Perry), *Trop.* 1-8
- Infantile**, cœliac, in convalescence (non-diarrhœic) stage, two cases (R. Miller), *Child.* 22, 23
 scorbutic, case (M. Cassidy), *Clin.* 16
- Infants**, acute purulent rhinitis in, sequelæ of, *Odont.* 37
 duodenal obstruction in, two cases (R. C. Jewesbury), *Child.* 10-12
- Infective** lesions underlying pernicious anæmia. *Med.* 6, 8
 underlying septic anæmia, *Med.* 6, 8
- Influenza**, acute antral sepsis arising in course of, *Odont.* 41
- Influenza**, blood pressure low in, *Baln.* 2
 leucocyte count in, typical case of, *War* 7
 trench fever and sandfly fever, temperature and leucocyte curves compared, *War* 8
- Ingleby**, Helen.—Case of cerebral degeneration, *Child.* 15
- Inhibition** reflex, produced by nervous impulses, *Neur.* 57 (footnote)
- Innominate artery and aorta**, aneurysm of, in woman, case (B. Myers), *Clin.* 9
- Insanity**, delusional, in uræmia, significance and causation of, *Urol.* 24
 genius and (H. J. Norman), *Psych.* 33-37
 paranoid and delusional, co-existent with high degree of intellect, *Psych.* 36
- Insomnia** in uræmia, significance and causation of, *Urol.* 24
- Instincts** and feelings, relationship to endocrine system, *Psych.* 27
- Instrument** for withdrawing serous effusions (H. O. Gunewardene and B. W. Cantrell), *Clin.* 38, 39
- Instruments** left in peritoneal cavity; effects and results as shown by analysis of 44 hitherto unpublished cases (C. White), *Obst.* 36-43
 frequency of accident from, *Obst.* 39
- Insulin**, extraction from islands of Langerhans, *Therap.* 17
- Intellect**, paranoid and delusional insanity co-existent with high degree of, *Psych.* 36
- Interarytænoid space** of larynx, tumours of, specimens of (W. J. Horne), *Laryng.* 58
- Interventricular septum**, patent, case of (M. A. Cassidy), *Clin.* 4
- Intestinal obstruction**, mortality from, 1901-1920, statistics, *Surg.* 7, 8
 perforation as complication of ulcerative colitis, incidence of, *Proct.* 93
 tissues, occurrence of *Leishmania* in, in Indian kala-azar, pathological changes occasioned by their presence and their possible significance in this situation, (Lt.-Col. H. M. Perry), *Trop.* 1-8
- Intestine**, large, anastomosis of, lateral, disadvantages of, *Surg.* 72
 result dependent upon blood supply of joined ends, *Surg.* 73
 small, changes in due to exposure to radium, *Electr.* 42
 microscopical appearances in two cases of Indian kala-azar, *Trop.* 2, 3
 sarcoma of, two cases of (C. Rowntree), *Surg.* 85
- Intestines**, diseases of, mortality from, 1901-1920, statistics, *Surg.* 8
- Intracranial tumour**, causing quadrantic hemiopia (R. Ford), *Ophth.* 30
- Intradermic test** of hyperexcitability of sympathetic nervous system, *Psych.* 8
- Irak**, rabies in, occurrence of, *Trop.* 50
- Irido-cyclitis**, dental sepsis in relation to, *Odont.* 27
- Iris**, deposits on, in case of calcareous degeneration of eye (R. Batten), *Ophth.* 1
- Iritis**, tubercular, *Ophth.* 3
- Iron**, oxide of, dust of, occupations with exposure to, with reference to mortality rates, *Epid.* 97

- Ironstone miners**, comparative mortality among (1910-1912) from certain causes, *Epid.* 97
mortality among, from phthisis per 1,000 living at various age periods, *Epid.* 96
from various causes, *Epid.* 94
- Ischæmic paralysis**, case (H. A. T. Fairbank), *Orth.* 11
- Ischio-rectal abscesses**, new method of treating (J. P. Lockhart-Mummery), *Proct.* 65
- Islam**, intellectual atmosphere of, in eighth century A.D., *Hist.* 55
- Isserlis**, Dr.—Discussion on age and sex distribution in scarlet fever, *Epid.* 29
- Jābir ibn Hayyān** (E. J. Holmyard), *Hist.* 47-58
birthplace and life, *Hist.* 47-49
chemical theories of, *Hist.* 57
definition of nature and chemistry by, *Hist.* 56
estimate of his chemical knowledge and his contributions to chemistry, *Hist.* 55
instructions for preparing chemical compounds, *Hist.* 57
possible identity with "Geber," *Hist.* 47
writings of, *Hist.* 47-51
- Jackson**, Chevalier.—Method of performing ventriculo-chordectomy, *Laryng.* 47
- Jacobæus**, H. C.—(auterization of adhesions in artificial pneumothorax treatment of pulmonary tuberculosis under thoracoscopic control, *Electr.* 45-60
- James**, W. Warwick.—Discussion on anæsthesia in dental surgery, *Anæsth.* 20
- Japan**, vaccination in, history of (Prof. M. Miyajima), *Hist.* 23-26
establishment of clinics for, *Hist.* 24
of institute for preparation of calf vaccine, *Hist.* 26
first published report on, *Hist.* 24
made compulsory in 1875, *Hist.* 26
pictorial circulars for popularizing, *Hist.* 24, 25
- Jaundice**, acholuric, cases of (V. Coates), *Clin.* 28; (D. Paterson) *Child.* 41; (R. Hutchison) *Child.* 41
case of pernicious anæmia dying in state of, post mortem character of, *Med.* 78
chronic hæmolytic (acholuric) acquired, case seen 15 years ago, with blood picture at that time resembling one of pernicious anæmia (F. Parkes Weber), *Med.* 73-77
congenital, non-familial, rare case of, without enlargement of liver or spleen, in an otherwise healthy man aged 56 (F. Parkes Weber), *Med.* 81-83
due to excessive doses of arsenical compounds, *Neur.* 74, 75, 76, 77
hæmolytic, congenital case (F. Parkes Weber), *Child.* 66
treatment of, by splenectomy, *Med.* 77
persistent, in infant, atresia of common bile-duct and biliary cirrhosis, case (B. Myers), *Child.* 17
with enlargement of liver, accompanying congenital family cholæmia, *Med.* 80
- Jaw**, ankylosis of, case (G. E. Waugh and A. T. Pitts), *Child.* 44
- Jaw**, upper, chronic hyperplasia of: relationship to other osseous tumours and to otosclerosis (D. Guthrie), *Laryng.* 73
hæmorrhagic angiosarcoma of, case (H. J. Banks-Davis), *Laryng.* 49
malignant disease in, treatment of, certain pathological and surgical points in (E. M. Woodman), *Laryng.* 87
removal of, in case of carcinoma of antrum (A. A. Smalley), *Laryng.* 94
- Jaws and teeth in puppies**, effects on, of diet sufficient in amount but defective in quality, experiments showing, *Odont.* 75, 76
upper and lower, nasal septum and floor of both nostrils, epithelioma of, case (A. Wylie), *Laryng.* 30
- Jeanselme**, views on yaws, quoted, *Trop.* 32
- Jejunum**, lesions in, in case of pernicious anæmia, *Med.* 15
sarcomatous cyst of, case (C. Rowntree), *Surg.* 86
- Jenkins**, G. J.—Discussion on case of brain abscess due to otitic infection, *Otol.* 54
discussion on cases of otosclerosis with unusual symptoms, *Otol.* 11
on labyrinthitis as complication of middle-ear disease, *Otol.* 16
on tests for hearing, *Otol.* 4
otosclerosis and osteitis deformans: a pathological and clinical comparison (abstract), *Otol.* 21-26
tinnitus associated with facial spasm, *Otol.* 8
- Jenner**, Edward.—Celebration of centenary of, *Occ. Lect.* 1-10
as naturalist, *Occ. Lect.* 2
correspondence with John Hunter, *Occ. Lect.* 2
experiments leading to introduction of vaccination, *Occ. Lect.* 4, 5
first account of association of diseased coronary arteries and angina pectoris, *Occ. Lect.* 3
honours conferred on, *Occ. Lect.* 8, 9
monetary grant to, by House of Commons (1802), *Occ. Lect.* 7, 8
paper on "Observations on Natural History of Cuckoo," *Occ. Lect.* 2
publication of work entitled "Enquiry," *Occ. Lect.* 5
- Jewell**, W. H., O.B.E.—Discussion on case of extensive lupus of palate, pharynx and larynx, *Laryng.* 52
tumour of larynx (? malignant), *Laryng.* 57
- Jewesbury**, R. C.—Case for diagnosis, *Child.* 54
of rickets treated by light therapy, *Child.* 25
of encephalitis lethargica, showing late results, *Child.* 29
pneumococcal septicæmia and enlargement of liver and spleen, case, *Child.* 26
two cases of duodenal obstruction in infants, *Child.* 10-12
and Page, C. Max, D.S.O.—Two cases of duodenal obstruction in infants treated by operation, *Child.* 50
- Jewish** patients, rarity of phlyctenular affections among, *Epid.* 57
- Joly**, J. Swift—Discussion on subcapsular pyelotomy for treatment of renal calculi, *Surg.* 40

- Joly, J. Swift.**—Discussion on operative treatment of vesical diverticula, *Urol.* 55-69
two cases of glandular epispadias, *Urol.* 39-41
- Jones, H. Buckland.**—Laryngeal case for diagnosis, *Laryng.* 51
- Jones, J. A.**—Cases of: (I) Extensive tuberculosis of nose, pharynx and larynx, treated by local measures and tuberculin, *Laryng.* 93; (II) Naso-pharyngeal sarcoma; surgical removal ten years ago; after treatment with radium; no recurrence, *Laryng.* 93; (III) Case of benign growth on posterior commissure of larynx, *Laryng.* 93
specimen of microscopical section of a hæmangioma removed from left vocal cord of a male, aged 45, by indirect method, *Laryng.* 93
of X-ray photographs illustrating involvement of left recurrent laryngeal nerve by mediastinal new growth producing paralysis of left vocal cord, *Laryng.* 93
of X-ray photographs showing spinal abscess which produced intense stridor by compression of trachea, *Laryng.* 93
- Jones, J. Arnold, O.B.E.**—Clinical observations on lingual tonsil (abstract), *Laryng.* 74
- Jones, Roeyn.**—Discussion on operative treatment of spastic paralysis, *Orth.* 39
- Jones, W. H. S.**—Greek medical etiquette, *Hist.* 11-17
- Joseph, H. M.**—Case of progressive macular changes associated with tremors, *Ophth.* 39
- Jugular** bulb, thrombo-phlebitis of, otitic pterygo-maxillary abscess induced by, case (D. McKenzie), *Otol.* 53
- Juler, F. A.**—Case of cicatrization of retina, *Ophth.* 30
of retinal degeneration, with mental deficiency, *Ophth.* 16
discussion on case of progressive macular changes with tremors, *Ophth.* 42
- Jung and Freud,** views on principle of psycho-analysis contrasted, *Psych.* 20
views of transference held by, *Psych.* 19
- Just, T. H.**—Brain abscess due to otitic infection; right temporo-sphenoidal abscess without clinical signs, *Otol.* 54
section of ependymal glioma growing from floor of fourth ventricle, simulating a cerebellar abscess, in case of bilateral chronic suppurative otitis media, *Otol.* 62
sequestra removed from region of Eustachian tube during radical mastoid operation, *Otol.* 61
- Kala-azar,** Indian, occurrence of *Leishmania* in intestinal tissues in, pathological changes occasioned by their presence and their possible significance in this situation (Lt.-Col. H. M. Perry), *Trop.* 1-8
ætiology of, insect-borne or alimentary theories, *Trop.* 6
course and symptoms, effect on, of pathological changes in small intestine, *Trop.* 3
increase in size of liver and spleen in, *Trop.* 1
- Kala-azar,** pathology of, views quoted, *Trop.* 2, 3
transmission of, relationship of intestinal infection to, discussed, *Trop.* 4, 5
two cases of, microscopical appearances of small intestine in, *Trop.* 2, 3
- Kalning,** discovery of mallein by, *Med.* 49
- Kaposi,** multiple idiopathic pigment sarcoma of (so-called), ? case of (H. MacCormac), *Derm.* 61
case of (A. H. M. Gray), *Derm.* 78
two cases of (J. H. Sequeira), *Derm.* 76
- Keay, C.**—Discussion on dental sepsis, *Odont.* 24
- Kelly, A. Brown.**—Discussion on case of œdema of septum in association with nasal polypi, *Laryng.* 28
- Keloid** after burns, case of (E. G. Graham-Little), *Derm.* 61
- Kelson, W. H.**—Discussion on case of vertigo cured by opening external semicircular canal, *Otol.* 60
and Thornhill, W. H.—Case of ulceration of palate and fauces, *Laryng.* 13
- Kempster, C.**—Discussion on dental sepsis, *Odont.* 23
- Kendall, G. M.**—Case for diagnosis (?) renal dwarfism, *Child.* 20
case of heart disease, *Child.* 69
of patent ductus arteriosus and mitral disease, *Child.* 48
- Keratitis,** nodular and reticular, familial, cases (M. L. Hine), *Ophth.* 43-45
phlyctenular, association of origin with exanthemata, *Epid.* 56
association with unclean conditions of mouth, *Epid.* 56
tubercular, *Ophth.* 6
- Khalil, M.**—Description of *Gastrodiscoides hominis* from Napu mouse deer, *Trop.* 8-14
- Kidd, F.**—Candle removed from bladder of a male, *Urol.* 84
case of large spindle-celled sarcoma arising in mesentery of coil of ileum successfully removed by operation, *Surg.* 86
of primary tumour of liver removed by operation, *Surg.* 61
discussion on incidence of malignant disease in the apparently benign enlargement of prostate, *Urol.* 77
on subcapsular pyelotomy in treatment of renal calculi, *Surg.* 38-40
three cases of enlargement of prostate illustrating difficulty of prognosis, *Urol.* 78, 79
- Kidney,** aberrant vessels of, kinking of ureter by, pyonephrosis due to (R. H. Jocelyn Swan), *Urol.* 41
bidigital exploration of, in treatment of renal calculi, *Surg.* 31-33
blood-vessels of, abnormal ligature of, necrosis following, *Urol.* 34
calculi in, origin and treatment of, subcapsular pyelotomy in relation to (W. S. Handley), *Surg.* 21-37
calculus in, large (R. Ogier Ward), *Urol.* 38
formation in massive (specimen) (J. Mac-alpine), *Urol.* 38
cortex of, extension of calyx calculi through, *Surg.* 38

- Kidney**, cortical substance of, cyst in, first step in formation of calculus, *Surg.* 21
disease of, cardio-vascular changes combined with, fatality of, *Med. & Ophth.* 33
vascular and other changes in retina in, significance of, discussion on, *Med. & Ophth.* 1-36
hydronephrosis of one, spontaneous rupture into peritoneal cavity (P. Turner), *Clin.* 24
"leaky," clinical distinction from toxæmic kidney, *Urol.* 23
uræmic or urinæmic symptoms absent in, *Urol.* 22, 23
lower pole of, multiple cystic formation in (R. H. Jocelyn Swan), *Urol.* 41
necrosis of, following ligature of abnormal renal vessels (W. Girling Ball), *Urol.* 34
normal, shadow of, as seen in radiogram, *Urol.* 8
pelvis of, malignant growth of, with calculi, case (Sir J. Thomson-Walker), *Urol.* 85-87
malignant papilloma of, specimen (J. Macalpine), *Urol.* 37
nearly filled with calculus (specimen), *Surg.* 30
simple papilloma of, specimen (J. Macalpine), *Urol.* 37
right, absent, with deformity of left ureter, case (W. Girling Ball), *Urol.* 35
serous cyst of (K. M. Walker), *Urol.* 45
splitting open, for removal of calculi, risk of, *Surg.* 38
toxæmic, causes of, *Urol.* 21
clinical distinction from "leaky" kidney, *Urol.* 23
definition of, *Urol.* 20
differentiation from chronic parenchymatous nephritis, *Urol.* 21
prognosis of, *Urol.* 21
toxæmia of pregnancy and, *Urol.* 21
transitional-celled growth of, specimen showing (W. Girling Ball), *Urol.* 35
tuberculosis of, relation of, to tuberculous abdominal glands, *Urol.* 4, 5
with compound branching pelvis (specimen), *Surg.* 31
- Killick**, C.—Treatment of conical cornea, (abstract), *Ophth.* 24, 25
- Kilner**, T. P.—Discussion on case of cardiac arrest under anæsthetic, *Anæsth.* 6
- King**, W. W.—Discussion on adenomyomata of female pelvic organs, *Obst.* 92
- Kingsford**, A. B.—Discussion on anæsthetization in Cæsarean section, *Anæsth.* 3
- Kirk**, W. H.—Idiosyncrasies to drug tolerance of animals as compared with man, *Therap.* 43-47
- Klumpke** type of birth palsy, case of (C. Worster-Drought), *Child.* 73
- Knee**, hæmophilic arthritis of, case (R. C. Elmslie), *Orth.* 27
- Knee-jerk**, fluctuations of affective state as shown by, *Psych.* 4, 8
- Knox**, R.—Cardiac diagnosis, a survey of development of physical methods, *Electr.* 1-30
- Knox**, R.—Discussion on laryngeal case of epithelioma (possibly syphilis) completely healed and arrested under X-rays, without operation, *Laryng.* 61
work on radiography of gall-bladder, influence on research work, *Electr.* 75
- Kobelt's** cyst, in case of torsion of pedicle of ovarian tumour, appearance of, *Obst.* 107
- Kohler's** disease, two cases of (Paul B. Roth), *Orth.* 28
- Korsakow**, polyneuritic psychosis (quoted), *Neur.* 16
- Kraepelin**, age incidence of commencement of symptoms of hysteria, *Psych.* 9
- Kymogram**, method of taking, *Electr.* 21
- Laboratory methods**, changes in medicine due to, *Med.* 46
- Labour**, pituitary extract in, investigation of, committee for, *Obst. & Therap.* 6, 7
- Labyrinthitis** as a complication of middle-ear suppuration (abstract) (A. Logan Turner and J. S. Fraser), *Otol.* 15
- Lacrymal sac**, tuberculous disease of, *Ophth.* 3
- Lacrymation**, obstinate, due to dental sepsis, *Odont.* 29
- Lactic fermentation and its bearings on pathology** (Lord Lister quoted), *Epid.* 67
- Laënnec**, introduction of stethoscope by, *Electr.* 2
reference to, in *Lancet* (1827), *Hist.* 21
Thomas Davies' account of, *Hist.* 21
- Lævulose** test, Straus's, in case of enlarged liver with persistent acetonuria and diaceturia, *Child.* 58
- Lake**, R.—Discussion on cases of otosclerosis with unusual symptoms, *Otol.* 10
- Lancet*, *The*, references to Laënnec in (1827), *Hist.* 21
- Landry's** paralysis, differential diagnosis from polyneuritis, *Neur.* 26
- Langerhans**, islands of, extract from, *Therap.* 17. See also *Insulin*
- Langmead**, F.—Case of calcinosis (abstract), *Clin.* 23
- Laryngeal** case, apparently epithelioma (possibly syphilis), completely healed and arrested under X-ray treatment without operation (Sir StClair Thomson), *Laryng.* 60
cases for diagnosis (H. B. Jones), *Laryng.* 51;
(H. Smurthwaite), *Laryng.* 31
growth, case (Sir W. Milligan), *Laryng.* 89
cystic, case (A. Wylie), *Laryng.* 44
paralysis, incidence of, in thyroid diseases, *Laryng.* 81
symptoms in two cases of pulmonary tuberculosis, (P. Franklin), *Laryng.* 25
- Laryngectomy**, carcinomatous larynx removed by (C. A. S. Ridout), *Laryng.* 9
complete, for malignant disease (W. Howarth), *Laryng.* 49
following laryngo-fissure, case (C. A. S. Ridout), *Laryng.* 8
- Laryngismus** stridulus, infantile form of asthma, *Therap.* 5
- Laryngitis**, chronic, of long standing, case (C. A. S. Ridout), *Laryng.* 23

- Laryngitis**, syphilitic, results following accidental swallowing of sulphuric acid in patient with, case showing (C. Gill-Carey), *Laryng.* 67
- Laryngo-fissure** for intrinsic cancer of larynx, two cases of (Sir StClair Thomson), *Laryng.* 59
- in case of epithelioma of right vocal cord (Sir W. Milligan), *Laryng.* 89
- laryngectomy following, case (C. A. S. Ridout), *Laryng.* 8
- Laryngology**, development of (C. A. Parker), *Laryng.* 1, 2
- diseases of thyroid gland in relation to (F. Holt Diggle), (abstract), *Laryng.* 81
- Fifth Annual Summer Congress of, Manchester, 1923, *Laryng.* 78-96
- Laryngostomy** for complete subglottic stenosis, case (W. Howarth), *Laryng.* 48
- Larynx** and lung, tuberculosis of, healed, case (Sir StClair Thomson), *Laryng.* 64
- cancer of, intrinsic, laryngo-fissure for, two cases of (Sir StClair Thomson), *Laryng.* 59
- carcinomatous, removed by laryngectomy, specimen (C. A. S. Ridout), *Laryng.* 9
- examination of, by direct method with aid of tongue depressor only, case (G. W. Dawson), *Laryng.* 20
- interarytæoid space of, tumours of (W. Jobson Horne), *Laryng.* 58
- lipoma of, removed by operation (A. J. M. Wright), *Laryng.* 11
- discussion on, *Laryng.* 11, 12
- orifice of, large cyst of, arising from aryteno-epiglottidean fold, post-mortem specimen of (E. D. D. Davis), *Laryng.* 54, 70
- papillomata of, multiple (H. J. Banks-Davis), *Laryng.* 45
- pharynx and palate, extensive lupus of, case (W. Howarth), *Laryng.* 50
- shrapnel-wound of, case and discussion (J. Atkinson), *Laryng.* 15
- stenosis of, caused by bilateral abductor paralysis, operative procedures in treatment, with special reference to new method by which the airway may be permanently enlarged, and patient decannulated (Irwin Moore), *Laryng.* 32-38
- bulbar lesions most common cause of, *Laryng.* 32
- treatment by arytenoidectomy, *Laryng.* 33
- by cordectomy, *Laryng.* 33
- by cordopexy, *Laryng.* 35
- by division of recurrent nerve, *Laryng.* 33
- by evisceration or ablation of vocal cord and soft parts lining larynx, *Laryng.* 35
- by re-establishment of nerve continuity by resection and anastomosis, *Laryng.* 33.
- by ventriculectomy, *Laryng.* 33
- by ventriculo-cordectomy, *Laryng.* 35
- tuberculosis of, case (J. A. K. Renshaw), *Laryng.* 98
- case of, with demonstration of instrument for sunlight treatment (Sir J. Dundas-Grant), *Laryng.* 12
- cured seven years ago by silence and galvanocautery (Sir StClair Thomson), *Laryng.* 64
- Larynx**, tumour of, ? malignant (W. H. Jewell), *Laryng.* 57
- ventricle of, outgrowth from, in subject of pulmonary tuberculosis (Sir J. Dundas-Grant), *Laryng.* 55
- webbing of, congenital, case (G. W. Dawson), *Laryng.* 20
- Laségue's dictum** on hysteria, *Psych.* 2
- Lateral sinus thrombosis**, tuberculosis and extra-cerebellar abscess, temporal bone from case of (E. D. D. Davis), *Otol.* 5
- Laughing and crying**, pathological (S. A. K. Wilson), *Psych.* 39
- organic affections apt to be associated with, *Psych.* 39
- Lawford, J. B. and Neame, H.**—Bilateral tuberculosis of choroid with detachment of retina, in the kitten, *Ophth.* 31
- Lawrence, T. W. P.**—Microscopical report on kidneys in case of parenchymatous nephritis, *Med. & Ophth.* 25
- Layton, T. B., D.S.O.**—Discussion on after treatment of empyema of maxillary antrum, *Laryng.* 85
- discussion on morbid anatomy and drainage of otitic meningitis, *Otol.* 45
- submucous lipoma in glosso-epiglottic furrows, *Laryng.* 11
- "The Disease of not Listening, the Malady of not Marking" (abstract), *Otol.* 12
- Lead-miners** in Wales, mortality among (1906-1918), *Epid.* 95
- mortality among, from various causes, *Epid.* 94
- Lead poisoning**, sciatica in woman suffering from (B. Myers), *Clin.* 7
- Leather collar**, moulded, application of, in treatment of fractured cervical spine of, *Orth.* 2, 3
- Ledingham, J. C. G., C.M.G.**—Natural resistance and the study of normal defence mechanisms, *Path.* 1-8
- Leiomyosarcoma** of a fibromyoma removed by subtotal hysterectomy, case (E. Holland), *Obst.* 64
- secondary, mass of, following subtotal hysterectomy (A. C. Palmer), *Obst.* 62
- following subtotal hysterectomy, histology of specimen, *Obst.* 63
- Leishmania** bodies, distribution in small intestine, in two cases of Indian kala-azar, *Trop.* 2, 3
- occurrence of, in intestinal tissues in Indian kala-azar, the pathological changes occasioned by their presence, and their possible significance in this situation (Lieut.-Col. H. M. Perry), *Trop.* 1-8
- Leishmaniasis** of skin, case (J. B. Christopher-son), *Derm.* 8
- resembling lupus vulgaris (J. B. Christopher-son), *Derm.* 8, 48
- Lens**, cataractous, total absorption of, case (T. Harrison Butler), *Ophth.* 22
- dislocated, see *Ectopia lentis*
- Leptospiræ** isolated from cases of sand-fly fever in Malta, *War* 9, 10
- Leucocyte count** in influenza (typical case), *War* 7
- trench fever and sand-fly fever, graph comparing, *War* 8
- in sand-fly fever (typical case), *War* 7

- Leucocytes**, extracts of, differences in action on bacterial types, *Path.* 5
proteolytic bodies in, *Path.* 6
- Leucocytosis** in dental sepsis, *Odont.* 13
in sand-fly fever in Malta, *War* 5, 6
- Leucocytozoon syphilidis*, action of arseno-benzene on, indirect, *Derm.* 66
- Leucopenia** in sand-fly fever in Malta, *War* 5, 6
other diseases showing, during febrile periods, *War* 6-8
in septic anæmia, *Odont.* 20
in ulcerative colitis, usually present, *Proct.* 103, 104
- Leukæmia cutis** (?), case for diagnosis (W. K. Sibley), *Derm.* 12
discussion on, *Derm.* 14
lymphatic, hip-joint from case of, specimen (C. B. Dansie), *Child.* 43
- Leukins**, constitution of, uncertain, *Path.* 5
properties of, *Path.* 5
- Levy, A. G.**—Action of chloroform on heart, *Anæsth.* 30, 31
- Levy, A. H.**—Case of amaurotic family idiocy, *Ophthalm.* 17
discussion on cases of endothelioma of orbit, *Ophthalm.* 36
- Leyton, O.**—Case of heart-block, *Clin.* 35
- Lichen planus** and syphilis, case of (S. E. Dore), *Derm.* 18
annulatus, with atrophy and herald patch, case (G. Pernet), *Derm.* 2
atrophicus, case of (E. G. Graham Little), *Derm.* 86
cases of, improving under extraction of teeth, *Odont.* 28
in woman aged 40, case (S. E. Dore), *Derm.* 20
lupus erythematosus associated with, case (G. Pernet), *Derm.* 27
spinulosus, case (J. E. M. Wigley), *Derm.* 108
- Light baths** in tuberculous disease of skin, benefit of, two cases illustrating (J. H. Sequeira), *Derm.* 63
effect of, on response of frogs to light (E. Boock and J. W. Trevaun), *Therap.* 8
therapy in case of rickets (R. C. Jewesbury), *Child.* 25, 26
- Limbs**, upper, and feet, congenital deformity of (R. C. Elmslie), *Orth.* 13
- Lingual tonsil**, clinical observations on (abstract) (J. Arnold-Jones), *Laryng.* 74
- Lipoma** (?), double tumour in perineal region of infant (B. Myers), *Child.* 16
of larynx removed by operation (A. J. M. Wright), *Laryng.* 11
submucous, in glosso-epiglottic furrows (T. B. Layton), *Laryng.* 11
- Lister, Lord.**—On the nature of fermentation (quoted), *Epid.* 67
lactic fermentation and its bearings on pathology (quoted), *Epid.* 67
- Lister, Sir W.**—Discussion on case of retinitis circinata, *Ophthalm.* 12
discussion on embryology of congenital crescents, *Ophthalm.* 46
- Little, E. G. Graham.**—Case for diagnosis, (?) diphtheria of skin, *Derm.* 86
case for diagnosis, (?) papulonecrotic tuberculides, *Derm.* 103
- Little, E. G. Graham.**—Case of acne agminata, *Derm.* 15
of folliculitis decalvans, *Derm.* 50
of folliculitis ulerythematosus reticulata, *Derm.* 81
of keloid after burns, *Derm.* 61
of lichen planus atrophicus, *Derm.* 86
(1) of lupus erythematosus; (2) for diagnosis; (3) of extensive urticaria pigmentosa nodularis in an infant, *Derm.* 101
of mycosis fungoides, *Derm.* 82
of onychatrophia, *Derm.* 59
of parapsoriasis, *Derm.* 11
of very extensive sclerodermia, *Derm.* 64
discussion on case of Boeck's sarcoid, *Derm.* 74
of bullous eruption, *Derm.* 55
of congenital onychogryphosis, *Derm.* 92
on cases of multiple superficial rodent ulcer, *Derm.* 25
of parakeratosis variegata, *Derm.* 105
of sclerodermia, *Derm.* 29
of urticaria pigmentosa, *Derm.* 73
of xanthoma (? diabetorum), *Derm.* 94
on dental sepsis, *Odont.* 27
on leishmaniasis of skin, *Derm.* 10
section of excised pigmented mole showing early malignancy, *Derm.* 59
two cases of favus of smooth skin, *Derm.* 51
urticaria pigmentosa in an adult, *Derm.* 72
- Little, E. Muirhead.**—Discussion on operative treatment of spastic paralysis, *Orth.* 37
- Liver**, adenoma of, weighing 2 lb. 3 oz. removed from case, with remarks on partial hepatectomy (G. G. Turner), *Surg.* 43-56
adenoma of, pathological features, *Surg.* 46
rupturing spontaneously, and causing internal hæmorrhage, excision of, case (F. Turner), *Surg.* 60
and gall-bladder, diseases of, mortality from, 1901-1920, statistics, *Surg.* 8
mortality in 1880 and 1920 compared, *Med.* 47
and spleen, enlargement of, case (R. C. Jewesbury), *Child.* 26
absent in rare case of congenital non-familial jaundice, in otherwise healthy man, aged 56 (F. Parkes Weber), *Med.* 81-83
- carcinoma** of, adrenal "rest" in, case (C. A. R. Nitch), *Surg.* 64
pathological report on specimen from case of, *Surg.* 57
primary, excised by operation, case (G. Wright), *Surg.* 56
- cirrhosis** of, mortality from, 1901-1920, statistics, *Surg.* 8
- complications** of, following dental sepsis, *Odont.* 14
cysts of, hepatectomy for, *Surg.* 63
- diseases** of, mortality from, among coal-miners (ages 25-64), period 1890-1912, *Epid.* 88
- efficiency**, tests of, in case of enlarged liver, with persistent acetonuria and diacetura, *Child.* 58
- enlarged**, with persistent acetonuria and diacetura, case (C. Worster-Drought), *Child.* 56
- enlargement** of, with jaundice accompanying, in congenital family cholæmia, *Med.* 80

- Liver**, granulomata removed from, *Surg.* 51
gummata removed from, *Surg.* 51
increased size of, in Indian kala-azar, *Trop.* 1
resection of, for malignant disease spreading from gall-bladder, case (C. Frankau), *Surg.* 59
hæmorrhage in, methods of controlling, *Surg.* 52, 53
indications for, *Surg.* 51
interposition of omentum in, *Surg.* 59
suture after, method of, *Surg.* 54
difficulty of, *Surg.* 53
technique of, *Surg.* 55
see also *Hepatectomy*, partial
tumour of, primary, removed by operation, case (F. Kidd), *Surg.* 61
removal by partial hepatectomy, recovery, *Surg.* 48
- Liverpool**, deaths from scarlet fever and rainfall in, relationship between (1853-1876), *Epid.* 32, 33
- Lockhart-Mummery**, J. P.—Case of early tabes dorsalis, *Proct.* 90
discussion on gonorrhœal stricture of rectum, *Proct.* 20
on ulcerative colitis, *Proct.* 97
new method of treating ischio-rectal and other abscesses, *Proct.* 65
technique of resection and anastomosis of colon for tumour, *Surg.* 69-81
- Lockyer**, Cuthbert.—Discussion on case of carcinoma of prolapsed cervix in woman aged 77, *Obst.* 109
- Logan**, Miss D. C.—Discussion on anæsthetization for Cæsarean section, *Anæsth.* 4
- London**, cases of scarlet fever and amount of rainfall in, relationship between (1840-1922), *Epid.* 31, 32
Lock Hospital, cases treated with manganese butyrate at, *Derm.* 68, 69
scarlet fever in, number of cases and mean age, 1887-1920, *Epid.* 20, 21
- Longstaff**, T. G.—Experiences with the Everest expedition (abstract), *Med.* 57
- Loosely**, C. J.—Discussion on anæsthesia in dental surgery, *Anæsth.* 22
- Lorenz and Hass**.—Operations for relief of pain in congenital dislocation of hips (quoted), *Orth.* 22
- Lothian**, N. V., Major, M.C.—Discussion on physical training, *War* 39
- Lovén** reflexes, *Baln.* 12
- Lowry**, E.—Case of acquired atresia of auditory meatus, *Otol.* 20
- Lowry**, T. M., F.R.S.—Pasteur as chemist, *Occ. Lect.* 16-20
- Ludloff's** sign in case of traction fracture of small trochanter, *Orth.* 12
- Luker**, S. Gordon.—Chorion-epithelioma of uterus showing very extensive growth in uterine wall, *Obst.* 67
discussion on radium treatment before operation for cancer of cervix, *Obst.* 34
- Lumbar** puncture, dangers of, *Otol.* 44, 45, 46
- Lung** and larynx, tuberculosis of, healed, case (Sir StClair Thomson), *Laryng.* 64
- Lung**, collapsed, adhesions of, X-ray appearances, difficulty of, interpretation, *Electr.* 51
flukes, of genus *Paragonimus* (G. M. Vevers), *Trop.* 43, 44
honeycomb, congenital, specimen (C. B. Dansie), *Child.* 43
- Lungs** and mediastinum, sarcoma of, instantaneous radiogram showing, *Electr.* 16
skiagrams of, difficulty of interpretation, *Electr.* 35
- Lupus** erythematosus associated with lichen planus, case (G. Pernet), *Derm.* 27
disseminated, associated with Raynaud symptoms and early sclerodactylia, case of (G. Pernet), *Derm.* 91
(?) in case of atrophic dermatitis of hands and feet (H. W. Barber), *Derm.* 99
extensive, of palate, pharynx and larynx, case (W. Howarth), *Laryng.* 50
treatment of, by light baths (J. H. Sequeira), *Derm.* 63
by salicylic ointment, *Derm.* 109
vulgaris, Leishmaniasis of skin resembling (J. B. Christopherson), *Derm.* 8, 48
treated by liquid acid nitrate of mercury (H. G. Adamson), *Derm.* 80
two cases, with treatment (H. MacCormac), *Derm.* 83
- Lymphangioma** circumscriptum of tongue, case (G. Petit), with discussion, *Derm.* 58
- Lymphatic** glands, abdominal, calcified, relation of, to urinary surgery (Sir J. Thomson-Walker), *Urol.* 1-17
anatomical and pathological aspects, *Urol.* 2
cases, *Urol.* 5-7
clinical symptoms and diagnosis, *Urol.* 5
diagnosis by X-rays, *Urol.* 8
distribution of, *Urol.* 2, 4
on 4th and 5th lumbar vertebræ, X-ray appearances of, *Urol.* 11
radiogram shadows, grouping of, *Urol.* 10
references, *Urol.* 17
size and shape, *Urol.* 9
treatment, with after results, *Urol.* 16, 17
when to operate on, *Urol.* 17
with pyelography, X-ray appearances of, *Urol.* 12, 13
X-ray appearances, *Urol.* 3
latent tubercular infection in, and ocular tuberculosis, *Ophth.* 5
not generally enlarged in yaws, *Trop.* 31
tuberculous, relation of, to renal tuberculosis, *Urol.* 4, 5
vessels, abdominal, in relation to groups of glands, *Urol.* 2, 4
- Lymphoblastic** erythrodermia, case of (W. Dyson), *Derm.* 21
- McAlpine**, D.—Case of quadriplegia with traumatic spondylitis, *Neur.* 33
case of unusual sequelæ of lethargic encephalitis (Parkinsonian syndrome associated with right hemiplegia, showing peculiar disturbances of tone and posture in limbs on hemiplegic side), *Neur.* 27

- McAlpine, D.**—Discussion on late effects of encephalitis lethargica, *Child.* 39, 40
- Macalpine, J.**—Malignant papilloma of renal pelvis, *Urol.* 37
massive calculus formation in kidney, *Urol.* 38
simple papilloma of renal pelvis, specimen, *Urol.* 37
- MacCallan, A. F.**—Seasonal influence in relation to prevalence of surface diseases of eyes in Egypt (quoted), *Epid.* 60
- McCardie, W. J.**—Discussion on anaesthetization in Caesarean section, *Anæsth.* 3
general anaesthesia in dental surgery, *Anæsth.* 11-20
- McClure, J. C.**—Discussion on blood pressure, *Baln.* 1
discussion on vagus and sympathetic nerves and their relation to climate and hydrology, *Baln.* 17
- MacCormac, H., C.B.E.**—Case for diagnosis, *Derm.* 84
case of mycosis fungoides, *Derm.* 106
of dermatitis artefacta, *Derm.* 106
discussion on case of parakeratosis variegata, *Derm.* 105
on treatment of neuro-syphilis, *Neur.* 64, 77
? idiopathic hæmorrhagic sarcoma of Kaposi, *Derm.* 61
two cases for diagnosis, *Derm.* 62
two cases of chronic erythema of legs, *Derm.* 11
two cases of lupus vulgaris, with treatment, *Derm.* 83
- McDonagh, J. E. R.**—Case of recurring erysipelas, *Derm.* 85
discussion on case for diagnosis, *Derm.* 57
on case of Leishmaniasis of skin resembling lupus vulgaris, *Derm.* 49
manganese as chemotherapeutic agent (abstract), *Derm.* 66, 67
- McDougall, W.**—Definition of instinct (quoted), *Psych.* 23
definition of suggestibility (quoted), *Psych.* 7
views on basis of behaviour (quoted), *Psych.* 23
- McKail, D.**—Discussion on mortality of coal- and metalliferous miners, *Epid.* 100
- McKechnie, Col.**—Discussion on dental sepsis, *Odont.* 26
- McKenzie, Dan.**—Clonic spasm of palate, *Laryng.* 57
discussion on case of vertigo cured by opening external semicircular canal, *Otol.* 60
epileptiform seizures subsequent to operation for temporo-sphenoidal abscess, *Otol.* 52
otitic pterygo-maxillary abscess induced by thrombo-phlebitis of jugular bulb, *Otol.* 53
submaxillary gland containing large salivary calculus, *Laryng.* 7
suppurating dental cyst, drained, subsequently obliterated by blood-clot method, *Laryng.* 2
timeous treatment of "broken nose," *Laryng.* 3
- and Dundas-Grant, Sir J.**—Case of sarcoma of tonsillar region treated by X-rays after partial removal, *Laryng.* 69
- and Sydenham, F.**—Epidemic cerebro-spinal meningitis, association with acute sup-puration of middle ear, case, *Otol.* 51
- Mackenzie Davidson Memorial Lecture:** The Pathological Gall-Bladder, (A. W. George), *Electr.* 75-90
- McLean, H.**—Significance of cardio-vascular changes and their effect on prognosis, (quoted), *Med. & Ophth.* 32
- MacLeod, J. M. H.**—Case of urticaria pigmentosa, *Derm.* 78
discussion on multiple superficial rodent ulcer, *Derm.* 27
- MacMahon, C.**—Voice training after laryngo-fissure for intrinsic cancer of larynx, *Laryng.* 62
- McMullen, W. H.**—Discussion on optic atrophy, after herpes ophthalmicus, *Ophth.* 30
- MacNalty, A. S.**—Discussion on late effects of encephalitis lethargica, *Child.* 38, 40
- Macpherson, Sir W. G., Major-Gen.**—Discussion on venereal disease as a war casualty, *War* 27, 44
- Macula, atrophic patches at; ? tuberculosis; ? cyst** (F. A. Williamson-Noble), *Ophth.* 32
degeneration of, dental sepsis in relation to, *Odont.* 27
in case of cerebro-macular degeneration, illustration, *Ophth.* 40, 41
primary disease of, oedema, early stage of, *Ophth.* 39
progressive changes in, associated with tremors, case (H. M. Joseph), *Ophth.* 39
illustration, *Ophth.* 40
- Maculo-cerebral degeneration, early, familial,** two cases (M. L. Hine), *Ophth.* 18, 19
- Magill, I. W.**—Discussion on anaesthesia in dental surgery, *Anæsth.* 22
- Malakoplakia, case of** (Sir J. Thomson-Walker and F. J. F. Barrington), *Urol.* 32
histological examination, *Urol.* 33
- Malar, frontal bones and maxilla, sarcoma of, case** (F. H. Westmacott), *Laryng.* 92
- Malignant disease causing polyneuritis, Neur.** 19
in apparently benign enlargement of prostate, incidence of (R. H. Jocelyn Swan), *Urol.* 71-77
in upper jaw, treatment of, certain pathological and surgical points in (E. M. Woodman), *Laryng.* 87
growth of renal pelvis, with calculi, case (Sir J. Thomson-Walker), *Urol.* 85-87
"pustule," *Med.* 52
- Malingierers, simulation of traumatic spondylitis by, Orth. 4**
- Mallein in eradication of glanders, Med. 49**
- Malta, sand-fly fever in** (Squadron-Leader H. E. Whittingham), *War* 1-14
commoner continued fevers occurring in, *War* 2
- Man and animals, eradication of glanders and anthrax in** (Sir J. Moore), *Med.* 49-56
idiosyncrasies of drug tolerance in animals compared with that in (W. H. Kirk), *Therap.* 43-47
- Manchester, scarlet fever in, cases notified and age tables of, 1891-1921, Epid. 27, 28**
- Mandel, L.**—Case of Tay-Sachs disease, *Child.* 55
- Manganese as chemotherapeutic agent** (J. E. R. McDonagh), *Derm.* 66-67
butyrate, cases treated by, at London Lock Hospital, *Derm.* 68, 69

- Manganese** butyrate, treatment by, 'diseases in which most useful, *Derm.* 67
sepsis and, *Derm.* 69
- Mange** in human being infected by dog, acarus from case of, *Derm.* 75
- Mania** in uræmia, significance and causation of, *Urol.* 24
mental symptoms of, *Psych.* 28
- Mann, Ida C.**—Suggestions on the embryology of congenital crescents (abstract), *Ophth.* 45
- Manson, Sir P.**—Foundation of London School of Tropical Medicine outcome of lecture by *Epid.* 38
Presidential address to Epidemiological Society (quoted), *Epid.* 38, 39
work on filariasis, influence of, *Epid.* 39
- Manson-Bahr, P., D.S.O.**—Case of ulcerating granuloma of pudenda in which healing commenced immediately subsequent to administration of antimony, *Clin.* 25
views on hosts of *Gastrodiscoides hominis* (quoted), *Trop.* 12
on yaws, quoted, *Trop.* 38
- Marchand**, natural defence mechanisms of the organism, quoted, *Path.* 3
- Marconi** otophone, new instrument for assisting the deaf, *Otol.* 51
- Marrack, J. R.**—Discussion on factors in uræmia, *Urol.* 27
- Marriage, H. J.**—Case of acute mastoid disease operated on under artificial respiration, *Otol.* 58
- Martel's** method of anastomosis of colon, *Surg.* 77, 78
- Martin, J. P.**—Discussion on case for diagnosis *Neur.* 51
and **Greenfield, J. G.**—Tumour of cisterna magna, *Neur.* 32-35
- Martindale, L.**—Discussion on cases of foreign bodies in peritoneal cavity, *Obst.* 44
- Masons**, (sandstone and limestone), mortality among, from various causes, *Epid.* 94
- Mastoid** operations, followed by parotid fistula (N. Patterson), *Otol.* 19
radical, sequestra removed from region of Eustachian tube during (T. H. Just), *Otol.* 61
wound, old, parotid fistula in (H. J. Banks-Davis), *Otol.* 30
- Maternal** mortality in eclampsia, statistics, *Obst.* 5
- Maxilla**, dental cysts in, treatment of, *Odont.* 41, 42
malar and frontal bones, sarcoma of, case (F. H. Westmacott), *Laryng.* 92
- Maxillary** antrum, empyema of (Denker's operation), after-treatment of (D. L. Sewell), (abstract), *Laryng.* 85
left, and left frontal sinus, suppurative disease of, case (Sir W. Milligan and D. L. Sewell), *Laryng.* 90
sinus in children, anatomy of, *Child.* 82
- Mayou, M. S.**—Case of subhyaloid hæmorrhage in a girl, *Ophth.* 31
discussion on cases of endothelioma of orbit, *Ophth.* 36
on cases of tumours of optic nerve, *Ophth.* 35
pathological slides showing various changes in retinal vessels, *Med. & Ophth.* 27
- Meakins, J. C.**—Discussion on medical aspects of life at high altitudes, *Med.* 60
- Measles**, blindness due to, *Epid.* 61
mortality in 1880 and 1920 compared, *Med.* 47
relation to onset of blepharitis, *Epid.* 56
scarlet fever and diphtheria, triple infection, necrosis of left temporal bone involving facial nerve and labyrinth in case of, *Otol.* 29
- Meatus**, auditory, and tympanic membrane, laceration of, produced by knitting-needle (H. J. Banks-Davis), *Otol.* 30
- Median** nerve, exploration of, in treatment of case of ischæmic paralysis, *Orth.* 11
- Medical** etiquette, Greek (W. H. S. Jones), *Hist.* 11-17
services of Henry V's campaign of the Somme in 1415 (G. E. Gask), *Hist.* 1-10
students, education in diagnosis of cancer, *Occ. Lect.* 34, 36
- Medicine**, changes in, and its methods, in past 45 years (G. N. Pitt), *Med.* 43-48
due to apparatus for clinical investigation, *Med.* 46
due to bacteriology, *Med.* 43
due to experimental medicine, *Med.* 46
due to facilities for learning, *Med.* 47
due to increased attention to physical signs, *Med.* 46
due to laboratory methods, *Med.* 46
Pasteur in relation to (Pasteur Celebration), (Sir W. Hale-White), *Occ. Lect.* 11-16
- Melancholia** in pernicious anæmia, *Med.* 21
mental symptoms of, *Psych.* 29
- Mellanby, May**—Effect of diet on resistance of teeth to caries, *Odont.* 74-82
influence of diet on teeth formation (quoted), *Odont.* 4
- Melville, C. H., Col.**—Discussion on physical training, *War* 39
- Melville, S.**—Pulmonary tuberculosis as shown by X-rays but without physical signs, *Electr.* 31-35
- "**Membrana eboris**," *Odont.* 59
- Membrane** theory (Bernstein's) of nervous impulse, *Neur.* 55, 56
- Mendel's** law, application to inheritance of specific magglutinable substance of red human blood-cells, *Path.* 45
- "**Ménière's** disease" vertigo simulating, with anomalous nystagmus reactions, case (Sir J. Dundas Grant), *Otol.* 20
symptoms, relations of pyorrhœa to, *Odont.* 40
- Meningitis**, cerebro-spinal, epidemic, associated with acute suppuration of middle ear, case (F. Sydenham and D. McKenzie), *Otol.* 51
otitic, morbid anatomy and drainage of (E. D. Davis), *Otol.* 43
- Mennell, Z.**—Discussion on Coroners' Inquests, *Anæsth.* 39
- Menopause** (radium), cause of, considered, *Obst.* 72, 73
- Menorrhagia**, after myomectomy, *Obst.* 15
- Menstruation**, effects of myomectomy for uterine fibroids on, *Obst.* 15
in cases of dysmenorrhœa, features of, *Obst.* 110
- Mental** and cerebral features in case of pernicious anæmia, *Med.* 31

- Mental** and nervous disorders in severe anæmias in relation to their infective lesions and blood changes (W. Hunter), *Med.* 1-42
and physical efficiency, effect of tropical climate on (T. S. Rippon), *War* 46-54
bibliography and references, *War* 54
fatigue factor, *War* 52
physiogenic or psychogenic origin, *War* 53
psychoneurosis, *War* 52
self-criticism, and race-hostility, as factors in, *War* 52
breakdown in geniuses, *Psych.* 35
changes, acute, in uræmia, significance and causation of, *Urol.* 24
deficiency in case of retinal degeneration (F. A. Juler), *Ophth.* 16
subsequent, connected with previous history of birth injury, *Child.* 77
disease, dental sepsis in relation to, *Odont.* 20, 21
endocrine factor in (J. L. Wilson), *Psych.* 21-30
dullness, due to deficiency of pituitary or thyroid glands, *Psych.* 24
hospital, outbreak of enteric fever in, due to "carriers," *Epid.* 7-9
recreations in the Services as counteracting venereal tendencies, *War* 20
stability associated with genius, instances of, *Psych.* 37
symptoms, morbid states of feeling as causes of, *Psych.* 28
- Mercurial** poisoning, chronic, case of (E. A. Carmichael), *Neur.* 80
- Mercury**, acid nitrate of, liquid, in treatment of lupus vulgaris (H. G. Adamson), *Derm.* 80
- Mesenteric** artery, with ileocolic groups of lymphatic glands (diagram), *Urol.* 2
gland, calcified, X-ray appearances of, *Urol.* 3
glands, tuberculosis of, usually an isolated affection, *Urol.* 4
- Mesentery** of coil of ileum, large spindle-celled sarcoma arising in, successfully removed by operation, case (F. Kidd), *Surg.* 86
- Mesosalpinx**, in case of torsion of pedicle of ovarian tumour, appearance of, *Obst.* 108
- Metabolism**, all forms of calcium not equally available for, *Child.* 3
depressants of, *Child.* 7
inborn errors of, three cases (D. Paterson), *Child.* 27
stimuli of, *Child.* 6
use in treatment of rickets, *Child.* 7
- Metalliferous-** and coal-miners in England and Wales, mortality of (E. L. Collis), *Epid.* 85-99
mining, comparative mortality from certain causes among, *Epid.* 94
- Metchnikoff**, conception of phagocytic action, *Path.* 4
- Metropolitan** Asylums Board, scarlet fever admissions and deaths during 1914, table, *Epid.* 19
- Metrorrhagia** after myomectomy, *Obst.* 15
- Micturition**, time of onset, following removal of suprapubic drain inserted after prostatectomy, *Surg.* 123, 125
re-establishment of, final closure of suprapubic fistula not possible till after, *Surg.* 1
- Middlesex** Hospital, treatment of syphilis at, details of, *Neur.* 65
- Milk**, infection of, by enteric carrier, outbreak due to, *Epid.* 6
- Miller**, H. Crichton—Discussion on endocrine factor in mental disease, *Psych.* 31
discussion on organic basis of hysterical syndrome, *Psych.* 10
on relationship between doctor and patient in psychotherapy, *Psych.* 19
- Miller**, R.—Discussion on case of congenital steatorrhœa, *Child.* 27
two cases of coeliac infantilism in convalescent (non-diarrhœic) stage, *Child.* 22, 23
- Miller**, Prof. W. D.—Bacteria found in dental infections (quoted), *Odont.* 7
- Milligan**, E. T. C.—Discussion on incidence of malignant disease in the apparently benign enlargement of prostate, *Urol.* 79
- Milligan**, Sir W.—Case of (?) papilloma of faucial mucous membrane and enlarged cervical glands, *Laryng.* 96
discussion on after-treatment of empyema of maxillary antrum, *Laryng.* 85
on case of extensive lupus of palate, pharynx and larynx, *Laryng.* 52
on case of laryngostomy for complete subglottic stenosis, *Laryng.* 48
on case of multiple papillomata of larynx, *Laryng.* 45, 46
on case of œdema of septum in association with nasal polypi, *Laryng.* 27
on lingual tonsil, *Laryng.* 76
on treatment of large foreign bodies impacted in gullet, *Laryng.* 79
epithelioma of right vocal cord, laryngo-fissure, case, *Laryng.* 89
of soft palate and left anterior faucial pillar, case, *Laryng.* 88
laryngeal growth, case, *Laryng.* 89
sarcoma of left tonsil, *Laryng.* 88
and **Sewell**, D. L.—Suppurative disease of left frontal sinus and left maxillary antrum, case, *Laryng.* 90
tuberculous growth in left naris, case, *Laryng.* 90
and **Wrigley**, F.—Orbital cellulitis, invasion of frontal sinus, osteo-myelitis of frontal bone, case, *Laryng.* 90
sarcoma of right tonsil and surrounding faucial region, case, *Laryng.* 91
- Minchin**, Prof. E. A.—Speculations with regard to simplest forms of life and their origin on the earth (quoted), *Epid.* 72, 73.
- Mind** and body, endocrine system as intermediate zone between, *Psych.* 31
unconscious, supreme controlling force of human power, *Psych.* 31
- Mines** under Coal Mines Acts, death-rates from different causes of accidents, 1873-1920, *Epid.* 87
mortality and other statistics, 1873-1912, *Epid.* 86
under Metalliferous Mines Act, mortality and other statistics, 1873-1912, *Epid.* 86
- Mining** industry, mortality in, sources of information, *Epid.* 85

- Mitral** disease and patent ductus arteriosus, case of (G. M. Kendall), *Child*. 48
- Miyajima**, Prof. Mikinosuke.—History of vaccination in Japan, *Hist.* 23-26
- Mobilization**, physical training and, *War*. 35
- Molar**, third, upper, carious, earache due to, *Odont.* 40
- Mole**, pigmented, excised, showing early malignancy, section of (E. G. G. Little), *Derm.* 59
- Mollison**, W. M.—Case of multiple foci of growth in palate and tonsil, *Laryng.* 19
 of swelling in nasopharynx on right side, displacing soft palate downwards, *Laryng.* 68
 of vertigo cured by opening the external semicircular canal, *Otol.* 60
 discussion on case of otitis media with facial palsy following scarlet fever, *Otol.* 13
 on cases of otosclerosis with unusual symptom, *Otol.* 11
 injury to nose from lift accident, *Laryng.* 4
 instrument for assisting the deaf (Marconi otophone), *Otol.* 51
- Molluscum contagiosum**, note on (J. J. Clarke), *Derm.* 3-7
 parasite of, identification of, *Derm.* 4-7
- Mongolism**, three cases exhibiting (O. C. M. Davis), *Child*. 80
- Monod**, G.—Pasteur as artist, *Occ. Lect.* 21-27
- Mononuclear** cells, nature of, in glandular fever, *Med.* 71
- Mononucleosis**, infective, glandular fever and (H. L. Tidy), *Med.* 70-72
- Moore**, Irwin.—Discussion on after-treatment of empyema of maxillary sinus, *Laryng.* 86
 discussion on case of foreign body removed from trachea of child aged 6 months, *Laryng.* 67
 on case of outgrowth from ventricle in subject of pulmonary tuberculosis, *Laryng.* 55
 on case of unusual tonsillar appendage, *Laryng.* 16
 on case of ventriculo-chordectomy for double abductor paralysis, *Laryng.* 47
 on specimen of cyst of larynx, *Laryng.* 54
 on specimen of threepenny piece impacted in perforation between œsophagus and trachea in baby aged 3 months, *Laryng.* 56
 operative procedures in treatment of stenosis of larynx caused by bilateral paralysis of abductor muscles, with special reference to new method by means of which it is suggested that the airway may be permanently enlarged, and patient decannulated, *Laryng.* 32-38
 on reduction or destruction of hypertrophied or diseased tonsils by means of caustic soda and slaked lime (London paste), *Laryng.* 96
- Moore**, Sir J., Major-General, K.C.M.G.—Discussion on operative procedures for bilateral abductor paralysis, *Laryng.* 40
 eradication of glanders and anthrax in man and animals, *Med.* 49-56
- Moore**, R. Foster, O.B.E.—Discussion on significance of vascular and other changes in retina in arterio-sclerosis and renal disease, *Med. & Ophth.* 5-15, 35
- Morax-Axenfeld** bacillus, conjunctivitis due to, rarity among infants and school-children, prevalence among adults, *Epid.* 57
- Morison**, J. M. W.—Cardiospasm and other diseases of œsophagus, *Laryng.* 96
 and **White**, L. M.—Radioscopic method for estimating hypertrophy of left ventricle (quoted), *Electr.* 12-19
 heart-measuring apparatus of (diagram), *Electr.* 17
- Moritz**, introduction of apparatus for orthodiagraphy by, *Electr.* 7
- Morphia** and other sedatives, treatment of eclampsia by, results, *Obst.* 9
 effects of, upon animals and man compared, *Therap.* 44, 45
 tolerance of animals to, idiosyncrasies of, *Therap.* 43
- Morphine-scopolamine**, preliminary administration in anesthetization for Cæsarean section, risk to child, *Anæsth.* 3, 4
- Morphœa** associated with vitiligo, case (H. W. Barber), *Derm.* 106
- Morson**, A. C.—Case of ectopia testis, *Urol.* 43-45
- Morsted**, Thomas.—Chief Surgeon in Henry V's campaign of the Somme, 1415, *Hist.* 2
- Mortality** among coal-miners, reduction of, factors influencing, *Epid.* 86, 87
 among general population in coal-mining counties (1901-1910), all causes and phthisis compared, *Epid.* 89
 comparative, among ironstone-miners from certain causes (1910-1912), *Epid.* 97
 among metalliferous miners from certain causes, *Epid.* 94
 from certain causes among coal-miners aged 25-64 inclusive (1890-1912), *Epid.* 88
 from alcoholic diseases in various coalfields compared, *Epid.* 92, 93
 different causes of accidents in mines under Coal Mines Acts (1873-1920), *Epid.* 87
 phthisis per 1,000 living at various age periods in certain occupations, *Epid.* 90
 of coal- and metalliferous-miners in England and Wales (E. L. Collis), *Epid.* 85-99
 proportionate, from cancer (1910-1912) according to parts affected, *Epid.* 93
 tables for various diseases, comparison between years 1880 and 1920, *Med.* 47
- Mortimer**, J. D.—Discussion on Coroners' Inquests, *Anæsth.* 41
- Mott**, Sir F., K.B.E., F.R.S.—Discussion on organic basis of hysterical syndrome, *Psych.* 10
 gonad deficiency in relation to dementia præcox (quoted), *Psych.* 31
- Mottram**, J. C.—Some effects of exposure to radium on blood platelets, *Path.* 9-13
 some effects of exposure to radium upon alimentary canal, *Electr.* 41-44
- Mouth**, kinds of streptococci found in, *Odont.* 17, 18
 pharynx and œsophagus, diseases of, mortality from, 1901-1920, *Surg.* 8
 unclean condition of, association of phlyctenular keratitis with, *Epid.* 56

- Mucous** membranes, involvement in case of dermatitis repens and infectious eczematoid dermatitis (H. W. Barber), *Derm.* 98
- Mud** baths, hyperthermal, relation to sympathetic nerve, *Baln.* 15
- Muhammadan** chemical literature, reference to Jābir ibn Hayyān in, *Hist.* 47
- Mummery**, J. Howard, C.B.E.—Case of multiple dentigerous cysts, *Odont.* 44-47
- Munro**, D., Air-Commodore, C.I.E.—Discussion on physical training, *War* 38
- Muriatic** acid erosion of fingers, case of (W. J. O'Donovan), *Derm.* 87
- Murray**, G. R.—Discussion on present position of organotherapy, *Therap.* 14
- Muscle-sliding** operation, flexion contracture of forearm treated by, four cases of (C. Max Page), *Orth.* 43, 44
- Muscles**, abdominal, rhythmical stimulation by interrupted currents, *Baln.* 14, 15
- congenital anomalies of, in myopathy, *Neur.* 8
- response of, to nocuous stimulus, increase of tonus showing, *Psych.* 4, 5, 7, 8
- Muscular** atrophy, distribution in dystrophia myotonica, *Neur.* 38
- of "peroneal" type apparently commencing in, and for some time confined to, right hand, case (C. P. Symonds), *Neur.* 80
- Tooth-Marie-Charcot type, case (E. C. Williams), *Child.* 79
- dystrophy (?) determined by trauma, case, *Neur.* 5
- weakness experienced by flyers at high altitudes, *Med.* 60
- Musculo-spiral** paralysis in chronic alcoholism, *Neur.* 15
- Mustard** oil, inflammatory effect of, application neutralized by application of cocaine to skin, *Baln.* 12
- inflammatory effect on skin not resulting after degeneration of sensory fibres of posterior spinal nerve-roots, *Baln.* 12, 15
- Myasthenia** gravis, case of, in which throat symptoms were an early sign (C. P. Symonds), *Laryng.* 17
- Mycosis** fungoides, cases (H. E. Claremont), *Clin.* 34; (E. G. Graham Little), *Derm.* 82; (H. MacCormac), *Derm.* 106
- Mydriasis**, Argyll Robertson pupils with, case (F. Parkes Weber), *Child.* 68
- Myeloma** of outer condyle of femur, showing result of bone-grafting, case (A. H. Todd), *Clin.* 3, 4
- Myers**, B., C.M.G.—Absence of both thumbs, with other deformities of upper extremities in infant, *Child.* 72
- case for diagnosis, *Child.* 19
- of adiposis dolorosa, *Clin.* 11
- of aneurysm of arch of aorta and innominate artery in woman, *Clin.* 9
- of persistent jaundice in infant; atresia of common bile-duct and biliary cirrhosis, *Child.* 17
- of purpura hæmorrhagica, *Clin.* 10
- discussion on birth injuries, *Child.* 75
- on case of patent ductus arteriosus and mitral disease, *Child.* 49
- Myers**, B., C.M.G.—Double tumour (?) lipoma in perineal region of infant, *Child.* 16
- Sciatica in woman suffering from lead poisoning, *Clin.* 7
- two cases of osteogenesis imperfecta, *Child.* 69-72
- Myocardium**, toxic action of quinidine on, symptoms due to, *Therap.* 34
- Myomectomy** combined with Cæsarean section in treatment of fibroids complicated by pregnancy, *Obst.* 22
- in treatment of uterine fibroids, indications for and results of (A. E. Giles), *Obst.* 13-21
- during pregnancy, indications for and table of results, *Obst.* 19-21
- effects on menstrual loss, *Obst.* 15
- indications for, *Obst.* 17
- after results of, *Obst.* 14
- imitations of, *Obst.* 19
- mortality statistics compared with those of hysterectomy, *Obst.* 14
- percentage in operations at different ages for married and single women (chart), *Obst.* 18
- proportion of, to hysterectomy, at different ages, table, *Obst.* 20
- recurrence of fibroids, percentage of, *Obst.* 14
- results in relation to pregnancy, *Obst.* 15, 16
- scope and technique of (V. Bonney) (abstract), *Obst.* 22
- Myopathic** origin, probable, of symmetrical atrophic paresis of quadriceps muscles, *Neur.* 1
- Myopathies**, distinction of dystrophia myotonica from, *Neur.* 42
- Myopathy**, observations on (E. Bramwell), *Neur.* 1-12
- case diagnosed as, recovery in (? toxic neuritis), *Neur.* 8, 9
- cases of, question of recovery in, *Neur.* 8
- congenital anomalies of muscles in, *Neur.* 3
- diagnosis of, importance of family history in, *Neur.* 9
- differentiation from myotonia atrophica, *Neur.* 10
- limited to quadriceps muscles, cases illustrating, *Neur.* 1, 2
- trauma in, ætiology of, cases reported, *Neur.* 3, 6, 7
- with facio-scapular distribution attributed to alleged trauma, case, *Neur.* 3, 4
- Myopia**, due to dental sepsis, *Odont.* 29
- heredity in, *Ophth.* 9
- with mixed astigmatism, standard for scholars and teachers at Council schools, *Ophth.* 8
- Mysarcoma** of epididymis, case (Sir J. W. Thomson-Walker), *Urol.* 31
- Myositis** ossificans exhibiting acute symptoms, case (C. Max Page), *Clin.* 32
- Myotonia** atrophica, name unsuitable, *Neur.* 37
- congenital stigmata in, *Neur.* 10
- differentiation from myopathy, *Neur.* 10
- in dystrophia myotonica, *Neur.* 39
- remarks on, and report of case with autopsy, *Neur.* 10-12
- testicular atrophy in, *Neur.* 10, 11
- with implication of left crico-arytænoideus muscle, case (H. Tilley), *Laryng.* 18

- Myotonia** congenita, case of (L. R. Yealland), *Neur.* 45
see also *Dystrophia myotonica*
- Myxœdema**, administration of thyroxin in, in relation to effect on basal metabolic rate, *Therap.* 15
sequel in Graves' disease and nymphomania in cows, *Obst.* 96
- Nabarro, D.**—Cases of congenital syphilis, *Child.* 42
- Nævus**, linear, extensive, in man aged 54, case (S. E. Dore), *Derm.* 105
- Nails**, ringworm of, two cases, in sisters (H. C. Semon), *Derm.* 1
- "Name amnesia"** case of, *Otol.* 56
- Napu** mouse deer, *Gastrodiscoides hominis* from, description of (M. Khalil), *Trop.* 8-14
- Naris**, left, tuberculous growth of, case (Sir W. Milligan and D. L. Sewell), *Laryng.* 90
- Nasal** cavities, application of bismuth and glycerine gauze to (Sir StC. Thomson), *Laryng.* 29
and sinuses, evolution of, in relation to function (J. F. O'Malley) (abstract), *Laryng.* 83, 84
infections, parathyroid medication in, *Therap.* 20
sepsis, underlying septic anæmia, *Med.* 6, 8
sinus, disease, acute, in young children (E. Watson-Williams), *Child.* 81-84
diagnosis, *Child.* 83
frequency, *Child.* 82
references, *Child.* 84
treatment, *Child.* 83, 84
two severe cases of, *Child.* 82
stenosis, mainly subjective, in case of Parkinson's disease (Sir J. Dundas-Grant and C. C. Worster-Drought), *Laryng.* 23
see also under *Nose*
- Nasmyth's membrane**, formation of, *Odont.* 51
- Nasopharyngeal** tumour, (?) cystic adenoma, containing cartilage (wet specimen and section) (A. A. Smalley), *Laryng.* 94
- Nasopharynx** on right side, swelling of, displacing soft palate downwards, case of (W. M. Mollison), *Laryng.* 68
tumour of, case (F. C. Ormerod), *Laryng.* 56
- Nattrass, F. J.**—Discussion on case of muscular atrophy of peroneal type, *Neur.* 81
- Natural** resistance and the study of normal defence mechanisms (J. C. G. Ledingham), *Path.* 1-8
immunity, mechanisms of defence in, *Path.* 3
summary of earlier work on, *Path.* 3, 4
to anthrax, application of defence mechanisms, animal experiments, *Path.* 6, 7
- Navy**, venereal disease in, incidence of, at various periods, *War.* 15
prophylactic measures used in, *War.* 18
- Neame, Humphrey**—Case of retinitis circinata, *Ophth.* 11
discussion on familial nodular and reticular keratitis, *Ophth.* 45
on case of hemangioma of orbit, *Ophth.* 14
tumours of optic nerve, *Ophth.* 34
and **Lawford, J. B.**—Bilateral tuberculosis of choroid with detachment of retina, in a kitten, *Ophth.* 31
- Neck**, osteo-arthritis, traumatic, treated by bone-graft (D. M. Aitken), *Orth.* 30
wrenching of, in treatment of dislocation of fifth cervical vertebra, *Orth.* 2
- Needle**, exploring, introducer of, *Hist.* 19
- Negri** bodies, demonstration of, in rabies, method of staining, *Trop.* 51
- Neisser**, views on yaws (quoted), *Trop.* 32
- Neon** lamp, use of, in apparatus for measuring sensori-motor reaction times, *Electr.* 64
- Nephritis**, acute, uræmia and, *Urol.* 21, 22
chronic, Ambard's constant in, formula for, *Urol.* 81
two cases of, *Urol.* 82
two points in connexion with (C. G. Cumston), *Urol.* 81-84
dental sepsis in relation to, *Odont.* 20
focal embolic, in relation to uræmia, *Urol.* 22
in relation to uræmia, *Urol.* 20
mortality in 1880 and 1920 compared, *Med.* 47
parenchymatous, case described, *Med.* 23-26
chronic, complicating conditions causing uræmia in, *Urol.* 22
- Nephro-lithotomy**, double, bilateral calculous pyonephrosis, ten years after, case (P. Turner), *Clin.* 40
in removal of renal calculi, cases suitable for, *Surg.* 24
objections to, *Surg.* 24
restricted, cases suitable for, *Surg.* 24
unipolar, in removal of renal calculi, *Surg.* 25
- Nephrotomy**, bipolar (Legueu), in removal of renal calculi, *Surg.* 24
- Nerve**, conductivity of, abolition by alcohol, *Neur.* 56
failure in toxic neuritis or polyneuritis, *Neur.* 58
deafness, complete, due to syphilis of internal ears; caloric and rotation tests negative, galvanic positive, case (Sir J. Dundas-Grant), *Otol.* 16
facial, involvement by acusticus tumour, symptoms of, *Otol.* 34
fibre, chronaxie of, *Neur.* 59
excitation of, normal process of, *Neur.* 59
impulse, refractory period of, *Neur.* 56
power of adaptation to stimulus, *Neur.* 59
fifth, involvement by acusticus tumours, symptoms of, *Otol.* 33
sixth, palsy due to pressure effects of acusticus tumour, *Otol.* 34
- Nerves**, "trophic," question of, discussed, *Baln.* 12
- Nervous** and mental disorders of severe anæmias in relation to their infective lesions and blood changes (W. Hunter), *Med.* 1-42
conduction, changes revealed by electrical stimuli, *Neur.* 55
diseases, dental sepsis in relation to, *Odont.* 15
mortality in 1880 and 1920 compared, *Med.* 47
high blood pressure associated with, *Baln.* 2
features in pernicious anæmia, *Med.* 7, 12, 31, 41, 42
interpretation of, *Med.* 39
relation of lesions to, *Med.* 40
impulse, Bernstein's membrane theory of, *Neur.* 55, 56
period of activity of, *Neur.* 56
producing reflex inhibition, *Neur.* 57 (footnote)

- Nervous system**, importance of ductless glands in speeding up, *Psych.* 36
of dental pulp, *Odont.* 63
syphilis of, discussion on, *Neur.* 61-78
see also under *Neuro-syphilis*
systems of afferent function in, *Psych.* 4
tissues, selective effect of toxins on, *Urol.* 20
- Neuralgia** dentalis, *Odont.* 35
nasal and aural, of dental origin, *Odont.* 35
- Neurasthenia**, associated with gastro-intestinal atony, low blood-pressure in, *Baln.* 2
- Neuritis**, asymmetrical, in typhoid and paratyphoid infections, *Neur.* 15
of toxic origin, *Neur.* 15
brachial, following tuberculous pleurisy, *Neur.* 18
hæmatoporphyritic, cases and clinical description, *Neur.* 23-25
multiple, classification on ætiological basis, *Neur.* 13
due to intense chill, *Neur.* 14
due to septicæmia, *Neur.* 16
optic, in polyneuritis, *Neur.* 21
peripheral, multiple (W. Harris), *Neur.* 13-26
(pressure), differentiation from toxic polyneuritis, *Neur.* 20
toxic, failure of nerve conduction in, *Neur.* 58
(?) in case diagnosed as myopathy which recovered, *Neur.* 8, 9
tuberculous, of feet, *Neur.* 18
- Neurofibromatosis**, two cases of (S. E. Dore), *Derm.* 104
- Neurone**, conduction by, disordered, imperfect, due to mechanical injury or pressure, *Neur.* 58
"conduction with a decrement," *Neur.* 57
excess of activity of, *Neur.* 59
failure, partial or complete, to transmit impulses, *Neur.* 57
function, disorders of (E. D. Adrian), *Neur.* 55-60
- Neuroses**, differentiation of hysterical syndrome from other forms of, by galvanic response to stimulus, *Psych.* 6
organic disability preceding, *Psych.* 2
paroxysms of epileptics subject to, *Neur.* 93
- Neurosyphilis**, congenital, in brother and sister, cases (S. A. K. Wilson), *Neur.* 50
alterations in cerebro-spinal fluid in, *Neur.* 67
Fournier's views, (quoted), *Neur.* 67
late stages of, types of lesion and effects of treatment, *Neur.* 67, 68
negative Wassermann reaction in presence of definite clinical signs, *Neur.* 63
special form of spirochæte as cause of, possibility of, considered, *Neur.* 66
treatment of, discussion on, *Neur.* 61-78
antisyphilitic, continuity essential, *Neur.* 62, 63
contra-indications to, *Neur.* 64
by intracisternal injections of salvarsanized serum, *Neur.* 69-72
comparative value of various arsenical compounds in, *Neur.* 76
examination of cytology of cerebro-spinal fluid in, *Neur.* 64
intra-spinal, intravenous and intra-theical, methods considered, *Neur.* 68, 76
results to be expected, *Neur.* 63
- Night blindness**: retinitis pigmentosa sine pigmento (J. A. Valentine), *Ophthalm.* 17
- Nitch**, C. A. R.—Carcinoma of adrenal "rest" in liver, case, *Surg.* 64
- Nitrogen**, rate of excretion by bowel and skin, *Urol.* 19
- Nitrous oxide**, administration in dental surgery, supplemented by ether, *Anæsth.* 15
and oxygen, as anæsthetic in dental surgery, *Anæsth.* 20
as anæsthetic in dental surgery, safety of, *Anæsth.* 12
impure, administration of, causing death, *Anæsth.* 15
- Nixon**, J. A.—Schick reaction and diphtheria anaphylaxis, *Child.* 84
- Nodules**, subcutaneous, generalized scleroderma with, case (A. M. H. Gray), *Derm.* 107
- Norbury**, L. E. C.—Case which was clinically one of inoperable carcinoma of rectum treated by colostomy and subsequent injections of cuprase-colossal selenium and colossal cuprum for over two years, with disappearance of growth, *Proct.* 67
discussion on gonorrhœal stricture of rectum, *Proct.* 20
- Norgate**, R. H. — Three cases of congenital syphilis, *Child.* 80
- Norman**, H. J.—Genius and insanity, *Psych.* 33-37
- Nose** and associated bones, depressed fracture of (H. D. Gillies), *Laryng.* 4
arch of, depressed fracture of, case (H. D. Gillies), *Laryng.* 6
bony bridge of, depressed, case of (H. D. Gillies), *Laryng.* 4
broken, timeous treatment of (Dan Mackenzie), *Laryng.* 3
fibroma of, case (L. Powell), *Laryng.* 66
infections of, in relation to dental buds, *Odont.* 37
injuries to, cases of, discussion on, *Laryng.* 6, 7
from lift accident (W. M. Mollison), *Laryng.* 4
papillomata of, multiple, specimen from case of (H. J. Banks-Davis), *Laryng.* 46
sarcoma of, cured by radium, case (E. M. Woodman), *Laryng.* 49
septum of, floor of both nostrils, alveolar surface upper jaw and left side lower jaw, epithelioma of, case (A. Wylie), *Laryng.* 30
œdema of, in association with nasal polypi, case (A. J. Wright), *Laryng.* 27
throat and ear, infections of teeth and gums in their relationship to, discussion on, *Odont.* 35-42
vestibule of, epithelioma of, after-treatment by radium, case (Sir J. Dundas-Grant), *Laryng.* 65
see also under *Nasal*
- Nostrils**, both, floor of, nasal septum, alveolar surface upper jaw, left side lower jaw, epithelioma of, case (A. Wylie), *Laryng.* 30
- Novarseno-billon**, excessive dosages of, toxic effects, *Neur.* 74-77
- Noxious** stimuli, reaction of thyroid gland to, *Psych.* 25
- Nutrition**, endocrine glands governing, *Psych.* 23

- Nymphomania** in cows and Graves' disease, assumption of male characters in, *Obst.* 98
 breed and heredity in, *Obst.* 95
 cardio-vascular symptoms, *Obst.* 96
 clinical picture of, *Obst.* 94
 comparison of, *Obst.* 95
 exciting causes, *Obst.* 95
 myxœdema as sequel in, *Obst.* 96
 nature of, *Obst.* 92, 93
 nervous symptoms, *Obst.* 96
 nymphomania as symptom, *Obst.* 97
 thyroid enlargement in, *Obst.* 96
- Nystagmus** reactions, anomalous, in case of vertigo simulating Ménière's disease (Sir J. Dundas-Grant), *Otol.* 20
- Obstetrical** and gynæcological practice, value of ergot in, with special reference to its position in British Pharmacopœia (H. H. Dale, with discussion), *Obst. & Therap.* 1-7
- Obstetrics** and Gynecology, Section of, Committee for Investigation of Pituitary Extract in Labour, *Obst. & Therap.* 6
 Report of Committee on Prognosis and Treatment of Eclampsia, *Obst.* 1-11
- Occipito-atlantal** puncture, method of, *Otol.* 47
- Occupational** diseases, differences in morbidity in, *Epid.* 85
- Ocular** torticollis, case of (Paul B. Roth), *Orth.* 46
 tuberculosis, see *Tuberculosis*, ocular
- O'Donovan, W. J.**—Case of carcinoma faciei apud puellam, *Derm.* 87
 case of muriatic acid erosion of fingers, *Derm.* 87
 discussion on cases of multiple superficial rodent ulcer, *Derm.* 26
 on case of parakeratosis variegata, *Derm.* 105
 on outbreak of alopecia, *Derm.* 101
 on treatment of lupus by light baths, *Derm.* 64
 squamous carcinoma of face in woman aged 24, *Derm.* 52
- Odontalgia**, not experienced by domesticated animals, *Odont.* 64
 types of, *Odont.* 70
- "Odontoblasts,"** *Odont.* 59
 term suggested in place of, *Odont.* 59
- Odontome**, compound follicular or compound composite, *Odont.* 55
 radicular, *Odont.* 56
- Odontomes**, two, described (A. Hopewell-Smith), *Odont.* 55-58
- Œdema**, effect on prognosis of eclampsia, *Obst.* 4
 of septum in association with nasal polypi, case (A. J. Wright), *Laryng.* 27
- Œsophagus** and trachea, perforation between, threepenny-piece impacted in, in baby aged 3 months, mounted specimen showing (H. J. Banks-Davis), *Laryng.* 55
 stricture of, congenital, specimen (R. Hutchinson), *Child.* 42
 simple fibrous, in child, skiagrams showing (A. Ryland), *Laryng.* 42
- Ogilvie, W. H.**—Case of renal dwarfism, *Orth.* 51
 specimen of synostosis of phalangeal joints, (?) congenital in origin, *Orth.* 51
- Oliver, M. W. B.**—Plastic operation for contracted sockets, *Ophth.* 15
- Oliver, W. J.**—Case for diagnosis, *Derm.* 47
- O'Malley, J. F.**—Case of necrosis of left temporal bone, involving facial nerve and labyrinth, following triple infection of scarlet fever, measles, and diphtheria, in child aged 7, *Otol.* 29
 discussion on cases of deafness due to falls, *Otol.* 50
 on case of œdema of septum in association with nasal polypi, *Laryng.* 28
 of otosclerosis with unusual symptom, *Otol.* 10
 of sarcoma of nose cured by radium, *Laryng.* 50
 of unusual tonsillar appendage, *Laryng.* 16
 evolution of the nasal cavities and sinuses in relation to function (abstract), *Laryng.* 83, 84
- Omentum**, interposition of, in resection of liver, *Surg.* 59
 protective action of, on foreign bodies left in peritoneal cavity, *Obst.* 89
- Onychatrophia**, case of (E. G. G. Little), *Derm.* 59
- Onychogryphosis**, congenital, case of (J. H. Sequeira), *Derm.* 92
- Opaque** meal examination of stomach, method for (S. G. Scott) (abstract), *Electr.* 35-41
- Openshaw, T. H., C.B., C.M.G.**—Discussion on operative treatment of spastic paralysis, *Orth.* 37
 traumatic spondylitis (President's address), *Orth.* 1-10
- Operations**, surgical, rise and fall of (J. Berry), *Surg.* 1
- Ophthalmia**, endemic, historical data, *Epid.* 49, 50
 neonatorum, blindness due to, *Epid.* 60
- Ophthalmoscope**, distinction between simple high blood-pressure and arterio-sclerotic changes by, *Med. & Ophth.* 16
- Optic** atrophy after herpes ophthalmicus (I. Paton), *Ophth.* 27-30
 cases reported, *Ophth.* 28
 Hutchinson's law in, *Ophth.* 29
 in relation to anti-syphilitic treatment, *Neur.* 64
 nerve, tumours of (H. Neame), *Ophth.* 34
 neuritis accompanying polyneuritis, *Neur.* 21
- Optometer**, estimation of amplitude of accommodation by, *War* 47
- Oral** sepsis in relation to severe anæmias, *Med.* 3, 6, 8; *Odont.* 21
- Orbit**, endothelioma of (F. A. Williamson-Noble), *Ophth.* 35
 hæmangioma of, *Ophth.* 13
 œdema of, chronic, two cases of (F. H. Westmacott), *Laryng.* 91
- Orbital** cellulitis. See under *Cellulitis*
- Organic** basis of hysterical syndrome (F. L. Golla), *Psych.* 1-11
- Organotherapy**, present position of, discussion on, *Therap.* 9-24
 adrenal medication in, *Therap.* 12
 doctrine and practice of, foundation for, *Therap.* 9
 in treatment of dysmenorrhœa, *Obst.* 111, 114
 limitation of use of term suggested, *Therap.* 14
 ovarian medication in, *Therap.* 13, 17

- Organotherapy**, pancreatic medication in, *Therap.* 13
 parathyroid medication in, *Therap.* 12, 16
 physiological effects of many substances used not yet proved, *Therap.* 10
 pituitary medication in, *Therap.* 12, 17
 present position of, summary, *Therap.* 14
 suprarenal gland medication in, *Therap.* 16
 testicular medication in, *Therap.* 13, 17
 thyroid medication in, *Therap.* 11, 16
- Oriental sores.** See *Leishmaniasis* of skin
- Ormerod**, F. C.—Tumour of nasopharynx, *Laryng.* 56
- Orthodiagraphic apparatus**, *Electr.* 12
 tracing, *Electr.* 13
- Orthodiagraphy** in cardiac diagnosis, *Electr.* 7
 in conjunction with fluoroscopy, Gösta Forssell's device for, *Electr.* 8-11
- Orthopædic surgeon**, status of, *Orth.* 1
 surgery, necessity for agreement in treatment of various conditions, *Orth.* 1
 scope of, *Orth.* 1
- Ortlepp**, R. J.—Life history of the gape-worm (abstract), *Trop.* 44
- Osseous** tumours, relationship of clonic hyperplasia of upper jaw to, *Laryng.* 73
- Ossiculectomy**, case of vertigo with fixation of ossicles, cured by (Sir J. Dundas-Grant), *Otol.* 18
- Osteitis** deformans, and otosclerosis; pathological and clinical comparison (abstract), (G. J. Jenkins), *Otol.* 21-26
 clinical aspects contrasted, *Otol.* 22
 conclusions regarding, *Otol.* 25
 microscopical appearances contrasted, *Otol.* 21, 27
 case of (Paul B. Roth.), *Orth.* 49
 deafness associated with, nine cases described, *Otol.* 23
- Osteo-arthritis** of neck, traumatic, treated by bone-graft (D. M. Aitken), *Orth.* 30
 of spine (C. Gouldesbrough), *Med.* 63.70
 frequency of, *Med.* 63
 types of, *Med.* 64
 X-ray pathology of, *Med.* 67
- Osteo-chondritis** of hip (D. M. Aitken), *Orth.* 13
- Osteogenesis imperfecta**, two cases of (B. Myers), *Child.* 69-72
 measurements in case of, *Child.* 70
- Osteomyelitis** of femur, outward dislocation of patella secondary to, case (H. A. T. Fairbank), *Orth.* 47
 of frontal bone, extensive, case (F. H. Westmacott), *Laryng.* 93
 invasion of frontal sinus, in case of orbital cellulitis (Sir W. Milligan and F. Wrigley), *Laryng.* 90
 syphilitic, involving elbow-joint, case (C. Max Page), *Clin.* 32
- Osteoporosis** in otosclerosis and osteitis deformans, comparison of, *Otol.* 21, 27
- Osteotomy**, sub-trochanteric, oblique, for relief of pain in congenital dislocation of hips, *Orth.* 22
- Oswald**, H. R.—Discussion on Coroners' Inquests, *Anæsth.* 39
- Otitic meningitis**, morbid anatomy and drainage of (E. D. D. Davis), *Otol.* 43
- Otitis media**, acute, right side, cerebellar abscess five weeks after onset of, case (S. Scott), *Otol.* 57
 chronic, associated with unilateral affection of cranial nerves 9 to 12 (Tapia's syndrome), case (C. P. Symonds), *Neur.* 53
 suppurative, chronic, ependymal glioma growing from floor of fourth ventricle, simulating cerebellar abscess in case of, section from (T. H. Just), *Otol.* 62
 with facial palsy, following scarlet fever, case, specimen (malleus and incus) shown (F. J. Cleminson), *Otol.* 17
- Otophone** (Marconi), new instrument for assisting the deaf, *Otol.* 51
- Otosclerosis** and osteitis deformans: a pathological and clinical comparison (abstract), (G. J. Jenkins), *Otol.* 21-26
 clinical aspects contrasted, *Otol.* 22
 conclusions regarding, *Otol.* 25
 microscopical appearances contrasted, *Otol.* 21, 27
 bone conduction usually reduced in, *Otol.* 22
 "group," definition of, *Otol.* 23
 paradoxa, cases (A. A. Gray), *Otol.* 9, 10
 relationship of chronic hyperplasia of upper jaw to, *Laryng.* 73
 times of better hearing during course of, *Otol.* 10, 11
 with unusual symptom (otosclerosis paradoxa) cases (A. A. Gray), *Otol.* 9, 10
- Ovarian** disease in the cow, relation of Graves' disease and thyroid instability to, *Obst.* 92-99
 extract in treatment of case of (?) thymic asthma with amenorrhœa, *Therap.* 4
 medication in organotherapy, *Therap.* 13, 17
 tumour, torsion of pedicle of, causing stretching of epithelium of tubal rugæ by blood effused into them (H. R. Spencer), *Obst.* 106-109
- Ovariectomy** in treatment of ruptured unilateral solid cancer of ovary, no recurrence six years later (H. R. Spencer), *Obst.* 105
- Ovary**, breast, uterus, cancer of, mortality compared (1900, 1915, 1920), *Occ. Lect.* 86
 cancer of, unilateral solid, ruptured; ovariectomy, no recurrence six years later (H. R. Spencer), *Obst.* 105
 cysts of, infiltrating, with tarry contents, association with adenomyoma of recto-vaginal space, *Obst.* 82
 dermoid tumour of, sarcoma in, case (H. R. Spencer), *Obst.* 101-105
 hæmatoma of, ruptured, with extensive intraperitoneal hæmorrhage (L. C. Rivett), *Obst.* 81
 left, cyst of, appearances and microscopical structure of, *Obst.* 102, 104
- Oxygen** dissociation curve at high altitudes, change in, *Med.* 58
 inhalation of, value of, in diminished atmospheric pressure, *Med.* 61
 saturation, arterial, at various altitudes, *Med.* 58, 162
 taken by flyers at high altitudes, benefits of, *Med.* 61
 want, height at which first experienced by flyers, *Med.* 60

- Page, C. Max., D.S.O.**—Case of myositis ossificans exhibiting acute symptoms, *Clin.* 32
 case of syphilitic osteomyelitis involving elbow-joint, *Clin.* 32
 discussion on birth injuries, *Child.* 77
 on cases of duodenal obstruction in infants, *Child.* 13
 four cases of flexion contracture of forearm treated by a muscle-sliding operation, *Orth.* 43, 44
 and **Jewesbury, R. C.**—Two cases of duodenal obstruction in infants treated by operation, *Child.* 50
- Paget's** disease and Bowen's disease, differential diagnosis, *Derm.* 27
- Pain**, chief symptom in calcified abdominal glands, *Urol.* 6
 in cases of dysmenorrhœa, features of, *Obst.* 110
 in congenital dislocation of hips, operation for relief of, *Orth.* 22
- Palate** and fauces, ulceration of, cases (T. J. Faulder), *Laryng.* 53 (W. H. Kelson and W. H. Thornhill), *Laryng.* 13
 and tonsil, growth in, multiple foci of, case (W. M. Mollison), *Laryng.* 19
 clonic spasm of, case (D. McKenzie), *Laryng.* 57
 left half of, and left vocal cord, paralysis of, two cases of (Sir J. Dundas-Grant), *Laryng.* 68
 pharynx and larynx, extensive lupus of, case (W. Howarth), *Laryng.* 50
 soft, and left anterior faucial pillar, epithelioma of, case (Sir W. Milligan), *Laryng.* 88
- Palmar** reflex, left, in case of right fronto-parietal tumour; cracked-pot percussion note over right frontal bone (G. Riddoch and W. R. Brain), *Neur.* 84
- Palmer, A. C.**—Mass of secondary leiomyosarcoma following subtotal hysterectomy, *Obst.* 62
- Panarolo, Domenico**, "Aerologia" of, described (G. Hinsdale), *Baln.* 19-21
 biographical notes of, *Baln.* 19
- Pancreas**, influence on nutrition, *Psych.* 24
- Pancreatic** extract, see *Insulin*
 medication in organotherapy, *Therap.* 13
- Pannett, C. A.**—Technique of axial anastomosis of alimentary canal, *Surg.* 81-83
- Papilloedema**, with detached retina in each eye, *Ophth.* 21
- Papilloma**, of renal pelvis, malignant, specimen (J. Macalpine), *Urol.* 37
 of renal pelvis, malignant, simple, of septum nasi, specimen (J. Macalpine), *Urol.* 37
 of septum nasi (H. Lawson Whale), *Laryng.* 12
- Papillomata**, multiple, of larynx, case (H. J. Banks-Davis), *Derm.* 45
 of nose, multiple, specimen from case of (H. J. Banks-Davis), *Laryng.* 46
 of trachea, case of (Sir J. Dundas-Grant and J. J. Perkins), *Laryng.* 7
- Papulonecrotic** tuberculides (?), case for diagnosis (E. G. Graham Little), *Derm.* 103
- Paracusis** willisii, cause of, discussed, *Otol.* 10
- Paræsthesiæ** in upper and lower limbs in pernicious anæmia, *Med.* 20
- Paragonimus** (genus), lung flukes of (G. M. Ververs), *Trop.* 43, 44
compactus, *Trop.* 43
Paragonimus, kellicotti, *Trop.* 43, 44
ringeri, *Trop.* 43
rudis, *Trop.* 43
 species of, differentiation by variations in cuticular spines, *Trop.* 44
westermanii, *Trop.* 43
- Parakeratosis** variegata in man aged 60 (S. E. Dore), *Derm.* 19
 in man aged 40 (S. E. Dore), *Derm.* 104
- Paralysis** agitans, following encephalitis lethargica, symptoms of, case (O. C. M. Davis), *Child.* 81
 post-encephalitic, cases of (E. Stolkind), *Clin.* 47-49
- Paralysis**, ischæmic, case (H. A. T. Fairbank), *Orth.* 11
 musculo-spiral, in chronic alcoholism, *Neur.* 15
 spastic, due to birth injury, *Child.* 76, 77
 operative treatment of, discussion on, *Orth.* 33-42
- Paralysis**, general, of insane, group of symptoms and signs, not a disease, *Neur.* 73
 intracisternal treatment of, *Neur.* 69-72
 juvenile, case of (C. Worster-Drought), *Neur.* 82
 mental symptoms in early stage of, *Psych.* 28
- Paralytic** dislocations of hips, treatment of, *Orth.* 23
- Paramore, R. H.**—Discussions on factors in uræmia, *Urol.* 28, 29
- Parapsoriasis**, case of (E. G. G. Little), *Derm.* 11
 type xantho-erythrodermia perstans, case (W. Fox), *Derm.* 91
- Parapsoriasis en plaques, erythrodermic pityriasi-que en plaques dissimulées** (Brocq), case (H. C. Semon), *Derm.* 103
- Parasite** of molluscum contagiosum, identification of, *Derm.* 4-7
- Parasympathetic** system, influence of, on asthma, *Therap.* 3
- Parathyroid** medication, details of cases treated by, *Therap.* 19, 20, 21
 dosage employed, *Therap.* 21
 in organotherapy, *Therap.* 12, 16
 sepsis underlying all diseases improved by, *Therap.* 23
- Parity**, maternal mortality in eclampsia in relation to, *Obst.* 2, 5
- Parker, C. A.**—Development of laryngology, *Laryng.* 1, 2
 discussion on case of chronic laryngitis of long standing, *Laryng.* 24
 on case of œdema of septum in association with nasal polypi, *Laryng.* 29
 on improved antrum-exploring trocar and cannula, *Laryng.* 53
 on lingual tonsil, *Laryng.* 75
- Parkinson, J.**—Discussion on systematic examination of heart, *Anæsth.* 31
- Parkinsonian** syndrome associated with right hemiplegia, showing peculiar disturbances of tone and posture in limbs on hemiplegic side, sequelæ of lethargic encephalitis, case (D. McAlpine), *Neur.* 27
 encephalitis lethargica and, *Child.* 35, 38, 39
- Parkinson's** disease, nasal stenosis mainly subjective, in case of (Sir J. Dundas-Grant and C. C. Worster-Drought), *Laryng.* 23

- Parotid fistula following mastoid operations** (N. Patterson), *Otol.* 19
in scar of old mastoid wound (H. J. Banks-Davis), *Otol.* 30
- Parotitis and uveitis, polyneuritis with**, *Neur.* 17
- Parsons, Sir J., F.R.S.**—Discussion on cases of tumours of optic nerve, *Ophth.* 35
on embryology of congenital crescents, *Ophth.* 47
- Parsons-Smith, B. T.**—Case of auricular fibrillation, reversion to normal rhythm under administration of quinidine, *Clin.* 50-52
discussion on action of quinidine in cases of cardiac disease, *Therap.* 41
- Parturition**, decrease of deafness immediately before, *Otol.* 11
- Pasteur Centenary**, celebration of, *Occ. Lect.* 11-28
as artist (G. Monod), *Occ. Lect.* 21-27
as chemist (T. M. Lowry), *Occ. Lect.* 16-20
biological application of his studies, *Occ. Lect.* 19, 20
researches of Malus, Arago, and Biot described, *Occ. Lect.* 16
work on tartaric acid, *Occ. Lect.* 16, 17, 18
in relation to medicine (Sir W. Hale-White), *Occ. Lect.* 11-17
discovery of doctrine of attenuation of virus, *Occ. Lect.* 13
investigations into rabies, *Occ. Lect.* 15
Lister's work in relation to, *Occ. Lect.* 14, 15
studies on fermentation described, *Occ. Lect.* 12, 13
- Patella**, dislocation of, outwards, secondary to osteomyelitis of femur, case (H. A. T. Fairbank), *Orth.* 47
fractures of, treatment of (R. H. A. Whitelocke), *Surg.* 111-119
causes and varieties, *Surg.* 111
osseous suture in, objections to, *Surg.* 113
treatment of, by open circumferential looping or cerclage, *Surg.* 114
by open operation only method of securing bony union, *Surg.* 112
importance of after-care in, *Surg.* 118, 119
technique of operation, *Surg.* 114-118
value of absorbable sutures in, *Surg.* 115
refracture of, ten years after fibrous union without open operation, *Surg.* 111
- Paterson, Donald**—Case of pellagra, *Child.* 61
case of progeria, *Child.* 42
discussion on case for diagnosis (? renal dwarfism), *Child.* 21
on case of multiple papillomata of larynx, *Laryng.* 45
three cases of inborn errors of metabolism, *Child.* 27
two cases illustrating the Schick test, *Child.* 42
- Paterson, D. R.**—Treatment of large foreign bodies impacted in gullet (abstract), *Laryng.* 77
- Patient and doctor**, use and abuse of relationship between, in practice of psychotherapy (Mary C. Bell), *Psych.* 12-19
- Patients**, systematic cardiac examination of, before operation, *Anesth.* 25, 26
- Paton, Leslie**—Case of recurrent detached retina after 17 years' reposition, *Ophth.* 14
discussion on case of amaurotic family idiocy, *Ophth.* 18
of primary band-shaped opacity of both corneæ, *Ophth.* 32
on cases of endothelioma of orbit, *Ophth.* 37
on case of retinitis circinata, *Ophth.* 12
on dystrophia myotonica, *Neur.* 44
hæmangioma of orbit, *Ophth.* 13
optic atrophy after herpes ophthalmicus, *Ophth.* 27-30
- Patterson, N.**—Myeloid sarcoma of posterior pillar of fauces, *Laryng.* 13
parotid fistula following mastoid operations, *Otol.* 19
and Cathcart, G. C.—Tuberculoma of pharynx, *Laryng.* 51
- Pearson, S. Vere**—Discussion on cauterization of adhesions under guidance of thoracoscope, *Electr.* 61
on economics and tuberculosis, *Epid.* 17
- Pectoral muscles**, congenital absence of, in male infant aged 16 months, case (H. T. Gray), *Child.* 44
- Pellagra**, ætiology discussed, *Child.* 62, 63, 64
cases of, (R. Hutchison), *Child.* 61 : (D. Paterson), *Child.* 61
(?) case of (H. S. Stannus), *Derm.* 27
discussion on (G. Pernet, A., Whitfield, H. G. Adamson), *Derm.* 28
dietetic factor in, *Child.* 63, 64
in Great Britain, *Child.* 62
infective origin advocated, *Child.* 62-64
Investigation Committee, work of, *Child.* 62
- Pelvic hypoplasia**, frequent association with dysmenorrhœa and menorrhagia, *Obst.* 111
inflammation, contra-indication to treatment by radium, *Obst.* 73
organs, female, adenomyomata of, clinical aspects of (A. Donald), *Obst.* 82-90
tumours, cystoscopic diagnosis of, *Surg.* 88
- Peninsular War**, venereal disease in, *War* 16
- Penis**, (?) epithelioma of, case (A. E. M. Woolf), *Clin.* 1
- Peri-apical dental infection** in relationship to nose, throat and ear affections, *Odont.* 36, 41
treatment, *Odont.* 41
bone necrosis, term suggested in place of apical dental abscesses, *Odont.* 11
- Perineal region of infant**, double tumour, (?) lipoma, in (B. Myers), *Child.* 6
- Perineum** of male aged 62, two large calculi removed from (W. G. Sutcliffe), *Urol.* 36
- Periodontitis**, cervical, in relationship to nose, throat and ear affections, *Odont.* 36
incidence of, factors determining, *Odont.* 36
in dysentery, *Odont.* 4
- Peripheral nervous features** in pernicious anæmia, *Med.* 20
stimuli, asthma and, *Therap.* 2
- Peristalsis**, intestinal, stimulation by thyroidal hormones, *Therap.* 16
- Peritomy**, in treatment of tubercular conjunctivitis, *Ophth.* 5, 6

- Peritoneal cavity**, instruments left in, effects and results as shown by analysis of 44 hitherto unpublished cases (C. White), *Obst.* 36-43
- Peritoneum**, recuperative powers of, *Surg.* 6
- Peritonitis**, mortality statistics 1880-1919, Guy's Hospital, *Med.* 45
(non-puerperal) mortality from 1901-1920, statistics, *Surg.* 7, 8
perforative, acute, cases illustrating dangers of washing out peritoneum, *Surg.* 6
- Perityphlitis**, mortality statistics (1901-1920), *Surg.* 7, 8
Guy's Hospital (1880-1919), *Med.* 45
- Perkins, G.**—Case of Erb's paralysis, *Child.* 74
case of pseudo-coxalgia in adult, *Orth.* 48
of spastic hemiplegia, *Child.* 75
discussion on birth injuries, *Child.* 78
- Perkins, J. J. and Dundas-Grant, Sir J.**—Case of papillomata of trachea, *Laryng.* 7
- Pernet, G.**—Case of disseminated lupus erythematosus associated with Raynaud symptoms and early sclerodactylia, *Derm.* 91
of lichen planus annulatus, with atrophy and a herald patch, *Derm.* 2
of lupus erythematosus associated with lichen planus, *Derm.* 27
discussion on cases of ringworm of nails, *Derm.* 2
on case of scleroderma, *Derm.* 29
- Pernicious anæmia**, see *Anæmia*, pernicious
- Peroneal type** of muscular atrophy commencing in, and for some time confined to, right hand, case (C. P. Symonds), *Neur.* 80
- Perry, H. M., Lt.-Col., O.B.E.**—Observations on occurrence of *Leishmania* in intestinal tissues in Indian kala-azar; on the pathological changes occasioned by their presence, and their possible significance in this situation, *Trop.* 1-8
- Perry, cases** of epithelioma adenoides cysticum (quoted), *Derm.* 31, 32
- Pertussis**, see *Whooping-cough*
- Pestilence**, Great, entry into England in 1348, *Hist.* 28
in 1348, (also called The Murrain, The Mortality and The Death), *Hist.* 33
descriptions of, *Hist.* 33, 34
economic and social changes influenced by, *Hist.* 35, 36
nature of, *Hist.* 33
origin of, supposed to be in China in 1333, *Hist.* 27
psychological effects of, *Hist.* 36
references to, in chronicles of various districts, *Hist.* 37-45
second and third outbreaks in 1361 and 1369, *Hist.* 32
symptoms of, *Hist.* 33
unequal distribution of deaths in various districts, *Hist.* 31
- Petit, G.**—Lymphangioma circumscriptum of tongue, *Derm.* 58
- Phagocytic action**, Metchnikoff's conception of, *Path.* 4
- Phalangeal joints**, synostosis of, (?) congenital in origin, specimen of (W. H. Ogilvie), *Orth.* 51
pouch, case of (A. Ryland), *Laryng.* 41
- Pharynx, larynx and palate**, extensive lupus of, case (W. Howarth), *Laryng.* 50
posterior wall of, swelling on (F. Spicer), *Laryng.* 57
tuberculoma of, case (N. Patterson and G. C. Cathcart), *Laryng.* 51
- Phenol-sulphone-phthalein** excretion, low in cases of retinitis with gross disturbance of renal functions, *Med. and Ophth.* 17, 18
- Phillips, G. E. S.**—Apparatus for recording X-ray doses, *Electr.* 30
- Phillips, H.**—Discussion on anæsthesia in dental surgery, *Anæsth.* 22
- Phillips, L.**—Treatment of dysmenorrhœa, an analysis of 100 cases, *Obst.* 110-115
- Phillips, M.**—Discussion on Cesarean section, *Obst.* 59
- Phlebotomus papatasi*, sand-fly, life history of (H. E. Whittingham), *Trop.* 45
- Phlyctenular** affections, rarity among Jewish patients, *Epid.* 57
- Phrenic nerve**, electric responses of, during respiration, *Neur.* 57
- Phthisis**, death-rate in Vienna (1912-1920), as compared with total death-rate, *Epid.* 11
mortality from, in 1880 and 1920 compared, *Med.* 47
among coal-miners (aged 25-64), period 1890-1912, *Epid.* 88
among ironstone-miners per 1,000 living at various age periods, *Epid.* 96
per 1,000 living, at various age periods, in certain occupations, *Epid.* 90
per 1,000 living at various age periods on different coalfields, *Epid.* 91
see also under *Tuberculosis*, pulmonary
- Physical and mental efficiency**, effect of tropical climate on (T. S. Rippon), *War* 46-54
exercises in the Services as counteracting venereal tendencies, *War* 20
signs, attention to, changes in medicine due to, *Med.* 46
training in Army (Col. R. C. Campbell), *War* 31-37
as preparation for campaigns, *War* 39
charlatanism in, *War* 37, 40
clothing to be worn during, *War* 39, 42
connection of Board of Education with, *War* 36, 38, 41
continuity in, *War* 34, 39
co-operation with Civil Boards and Associations, *War* 36
human factor in, grading, *War* 32, 33
importance of, *War* 31
in peace and war, *War* 35
in regard to convalescents, *War* 34, 38, 41
instructors in, creation of, *War* 36
larger staff for, needed, *War* 35
mobilization and, *War* 35
preparation, *War* 32
psychological aspects, *War* 42
purpose of, *War* 33
standards of, means of securing, *War* 38, 41
time to be spent upon daily, *War* 39, 42
- Physics** of disruptive phenomena in gunshot injuries (S. G. Shattock), *Path.* 17-34
- Physiogenic** or psychogenic origin of mental disorder, *War* 53
- Physiological tests**, use of, in Royal Air Force, *War* 39

- Pickard, R.**—Discussion on optic atrophy after herpes ophthalmicus, *Ophth.* 30
- Pigmented** lesion for diagnosis (H. C. Semon), *Derm.* 59
- Pilocarpine**, effect on gastric secretion, *Therap.* 6
- Pineal** gland, effect on sexual development, *Psych.* 24
- Pitt, G. Newton, O.B.E.**—Changes in medicine and its methods in past 45 years, *Med.* 43-48
discussion on significance of vascular and other changes in retina in arterio-sclerosis and renal disease, *Med. & Ophth.* 1, 28
- Pitts, A. T., D.S.O.**—Case of Hutchinsonian teeth, *Child.* 45
congenital absence of all teeth except two, *Child.* 45
of teeth in three members of a family, *Child.* 44
discussion on dental sepsis, *Odont.* 29
on infections of teeth and gums in their relationship to nose, throat and ear, *Odont.* 41
- and **Waugh, G. E.**—Case of ankylosis of jaw, *Child.* 44
- Pituitary** extract and ergot, action compared, *Obst. & Therap.* 6
caution in use of, *Obst. & Therap.* 6
in labour, committee for investigation of, (*Obst. & Therap.* 6, 7
replacing ergot in obstetrical practice, *Obst. & Therap.* 4
gland, influence on nutrition, *Psych.*
over-stimulation, exemplified in physiognomy of inhabitants of United States, *Baln.* 13
medication in organotherapy, *Therap.* 12, 17
secretion in cerebro-spinal fluid, *Therap.* 23
- Pituitrin**, adrenalin reinforced by, *Therap.* 4
- Pityriasis** lichenoides chronica, case of (H. Davis), *Derm.* 92
- Place-Wertheim** Salomonson galvanometer, introduction of, *Elect.* 6
- Plague**, bubonic, great pestilence of 1348 regarded as virulent form of, *Hist.* 33
in England in 1087 and 1221, nature of, *Hist.* 32
spread of, views current during last half-century, *Epid.* 41
see also under *Pestilence*
- Planorbis dufourii** (Graells) intermediate host in bilharzia disease, specimens of, with photograph of endemic focus in Portugal, (J. B. Christopherson), *Trop.* 47
- Plassomyra contagiosa**, suggested name for parasite of molluscum contagiosum, *Derm.* 7
- Pleurisy**, tuberculous, brachial neuritis following, *Neur.* 18
- Pleuritic** exudates complicating cauterization of adhesions under guidance of thoracoscope, *Electr.* 55
- Plombières** douche, use of (A. G. Gibson), *Baln.* 22, 23
diagnostic value of, *Baln.* 22
therapeutic use of, conditions indicating, *Baln.* 23
- Pneumonia** after anaesthesia in dental surgery, two cases, *Anaesth.* 19
mortality from, among coal-miners (ages 25-64), period 1890-1912, *Epid.* 88
in 1880 and 1920 compared, *Med.* 47
- Pneumopericardium**, artificial, case (R. W. A. Salmoud), *Elect.* 71-74
- Pneumothorax** (artificial), treatment of pulmonary tuberculosis under thoracoscopic control, cauterization of adhesions in (H. C. Jacobæus), *Electr.* 45-60
artificial, adhesions in, injurious effects of, *Electr.* 46
- Poisoning**, chronic, causing acute toxic hæmaturia, *Neur.* 25
mercurial, chronic, case (E. A. Carmichael), *Neur.* 80
- Polio-encephalitis**, diagnosis of spastic diplegia and hemiplegia due to birth-injury from the same diseases resulting from, *Child.* 78
- Poliomyelitis**, acute, and polyneuritis, differential diagnosis, *Neur.* 26
- Pollock, Inglis.**—Discussion on standards of vision for scholars and teachers, *Ophth.* 9
- Polyarthrititis**, case of (J. H. Thursfield), *Child.* 41
- Polycythæmia**, ? case for diagnosis (P. Bardsley), *Ophth.* 19
vera, with cerebral hæmorrhage, case of (J. A. Ryle), *Med.* 63
- Polyneuritic** psychosis (Korsakow), *Neur.* 16
- Polyneuritis**, auto-toxæmia causing, *Neur.* 19
beri-beri and, *Neur.* 19, 20
cachectic causes of, *Neur.* 18
chronic, slow, *Neur.* 21, 22
cranialis, syndrome of, *Neur.* 21
differential diagnosis from acute poliomyelitis and Landry's paralysis, *Neur.* 26
due to carbon bisulphide, *Neur.* 14
to carbon monoxide, *Neur.* 14
to malignant disease, *Neur.* 19
to poisoning from lead water-pipes, case, *Neur.* 14
to syphilis, *Neur.* 17
to tuberculosis, rarity, *Neur.* 18
failure of nerve conduction in, *Neur.* 58
febrile, acute, *Neur.* 17
optic neuritis in, *Neur.* 21
puerperal, *Neur.* 20
relapsing and recurrent, *Neur.* 22
rheumatic, *Neur.* 14, 19
silver, Gowers' case of, *Neur.* 13
toxic, *Neur.* 13
differentiation from pressure neuritis, *Neur.* 20, 21
- Polypi**, nasal, in association with œdema of septum, case (A. J. Wright), *Laryng.* 27
- Polypus**, bleeding (pedunculated angioma) of inferior turbinal, case (S. Hastings), *Laryng.* 25
- Portsmouth**, Town Council of, steps taken by, as to education in control of cancer, *Occ. Lect.* 38, 39
- Portugal**, endemic focus of bilharzia disease in, photograph of; specimens of intermediate host, *Planorbis dufourii* (Graells), remarks on (J. B. Christopherson), *Trop.* 47
- Post-encephalitic** paralysis agitans, cases of (E. Stolkind), *Clin.* 47-49
- Potter-Bucky** diaphragm, use of, in X-rays of spine, advantages of, *Med.* 68
- Potters**, mortality from phthisis per 1,000 living at various age periods, *Epid.* 90

- Potts, W. A.**—Discussion on relationship between doctor and patient in psychotherapy, *Psych.* 20
- Pouch, pharyngeal**, case of (A. Ryland), *Laryng.* 41
- Poulton, E. P.**—Views on acidosis in rickets (quoted), *Child.* 3
- Poverty** and prevalence of tuberculosis, relationship between, *Epid.* 11
- Powell, A.**—Discussion on case for diagnosis, *Derm.* 57
on leishmaniasis of skin, *Derm.* 10
framboesia, history of its introduction into India, with personal observations on over 200 initial lesions, *Trop.* 15-42
- Powell, L.**—Case of fibroma of nose, *Laryng.* 66
- Poynton, F. J.**—Case of cretinism, *Child.* 43
discussion on case of acquired chronic hæmolytic (acholuric) jaundice, *Med.* 80
encephalitis lethargica, showing late results, *Child.* 31
- Pregnancy** and parturition, mortality in 1880 and 1920 compared, *Med.* 47
complicating fibroids, treatment by myomectomy combined with Cæsarean section, *Obst.* 22
deafness becoming worse after each successive, *Otol.* 11
full term, hemiplegia occurring in, sudden onset accompanied by transient albuminuria; Cæsarean section; gradual recovery, case (F. Cook), *Clin.* 43
myomectomy for uterine fibroids during, indications for and table of results, *Obst.* 19-21
results of myomectomy for uterine fibroids in relation to, *Obst.* 15, 16
toxæmic retinitis of, preferred to term albuminuria, *Med. & Ophth.* 30
- Pressure** neuritis, differentiation from toxic polyneuritis, *Neur.* 20
- Printers**, mortality from phthisis per 1,000 living at various age periods, *Epid.* 90
- Pritchard, Eric**—Discussion on case for diagnosis, *Child.* 55
on case of enlarged liver with persistent acetoneuria and diaceturia, *Child.* 59
on case of osteogenesis imperfecta, *Child.* 72
on case of patent ductus arteriosus and mitral disease, *Child.* 49
on cases of pellagra, *Child.* 61
on case of Raynaud's syndrome in non-syphilitic infant, *Child.* 48
on cases of duodenal obstruction in infants, *Child.* 13
on late effects of encephalitis lethargica, *Child.* 33
pathogenesis of rickets (President's address), *Child.* 1-9
- Proctitis**, gonorrhœal, *Proct.* 16
- Progeria**, case of (D. Paterson), *Child.* 42
- Prognosis**, effect of cardio-vascular changes on, *Med. & Ophth.* 32
injurious declarations of, *Med. & Ophth.* 32, 33
- Projection**, definition of, *Psych.* 13
identification process in psychotherapy, *Psych.* 13, 14
- Proptosis**, bilateral, with limitation of movement in one eye, case (R. A. Greeves), *Ophth.* 15
- Prostate**, adenoma of, suprapubic prostatectomy for, primary union after, 14 cases, *Urol.* 47, 48
and bladder, operations on, primary union after, advantages, *Urol.* 50
conditions contra-indicating, *Urol.* 51
propriety of attempting to secure (A. R. Thompson), *Urol.* 47-53
cyst of, case (Sir J. W. Thomson-Walker), *Urol.* 31
enlargement of, apparently benign, malignant disease in, incidence of (R. H. Jocelyn-Swan), *Urol.* 71-77
malignant disease in, five cases illustrating, *Urol.* 71-74
complicating vesical diverticula, operative treatment in, *Urol.* 66
diagnosis of, nature of, extreme care required, *Urol.* 78
parathyroid medication in, *Therap.* 21
pathology of, *Urol.* 76
prognosis difficulty of three cases illustrating (F. Kidd), *Urol.* 78, 79
malignant disease of, suprapubic cystotomy for, primary union after, case, *Urol.* 48
treatment by X-rays, *Urol.* 78
- Prostates**, removed by operation, examination of, table showing results of, *Urol.* 75
- Prostatectomy**, prostate removed by, weight 12 oz. or 340 grm. (R. H. Jocelyn Swan), *Urol.* 42
suprapubic, closure of suprapubic urinary fistula following, observations on 68 cases (H. P. W. White), *Surg.* 119-125
see also under *Fistula*
for adenoma of prostate, primary union after, 14 cases, *Urol.* 47, 48
total, Young's operation, *Urol.* 78
- Prostatic** nodule, aberrant, case (Sir J. W. Thomson-Walker), *Urol.* 32
- Proteins**, foreign, and asthma, *Therap.* 2
separation from carbohydrates during sterilization, new fermentation tube for (C. Dukes), *Path.* 13-16
- Proteolytic** bodies in leucocytes, *Path.* 6
- Protozoa**, cell processes of, conduction with a decrement in, *Neur.* 57
- Provis, F. L.**—Discussion on treatment of uterine hæmorrhage by radium, *Obst.* 79
- Pseudo-coxalgia** in adult, case (G. Perkins), *Orth.* 48
- Psoriasis** of anomalous type, case (H. Davis), *Derm.* 72
thyroidal hormone in treatment of, *Therap.* 15
- Psychæsthesia**, or feeling-tone, endocrine system as basis of, *Psych.* 26
normal variations in, *Psych.* 27
- Psychical** stimuli, asthma and, *Therap.* 2
effects of, illustrated, *Psych.* 27
- Psycho-analysis**, practice of, qualities required for, *Psych.* 18, 19
principle of, views of Jung and Freud contrasted, *Psych.* 20
- Psychogenic** or physiogenic origin of mental disorder, *War* 53
- Psychology**, biology in relation to, *Psych.* 34
- Psychoneurosis**, effects of, on mental efficiency in tropical climate, *War* 52, 53
in flying personnel, *War* 53

- Psychoneurosis**, visual disorder as cause of, *War* 53
- Psycho-physical** reaction, endocrine balance in relation to, *Psych.* 31
- Psychotherapy**, practice of, relationship between doctor and patient in, use and abuse of (Mary C. Bell), *Psych.* 12-19
- processes of projection and identification in, *Psych.* 13, 14
- Pterygo-maxillary** abscess, otitic, induced by thrombo-phlebitis of jugular bulb (D. McKenzie), *Otol.* 53
- Ptosis**, visceral, see *Visceroptosis*
- Puerperal** neuritis, *Neur.* 20
- Puerperium**, inversion of uterus occurring at third week of (W. R. White-Cooper and H. K. Griffith), *Obst.* 48, 49
- Pugh**, L. P.—Graves' disease and thyroid instability in the cow, and its relation to ovarian disease, *Obst.* 92-99
- Pugh**, W. T. Gordon.—Discussion on operative treatment of spastic paralysis, *Orth.* 39
- fracture of small trochanter, *Orth.* 12
- two cases of fractured neck of femur in training-ship boys, *Orth.* 31
- Pulmonary** complications after Cæsarean section, *Obst.* 50, 51
- fibrosis following tuberculous infection, case (E. C. Williams), *Child.* 79
- Pulp**, dental, see *Dental pulp*
- Pulse**, effect of high altitudes on, *Med.* 59
- rate, effect upon prognosis of eclampsia, *Obst.* 3
- rhythm, alteration of, in response to nocuous stimulus, *Psych.* 5
- small, associated with congenital heart lesion and ichthyosis, case (O. C. M. Davis), *Child.* 80
- Purpura** hæmorrhagica, case (B. Myers), *Clin.* 10
- Purves-Stewart**, Sir J., K.C.M.G.—Discussion on treatment of neuro-syphilis, *Neur.* 69
- Pyelogram** illustrating breaking of two shadows into multiple shadows as result of injection of sodium bromide (W. Girling Ball), *Urol.* 85
- Pyelography** in X-ray diagnosis of calcified abdominal glands, value of, *Urol.* 13, 14
- lateral, showing calcified abdominal gland, *Urol.* 14
- Pyelotomy**, subcapsular, with remarks on origin and treatment of renal calculi (W. S. Handley), *Surg.* 21-37
- case records, *Surg.* 34-37
- complications of, *Surg.* 29-31
- risk of, *Surg.* 38
- technique of, *Surg.* 26-29
- with small incisions over thinned areas in removal of calculi, *Surg.* 38
- Pylorus**, duodenal ulcer involving, change in epoch of pain, relief by gastro-enterostomy, case (H. S. Souttar), *Clin.* 6
- Pyonephrosis**, calculous, bilateral, ten years after double nephro-lithotomy, case (P. Turner), *Clin.* 40
- due to kinking of ureter by aberrant renal vessels (R. H. Jocelyn Swan), *Urol.* 41
- Pyorrhœa** alveolaris, a deficiency disease, *Odont.* 4
- factors in causation of, *Odont.* 3
- incidence of, factors determining, *Odont.* 36
- latent, more injurious than purulent septic gingivitis, *Odont.* 39
- prevalence in India widespread, *Odont.* 26
- relation to Ménière's symptoms, *Odont.* 40
- treatment, local, *Therap.* 22
- Pyriform** sinus, left, (?) case of clinically malignant disease of (F. Holt Diggle), *Laryng.* 95
- Quadriceps** muscles, atrophic paresis of, symmetrical, of probable myopathic origin, *Neur.* 1
- bilateral progressive weakness and wasting of, case, *Neur.* 1
- with congenital absence of brachio-radialis and biceps femoris, case, *Neur.* 1, 2
- Quadriplegia** with traumatic spondylitis, case (D. McAlpine), *Neur.* 83
- Quartz**, optical rotatory power of, *Occ. Lect.* 19
- Quinidine**, action of, in cases of auricular fibrillation associated with thyro-toxic activity, *Therap.* 27, 28
- action in cases of cardiac disease, discussion on, *Therap.* 25-42
- conclusions regarding, *Therap.* 30
- prognosis, and tendency to relapse, *Therap.* 36
- restoration to normal rhythm as therapeutic procedure, *Therap.* 35
- summary, *Therap.* 37, 38
- in cases of thyro-toxic activity, *Therap.* 27-29
- administration in cases of cardiac disease, dangerous symptoms resulting, *Therap.* 26
- ill effects of, *Therap.* 33
- subjects most suitable for, *Therap.* 39
- in paroxysmal tachycardia, *Therap.* 39, 42
- sudden death following, case of, *Therap.* 40
- symptoms due to toxic action on myocardium, *Therap.* 34
- of cerebral nature due to, *Therap.* 34
- effects on cardiac musculature, *Therap.* 30-32, 42
- in treatment of auricular fibrillation, details of 45 cases, *Therap.* 33
- causing reversion to normal rhythm (B. T. Parsons-Smith), *Clin.* 50
- dosage employed, *Therap.* 33, 40
- results, *Therap.* 40
- intoxication, symptoms of, *Therap.* 33
- poisoning, symptoms of, *Therap.* 30
- therapy, indications for, *Therap.* 37
- and digitalis in treatment of auricular fibrillation, relative merits of, *Therap.* 35
- Rabies**, Pasteur's investigation into, *Occ. Lect.* 15
- in Irak, prevalence of, *Trop.* 50
- in the Tropics, diagnosis of, methods of staining in, *Trop.* 51
- presence of, biological test to prove first cases of, necessity for, *Trop.* 52
- value of early vaccine treatment in, case illustrating, *Trop. Med.* 50, 51
- Radiograms**, position of shadows in renal and gall-bladder areas as seen in, compared, *Urol.* 8

- Radiographic evidence of alveolar disease in dental sepsis**, *Odont.* 10, 11
- Radiographs of teeth in dental sepsis, importance of correct interpretation of**, *Odont.* 22, 23
- value of, in diagnosis of gastric lesions, *Electr.* 36
- Radiography, instantaneous, in cardiac diagnosis**, *Electr.* 8
- lateral and pyelography in diagnosis of calcified abdominal glands, *Urol.* 13
- showing renal calculi, *Urol.* 15
- Radioscopy, investigation of cardiac shadow by**, *Electr.* 6
- Radium, before operation, in treatment of cancer of cervix, two cases of** (T. W. Eden and A. Goodwin), *Obst.* 32, 34, 35
- epithelioma of vestibule of nose after treatment by, case of (Sir J. Dundas-Grant), *Laryng.* 65
- exposure to, effects of, in relation to thrombopenia and anaemia, *Electr.* 44
- effects on alimentary canal, (J. C. Mottram), *Electr.* 41-44
- on blood platelets (J. C. Mottram), *Path.* 9-13
- on caecum, *Electr.* 42
- on colon, *Electr.* 42
- on small intestine, *Electr.* 42
- menopause, cause of, considered, *Obst.* 72, 73
- sarcoma of nose cured by, case (E. M. Woodman), *Laryng.* 49
- treatment by, of adenomyomata of female pelvic organs, *Obst.* 91, 92
- of dysmenorrhœa, *Obst.* 115, 116
- of malignant disease in upper jaw, destructive effects of, *Laryng.* 87
- of multiple papillomata of larynx, use discussed, *Laryng.* 45, 46
- of severe and persistent uterine hæmorrhage, with report on 45 cases (S. Forsdike), *Obst.* 69
- classification of cases, *Obst.* 71
- clinical types of cases, *Obst.* 70
- contra-indications to, *Obst.* 73
- dosage, *Obst.* 70
- epitome and selection of cases, *Obst.* 69, 77, 78
- microscopical appearances of endometrium before and after exposure, *Obst.* 74-76
- number and types of operations, *Obst.* 70
- relative advantages of radium, X-ray and operation, *Obst.* 77
- symptoms following, *Obst.* 72
- technique, *Obst.* 70
- uterus removed for carcinoma of cervix after (A. H. Richardson), *Obst.* 31
- vasomotor symptoms, *Obst.* 72
- Radius, congenital absence of, with extreme eversion of hand, case** (H. T. Gray), *Child.* 43
- "Ramul," for opaque meal examination of stomach**, *Electr.* 37
- Radford, M.**—Case for diagnosis (prominence of right lower anterior thoracic wall in infant aged 9½ months), *Child.* 69
- Rainfall and scarlet fever, relationship between** (J. Brownlee), *Epid.* 30-34
- Ramsay, R. A.**—Discussion on cases of duodenal obstruction in infants, *Child.* 13
- Rat, Numa**, views on yaws quoted, *Trop.* 31
- Rat, in relation to anthrax infection**, *Path.* 7
- serum, thermostable bactericidal body in, *Path.* 5, 6, 7
- Rats, destruction of, on ships by hydrocyanic acid**, *Epid.* 41
- Raynaud symptoms and early sclerodactylia associated with disseminated lupus erythematosus, case of** (G. Pernet), *Derm.* 91
- syndrome in non-syphilitic infant, with remarkable family history (abstract), (F. Parkes Weber), *Child.* 47
- Rea, R. L.**—Case of hole in the hyaloid, *Ophth.* 20
- of papillœdema with detached retina in each eye, in young woman aged 22, *Ophth.* 21;
- of tuberculous eyelids, together with disseminated tubercle of body and limbs, *Ophth.* 45
- Read, C. Stanford**—Discussion on organic basis of the hysterical syndrome, *Psych.* 9, 10
- Recruits, unfit, methods of weeding out**, *War* 43
- untrained, methods of testing capacities of, *War* 39, 43
- Recto-vaginal septum, calcified tumour of, case** (L. C. Rivett), *Obst.* 81
- Rectum, carcinoma of, inoperable, case appearing clinically as, treated by colostomy and subsequent injections of cuprase-collosal selenium and colossal cuprum for over 2 years, with disappearance of growth** (L. E. C. Norbury), *Proct.* 67
- cancer of, operated upon by abdomino-anal method in June 1920, present condition of patient (H. Brown), *Proct.* 89
- stricture of, causes of, opinions of various authorities (quoted), *Proct.* 17, 18
- due to other causes than gonorrhœa, *Proct.* 20
- gonorrhœal (Sir C. Symonds), *Proct.* 12-19
- cases described, *Proct.* 12-15
- sex and age incidence of, *Proct.* 15, 16
- treatment of, *Proct.* 18
- Recurrent nerve, division of, in treatment of stenosis of larynx caused by bilateral abductor paralysis**, *Laryng.* 33
- Red Cross Society, educational work for control of cancer, willingness to undertake**, *Occ. Lect.* 37
- Reece, R. J., C.B.**—Discussion on mortality of coal and metalliferous miners, *Epid.* 100
- progress and problems in epidemiology, *Epid.* 35-48
- Rees, W.**—The Black Death in England and Wales, as exhibited in manorial documents, *Hist.* 27-45
- Registrar-General's reports of mortality from appendicitis, perityphlitis, peritonitis, intestinal obstruction and gastric ulcer, 1901-1920**, *Surg.* 7-8
- of mortality in mining industry, *Epid.* 85
- (1900, 1915-1920) showing comparative mortality from cancer of uterus, ovary and breast, *Occ. Lect.* 36
- Reld, Sir G. Archdall, K.B.E.**—New method of treating skin diseases, *Derm.* 109
- Renal complications of dental sepsis**, *Odont.* 14
- dwarfism after operation for genu-valgum, case (Paul B. Roth), *Orth.* 45
- case of (W. H. Ogilvie), *Orth.* 51
- (?) case for diagnosis (G. M. Rendall), *Child.* 20

- Renal function, disturbance of, and occurrence of**
retinitis, relationship between, table of
20 cases, *Med. & Ophth.* 17-19
insufficiency, contra-indication to antisyphilitic
treatment, *Neur.* 64
- Renshaw, J. A. K.**—Case of difficulty in enuncia-
tion and dysphagia, *Laryng.* 96
swelling of right side of palate, *Laryng.* 96
of tuberculosis of larynx, *Laryng.* 93
- Respiratory complications of dental sepsis**, *Odont.*
13
disease, mortality in 1880 and 1920 compared,
Med. 47
response to nocuous stimulus, *Psych.* 4, 5
- Respiration**, effect of high altitudes on, *Med.* 59
electric responses of phrenic nerve during,
Neur. 57
- Retina**, angeoid streaks of (F. A. Williamson-
Noble), *Ophth.* 1
changes in, in eclampsia, due to blood-
conditions, *Med. & Ophth.* 5
"cotton-wool patches in," in secondary
anemias, origin of, *Med. & Ophth.* 4, 5
cyst of (J. A. Valentine), *Ophth.* 1
degeneration with mental deficiency, case (F. A.
Juler), *Ophth.* 16
detached, recurrent, after 17 years' reposition,
case (L. Paton), *Ophth.* 14
hæmorrhages into, in anæmia and chlorosis,
Med. & Ophth. 4, 5
in arterio-sclerotic retinitis, capillary origin
considered, *Med. & Ophth.* 4
tuberculosis of, primary, *Ophth.* 3
vascular and other changes in, in arterio-
sclerosis and renal disease, significance
of, discussion on, *Med. & Ophth.* 1-36
- Retinitis** and disturbances of renal function,
relationship between, table of twenty
cases, *Med. & Ophth.* 17, 18, 19
arterio-sclerotic, hæmorrhage in, capillary origin
considered, *Med. & Ophth.* 4
relation of, to condition of vessels supplying
brain, *Med. & Ophth.* 27
suggestion of term made, *Med. & Ophth.* 10
association of high blood-pressure with, com-
patible with good health, *Med. & Ophth.*
32
circinata, *Med. & Ophth.* 30, 31
case (H. Neame), *Ophth.* 11
originating in choroid, *Ophth.* 11, 12
diabetic, explained, *Med. & Ophth.* 31
principal age-incidence of, *Med. & Ophth.*
31
differentiation according to disturbance of renal
function, cases of, *Med. & Ophth.* 17-19
distinction between simple high blood-pressure
and arterio-sclerotic changes in, by
ophthalmoscope, *Med. & Ophth.* 16
due to general arterio-sclerosis, formation of
exudates in, *Med. & Ophth.* 6
due to local vascular disease in retina, in some
cases of arterio-sclerosis, *Med. & Ophth.*
5-10
condition of urine in, *Med. & Ophth.* 10
histology, *Med. & Ophth.* 8
local development and disappearance of
exudate in, *Med. & Ophth.* 7, 8
length of life and manner of death of patients
with, *Med. & Ophth.* 9, 10, 14
- Retinitis**, ophthalmoscopic appearances, *Med. &*
Ophth. 6, 7
differences from those of renal retinitis,
Med. & Ophth. 7
table of cases (R. Foster Moore), *Med. &*
Ophth. 10-15
unilateral, frequency of, *Med. & Ophth.* 7
"hæmorrhagic" term proposed, *Med. & Ophth.*
22
new nomenclature for various changes sug-
gested, *Med. & Ophth.* 5
ophthalmoscopic examination in, value of, *Med.*
& Ophth. 15, 16
renal, cardinal signs of, present in cases with
albuminuria, *Med. & Ophth.* 15
classical signs of, *Med. & Ophth.* 15
difficulty of assigning cases as, *Med. & Ophth.*
23
pathology of, *Med. & Ophth.* 21
term criticized, *Med. & Ophth.* 15
toxic origin considered, *Med. & Ophth.* 2, 3, 4,
5, 15, 16
toxæmic of pregnancy, *Med.* 31
preferred to term "albuminuric," *Med. &*
Ophth. 30
- Retinoscope**, plane glass, (F. A. Williamson-
Noble), *Ophth.* 1
- Rheumatic conditions**, dental sepsis in relation to,
Odont. 15, 17
polyneuritis, *Neur.* 14, 19
- Rheumatism**, acute, parathyroid medication in,
Therap. 20
- Rhinitis**, purulent, acute, in infants, sequelæ of,
Odont. 37
- Rhinotomy**, lateral, in case of endothelioma of
ethmoid, *Laryng.* 94
- Richardson, A. H.**—Uterus removed for car-
cinoma of cervix after treatment by
radium, *Obst.* 31
- Rickets**, active, diminution of amount of calcium
in blood in, *Child.* 3
ætiology of depressants of metabolism in, *Child.* 7
factors in, groups schematized, *Child.* 8, 9
individual symptom complex in, *Child.* 9
development of, acidosis as cause of, *Child.* 2
pathogenesis of (E. Pritchard), *Child.* 1-9
references, *Child.* 9
treated by light therapy, case (R. C. Jewesbury),
Child. 25
treatment of, preventive or curative, stimuli
of metabolism used in, *Child.* 7
- Riddoch, G.**—Complete amaurosis, dementia and
spastic paralysis in Hebrew boy aged
10, *Neur.* 29
discussion on operative treatment of spastic
paralysis, *Orth.* 37
on treatment of neuro-syphilis, *Neur.* 74
and **Brain**, W. Russell—Case of right fronto-
parietal tumour; cracked-pot percussion
note over right frontal bone; left palmar
reflex, *Neur.* 84
- Ridout, C. A. S.**—Case of chronic laryngitis of
long standing, *Laryng.* 23
of laryngectomy following thyro-fissure,
Laryng. 8
parts removed post mortem in case of tracheal
obstruction, *Laryng.* 10
specimen of carcinomatous larynx removed by
laryngectomy, *Laryng.* 9

- Ridout, C. A. S.**—Tracheal obstruction due to (?) arrest of development of trachea, *Laryng.* 58
- Rifle bullet**, British, composition of, in relation to disruptive injuries from, *Path.* 29, 30
condition of leaden core when bullet leaves rifle considered, *Path.* 31, 32
effects of compression upon, *Path.* 32
- Rigidity** in fracture-dislocations in dorsal and lumbar regions of vertebral columns, *Orth.* 3
- Ringworm** of nails, two cases, in sisters (H. C. Semon), *Derm.* 1
treatment by salicylic ointment, *Derm.* 109
- Rinne test** in otosclerosis, *Otol.* 22
- Rippon, T. S.**—Discussion on measuring sensorimotor reaction times, *Electr.* 69
effect of tropical climate on physical and mental efficiency, *War* 46-54
- Rivett, L. C.**—Calcified tumour of rectovaginal septum, *Obst.* 81
ruptured hæmatoma of ovary, with extensive intraperitoneal hæmorrhage, *Obst.* 81
- Roberts, L.**—Discussion on case of psoriasis, *Derm.* 72
- Roberts, Morley.**—Nervous factor in asthma (quoted), *Therap.* 5
- Robertson, A.**—Specimens from human case of infection with *Dientamoeba fragilis* (Jepps and Dobell, 1917), *Trop.* 48
- Rodent ulcer**, multiple, case; possible embryonic sweat-duct origin (H. G. Adamson), *Derm.* 24, 25
under treatment with arsenic paste, case (A. H. M. Gray), *Derm.* 78
- Rodger, T. R.**—Discussion on otosclerosis and osteitis deformans, *Otol.* 27
- Roentgenkymography** in cardiac diagnosis, *Electr.* 19
- Roentgenocardiogram**, method of taking, *Electr.* 21
- Roentgenoradiograph**, disadvantages of, *Electr.* 21
- Roentgen-therapy**, treatment of two cases of erythremia (Vaquez disease) by (C. Stolkind), *Clin.* 35-38
see also X-ray
- Rolleston, Sir Humphry, K.C.B.**—Discussion on Pasteur, *Occ. Lect.* 27
on ulcerative colitis, *Proct.* 91-96
- Rolleston, J. D.**—Hereditary tylosis, *Child.* 24
- Roth, Paul B.**—Case of ocular torticollis, *Orth.* 46
of osteitis deformans, *Orth.* 49
of renal dwarfism shown after operation for genu valgum, *Orth.* 45
injury to epiphysis of left acromion process, *Orth.* 14
two cases of Kohler's disease, *Orth.* 28
- Row, R.**—Discussion on occurrence of Leishmania in Indian kala-azar, *Trop.* 7, 8
- Rowbotham, E. S.**—Case of cardiac arrest under anæsthetic followed by heart massage, *Anæsth.* 5
- Rowlands, R. P.**—Case of partial gastrectomy for cancer of stomach, *Clin.* 2
- Rowntree, C.**—Discussion on urgent need for education on control of cancer, *Occ. Lect.* 37
two cases of sarcoma of small intestine, *Surg.* 85
- Royal Air Force**, use of physiological tests in, *War* 39
- Ruminants**, effects of morphine upon, *Therap.* 45
- Ruthin, Lordship of**, course of Pestilence of 1349 in, table and chart showing, *Hist.* 31
- Ryland, A.**—Case of absolute bilateral deafness, with almost complete loss of vestibular activity, *Otol.* 7
of pharyngeal pouch, *Laryng.* 41
discussion on case of vertigo cured by opening external semicircular canal, *Otol.* 60
skiagrams showing simple fibrous strictures of œsophagus in child, *Laryng.* 42
- Ryle, J. A.**—Case of erythremia (polycythæmia vera, Vaquez-Osler's disease) with cerebral hæmorrhage, *Med.* 83
- Sacculus** and ventricle, difference between, in horse and man, *Laryng.* 34
- Sakheim, J.**—Hunterian glossitis in pernicious anæmia (quoted), *Med.* 7
- Salicylic acid**, in great strength, in treatment of skin diseases, cases, *Derm.* 109, 110
- Salivary calculus**, large, submaxillary gland containing (Dan McKenzie), *Laryng.* 7
- Salmond, R. W. A., O.B.E.**—Artificial pneumopericardium, case, *Electr.* 71-74
- Sambon, L.**—Discussion on cases of pellagra, *Child.* 61, 62
tropical and sub-tropical diseases (quoted), *Epid.* 44, 45
- Sand-fly fever** in Malta (Squadron-leader H. E. Whittingham), *War* 1-14
areas most abounding in, *War* 2
diagnosis by agglutination tests, *War* 3, 5
by blood cultures, *War* 2, 3, 5
by urine examination, *War* 3, 8
by Wassermann tests, *War* 3, 5
differential, *War* 2, 5
difficulties in, *War* 1
experiments with leptospira isolated, *War* 11, 12
influenza and trench fever, temperature and leucocyte curves compared, *War* 8
isolation of causal virus by direct blood culture, *War* 3, 4, 9
by inoculation of guinea-pigs with whole blood, *War* 3, 5, 10
leptospiræ isolated from cases of, *War* 9, 10
leucocyte count in typical case of, *War* 7
references, *War* 13, 14
- Sand-fly, *Phlebotomus papatasi***, life history of (H. E. Whittingham), *Trop.* 45
- Sandstone masons**, mortality from phthisis per 1,000 living at various age periods, *Epid.* 90
- Sandwith, F. M.**—Views on ulcerative colitis and dysentery (quoted), *Proct.* 104
- Sarcoid**, Boeck's (J. L. Bunch), *Derm.* 73
- Sarcoma**, (?) idiopathic hæmorrhagic, of Kapos (H. MacCormac), *Derm.* 61
in ovarian dermoid tumour, case (H. R. Spencer), *Obst.* 101-105
macroscopical and microscopical appearances, *Obst.* 102-105
myeloid, of posterior pillar of fauces, case (N. Patterson), *Laryng.* 13
of ileum, case (C. Rowntree), *Surg.* 85
of left tonsil, case (Sir W. Milligan), *Laryng.* 83

- Sarcoma** of lungs and mediastinum, instantaneous radiogram showing, *Electr.* 16
 of maxilla and malar and frontal bones, case (F. H. Westmacott), *Laryng.* 92
 of nose, cured by radium, case (E. M. Woodman), *Laryng.* 49
 of right tonsil and surrounding faucial region, case (Sir W. Milligan and F. Wrigley), *Laryng.* 91
 of small intestine, two cases of (C. Rowntree), *Surg.* 85
 tonsillar region treated by X-rays after partial removal, case (Sir J. Dundas-Grant and Dan McKenzie), *Laryng.* 69
 of uterus, two specimens of (J. D. Barris), *Obst.* 65-67
 spindle-celled, large, arising in mesentery of coil of ileum, successfully removed by operation, case (F. Kidd), *Surg.* 86
Sarcomatous cyst of jejunum, case (C. Rowntree), *Surg.* 86
Sarcoptes canis, comparison with human acarus, *Derm.* 76
Savatard, L. A.—Case of multiple epidermoid cysts, *Derm.* 94
 of ulcer rodens erythematoides, *Derm.* 65
 of xanthoma (? diabeticorum), *Derm.* 93
 discussion on cases of multiple superficial rodent ulcer, *Derm.* 26
 on pigmented lesion for diagnosis, *Derm.* 60
 epithelioma adenoides cysticum, *Derm.* 30-46
Savill, T. D.—Description of Graves' disease (quoted), *Obst.* 94
Scalp, folliculitis of, peculiar case of (A. Castellani), *Derm.* 97
Scarlet fever, admissions and deaths under Metropolitan Asylums Board in 1914, *Epid.* 19
 age and sex distribution of (F. M. Turner), *Epid.* 19-30
 and rainfall, relationship between (J. Brownlee), *Epid.* 30-34
 blindness due to, *Epid.* 61
 cases admitted to Metropolitan Asylums Board Hospitals (both sexes), age-distribution per 1,000 persons, *Epid.* 26
 diminished virulence of, *Med.* 47
 epidemic prevalence, rise in mean age during, *Epid.* 27, 29
 high prevalence and high mean ages, significance of, *Epid.* 23
 in Manchester, cases notified and age-tables of, 1891-1921, *Epid.* 27, 28
 low prevalence and low mean ages, significance of, *Epid.* 23
 measles and diphtheria, triple infection, necrosis of left temporal bone involving facial nerve and labyrinth in case of, *Otol.* 29
 mortality in 1880 and 1920 compared, *Med.* 47
 notification greater for females than for males, proportions of, *Epid.* 25
 number of cases and mean age in London, 1887-1920, *Epid.* 20, 21
 otitis media with facial palsy following, case, specimens (malleus and incus) shown (F. J. Cleminson), *Otol.* 17
Scarring, abnormal, after chicken-pox, case (R. T. Smith), *Derm.* 82
 (gas-burn), case of (H. C. Semon), *Derm.* 95
Scheube, views on yaws quoted, *Trop.* 33
Schistosoma bovis and its snail carrier, specimen of (J. B. Christopherson), *Trop.* 56
mansoni, intermediate host of, specimen of (J. B. Christopherson), *Trop.* 56
Schmidt, L.—Discussion on vagus and sympathetic nerves and their relation to climate and hydrology, *Baln.* 15
School children, examination of, for surface disease of eyes, bacteriological results, *Epid.* 53, 54
 rarity of conjunctivitis due to Morax-Axenfeld bacillus among, *Epid.* 57
Schools, Council, standard of vision for scholars and teachers in (abstract) (N. B. Harman), *Ophth.* 7
Sciatica, parathyroid medication in, *Therap.* 20
 in woman suffering from lead poisoning (B. Myers), *Clin.* 7
Scientific progress, effect on war, *War* 32
Scleritis, tubercular, *Ophth.* 6
Sclerodactylia, early, and Raynaud symptoms, associated with disseminated lupus erythematosus, case (G. Pernet), *Derm.* 91
Scleroderma, abating after extraction of teeth, case-record, *Odont.* 28
 cases of (J. Collier), *Neur.* 30; (Haldin Davis), *Derm.* 29; (M. G. Hannay), *Derm.* 60; (T. Grainger Stewart), *Neur.* 31
 generalized, with subcutaneous nodules, case (A. M. H. Gray), *Derm.* 107
 very extensive case of (E. G. Graham-Little), *Derm.* 64
Sclerosis, disseminated, cases of (J. Taylor), *Neur.* 48; (W. A. Turner), *Neur.* 52
 with retraction of eyelids, case (J. Collier), *Neur.* 47
 of spinal cord, anæmia with, blood changes in, *Med.* 36
 blood changes in, compared with those presented in cases of combined sclerosis, *Med.* 33, 34
 in case of pernicious anæmia, *Med.* 30
 or spinal compression, case for diagnosis (F. M. R. Walshe), *Neur.* 48
Scorbutic infantilism, case (M. Cassidy), *Clin.* 16
Scotland, gonorrhœa in, incidence of, *Epid.* 81, 82
 illegitimacy in, incidence of, *Epid.* 81
 pellagra in, *Child.* 42
 syphilis in, incidence of, *Epid.* 81, 82
 venereal disease in, incidence of (T. F. Dewar), (abstract), *Epid.* 81-84
 Registrar-General's reports quoted, *Epid.* 81, 82
Scott, S.—Cerebellar abscess, five weeks after onset of acute otitis media, right side, *Otol.* 57
 cerebellar abscess, sudden coma and apnoea, recovery after operation during artificial respiration, *Otol.* 56
 discussion on case of myasthenia gravis in which throat symptoms were an early sign, *Laryng.* 18
 on case of tinnitus associated with facial spasm, *Otol.* 8
 on eighth nerve tumours, *Otol.* 41
 on labyrinthitis as complication of middle ear suppuration, *Otol.* 15
 on otosclerosis and osteitis deformans, *Otol.* 27

- Scott, S.**—Left temporo-sphenoidal abscess, amnesia for names of objects, *Otol.* 55
ossification of incus to tegmen, *Otol.* 20
- Scott, S. Gilbert.**—Method for opaque meal examination of stomach (abstract), *Electr.* 35-41
- Scottish Board of Health** regulations as to carriers of infection, *Epid.* 9, 10
- Scurvy**, dental sepsis in relation to, *Odont.* 16
- Sea anemone**, nerve network of, conduction with a decrement in, *Neur.* 57
- Seasonal** incidence and variations of glossitic hæmolytic anæmia, individuality as shown by, *Med.* 26
records of twenty-two cases, *Med.* 27, 28
- Secretions**, internal, relationship of emotion to, *Psych.* 25
- Semon, H. C.**—Case for diagnosis, *Derm.* 15
of adenoma sebaceum, *Derm.* 53
of gas-burn scarring, *Derm.* 95
of unusual localization of ichthyosis, *Derm.* 94
outbreak of alopecia (two cases), *Derm.* 100
pigmented lesion for diagnosis, *Derm.* 59
two cases of ringworm of the nails in sisters, *Derm.* 1
xantho-erythrodermia perstans (Crocker) case, *Derm.* 103
- Sensori-motor** reaction times, measuring of, new apparatus for (M. D. Hart and W. W. Smith), *Electr.* 63-69
measuring of, significance of degree of variability, *Electr.* 69
requirements necessary, *Electr.* 63, 64
use of, and information derived from, *Electr.* 68
- Sepsis**, cases of, treated by manganese, *Derm.* 69
dental, see under *Dental sepsis*
in Cæsarean section, *Obst.* 51
underlying factor in diseases improved by thyroid medication, *Therap.* 23
- Septic** complications in case of pernicious anæmia, *Med.* 30
focus, slightly purulent, latent, danger of, *Odont.* 38
- Septicæmia**, coccal, passive transference of immune serum in, *Path.* 2
multiple neuritis due to, *Neur.* 16
pneumococcal, and enlargement of liver and spleen, case (R. C. Jewesbury), *Child.* 26
streptococcal, acute, dental sepsis causing, *Odont.* 12
- Septum** nasi, papilloma of (H. L. Whale), *Laryng.* 12
- Sequeira, J. H.**—Case of bullous eruption, *Derm.* 55
of congenital onychogryphosis, *Derm.* 92
of xanthoma diabeticorum, *Derm.* 30
showing results of treatment by trepol, *Derm.* 21
discussion on (?) case of idiopathic hæmorrhagic sarcoma of Kaposi, *Derm.* 62
on pigmented lesion for diagnosis, *Derm.* 60
two cases illustrating benefit of light baths in tuberculous disease of skin, *Derm.* 63
two cases of angiomatous granuloma (multiple idiopathic pigment sarcoma of Kaposi), *Derm.* 76
two cases of multiple carcinoma, *Derm.* 23, 24
- Serous** effusions, instrument for withdrawing, (H. O. Gunewardene and B. W. Cantrell), *Clin.* 38, 39
- Serratus magnus**, isolated unilateral paralysis of, association with lower portion of trapezius muscles, *Neur.* 8
- Serum**, anti-dysenteric, in treatment of ulcerative colitis, *Proct.* 107
immune, bacteriolysin in, *Path.* 4
passive transference of, in anthrax, *Path.* 2
in coccal septicæmias, *Path.* 2
salvarsanized, injection into cisterna magna in treatment of general paralysis, *Neur.* 69-72
- Sewell, D. Lindley.**—Case of abductor paralysis of left vocal cord, *Laryng.* 96
of epithelioma of anterior pillar of fauces and base of tongue, *Laryng.* 96
of extensive tuberculous disease of pharynx and larynx, *Laryng.* 96
of lupus of anterior end of inferior turbinal, *Laryng.* 96
clinical note on after-treatment of empyema of maxillary antrum (Denker's operation) (abstract), *Laryng.* 85
and **Milligan, Sir W.**—Suppurative disease of left frontal sinus and left maxillary antrum, case, *Laryng.* 90
tuberculous growth in left naris, case, *Laryng.* 90
- Sex** and age distribution in scarlet fever (F. M. Turner), *Epid.* 19-30
- Sexual** development, endocrine glands directing, *Psych.* 24
- Shattock, C. E.**—Case of multiple exostosis and hip disease, *Clin.* 2
- Shattock, S. G., F.R.S.**—Discussion on case of pharyngeal pouch, *Laryng.* 41
on operative procedures for bilateral abductor paralysis, *Laryng.* 39
disruptive phenomena in gunshot injuries, their physics, *Path.* 17-34
report on case of obstruction of trachea due to arrest in development, *Laryng.* 58
report on specimen of large cyst of orifice of larynx arising from arytaeno-epiglottidean fold, *Laryng.* 70
- Shaw, E. H.**—Demonstration on the immediate microscopic diagnosis of tumours at the time of operation, *Surg.* 85
- Shaw, H. Batty.**—Discussion on significance of vascular and other changes in retina in arterio-sclerosis and renal disease, *Med. & Ophth.* 1, 33
- Shipway, F. E.**—Discussion on anæsthesia in dental surgery, *Anæsth.* 21
discussion on anæsthetization in Cæsarean section, *Anæsth.* 4
- Shoemakers**, mortality from phthisis per 1,000 living at various age-periods, *Epid.* 90
- Shore, T. G.**—Report on liver in case of persistent jaundice in infant, *Child.* 17
- Shoulder-muscles**, right side, congenital absence of, case (H. T. Gray), *Child.* 43
- Shrubsall, F. C.**—Discussion on birth injuries, *Child.* 76
discussion on late effects of encephalitis lethargica, *Child.* 35
on standards of vision for scholars and teachers, *Ophth.* 8

- Sibley, W. K.**—Case for diagnosis, (?) leukæmia cutis, *Derm.* 12
 of acne varioliformis, *Derm.* 108
 discussion on case of folliculitis ulerythematosia reticulata, *Derm.* 82
 of lupus vulgaris treated with potassium iodide, *Derm.* 84
 of trichorrhæxis nodosa, *Derm.* 74
 treatment of ringworm of nails by ionization advocated, *Derm.* 2
- Sigmoid** flexure, plication of, partial relief of Hirschsprung's disease following (W. G. Spencer), *Clin.* 31
- Sigmoidoscope** in diagnosis of amœbic and bacillary dysentery, *Proct.* 107
 value of, in diagnosis of ulcerative colitis, *Proct.* 94, 98, 106, 108
- Silicosis**, tuberculous, low infectivity of, *Epid.* 99
- Silk** as suture-material in Cæsarean section, *Obst.* 53
 worm disease, Pasteur's investigations on, *Occ. Lect.* 12, 13
- Silvester-Bradley, C. R. Lieut.-Col.**—Discussion on physical training, *War* 39
- Singer's** nodes, hoarseness due to, case (Sir J. Dundas-Grant), *Laryng.* 44
- Sinus**, nasal, disease of, acute in young children (E. Watson-Williams), *Child.* 81-84
 diagnosis of, *Child.* 83
 frequency of, *Child.* 82
 references, *Child.* 84
 treatment of, *Child.* 83, 84
 two severe cases of, *Child.* 82
 maxillary, in children, anatomy of, *Child.* 82
- Sinuses** and cavities of nose, evolution of, in relation to function (J. F. O'Malley), *Laryng.* 83, 84
- Sinusitis**, atrial, chronic, latent, *Odont.* 37
 ethmoidal or frontal, dental neuralgia in reference to, *Odont.* 35
 halisteresis and absorption of bone in, *Odont.* 39
 of dental origin, conditions giving rise to, *Odont.* 40
 nasal, in infants, *Odont.* 37, 41
 latent, *Odont.* 40
 signs and diagnosis of, *Odont.* 38
- Skin**, asthma, anaphylaxis and, relationship between, *Therap.* 4
 diphtheria of (?) case of, for diagnosis (E. G. Graham Little), *Derm.* 86
 diseases of, dental sepsis in relation to, *Odont.* 14, 27, 28
 new method of treating (Sir G. Archdall Reid), *Derm.* 109
 excretion of toxins by, causing rashes and pruritus, *Urol.* 19
 leishmaniasis of, case (J. B. Christopherson), *Derm.* 8
 resembling lupus vulgaris (J. B. Christopherson), *Derm.* 48
 pigmentation of, endocrine function in relation to, *Baln.* 13
 resistance, diminution of, in response to noxious stimulus, *Psych.* 5
 smooth, favus of, two cases (E. G. G. Little), *Derm.* 51
 tuberculous disease of, benefit of light baths in, two cases illustrating (J. H. Sequeira), *Derm.* 63
- Skull**, fractures of, punctured, disruptive phenomena of, *Path.* 25
 gunshot injuries in, disruptive phenomena of, *Path.* 21, 22
- Sleep**, importance of, in regard to teeth of children, *Odont.* 5
- Smalley, A. A.**—Carcinoma of antrum; removal of upper jaw, *Laryng.* 94
 double abductor paralysis, case, *Laryng.* 94
 endothelioma of ethmoid; lateral rhinotomy, case, *Laryng.* 94
 nasopharyngeal tumour: (?) a cystic adenoma, containing cartilage (wet specimen and section), *Laryng.* 94
- Small-pox**, mortality from, in 1572, *Occ. Lect.* 4
 in later eighteenth century, *Occ. Lect.* 4
 in 1880 and 1920 compared, *Med.* 47
- Smith, A. Laphorn.**—Discussion on case of calcified tumour of recto-vaginal septum, *Obst.* 81
 discussion on gonorrhœal stricture of rectum, *Proct.* 19
 on myomectomy for uterine fibroids, *Obst.* 24
 on treatment of uterine hæmorrhage by radium, *Obst.* 79
- Smith, Boylan, Lieut.-Col.**—Discussion on physical training, *War* 38
- Smith, E. Bellingham.**—Discussion on birth injuries, *Child.* 77
 discussion on case for diagnosis, *Child.* 21, 55
 on case of cerebral degeneration, *Child.* 16
 on case of patent ductus arteriosus and mitral disease, *Child.* 49
 on late effects of encephalitis lethargica, *Child.* 40
- Smith, L.**—Discussion on anæsthetization in Cæsarean section, *Anæsth.* 2
- Smith, R. T.**—Abnormal scarring after chicken-pox, *Derm.* 82
- Smith, W. W. and Hart, M. D.**—New apparatus for measuring sensori-motor reaction times, *Electr.* 63-69
- Smurthwaite, H.**—Discussion on case of extensive lupus of palate, pharynx and larynx, *Laryng.* 52
 discussion on case of multiple papillomata of larynx, *Laryng.* 46
 on treatment of large foreign bodies impacted in gullet, *Laryng.* 80
 foreign body removed from trachea of child aged 6 months, *Laryng.* 66
 laryngeal case for diagnosis, *Laryng.* 31
- Sodium** bromide, injection of, resulting in breaking of two shadows into multiple shadows, pyelogram illustrating (W. Girling Ball), *Urol.* 85
- Soil** infection in anthrax, *Med.* 53
- Somervell, T. H.**—Discussion on medical aspects of life at high altitudes, *Med.* 59
- Somme, Henry V's** campaign of, in 1415, medical services of (G. E. Gask), *Hist.* 1-10
- Sonntag, C. F.**—Discussion on vagus and sympathetic nerves and their relation to climate and hydrology, *Baln.* 7
- "Sore tongue"** of pernicious anæmia, *Med.* 6, 7
- Souttar, H. S., C.B.E.**—Cases of duodenal ulcer to illustrate certain points in diagnosis, *Clin.* 5, 6

- Souttar, H. S., C.B.E.**—Tumour removed from brain of child aged 12, *Clin.* 27
- Spastic** contracture, physiological aspects of, *Orth.* 34
- paralysis, chief causes of, *Orth.* 33
- dementia and complete amaurosis in Hebrew boy aged 10 (G. Riddoch), *Neur.* 29
- operative treatment of, discussion on, *Orth.* 33-42
- by Foerster's operation, *Orth.* 34
- by Stoffel's operation, *Orth.* 35
- Spencer, H. R.**—Adenoma of vaginal fornix, simulating cancer of cervix, *Obst.* 27
- anaesthetization of patients for classical Cæsarean section, *Anæsth.* 1
- discussion on adenomyomata of female pelvic organs, *Obst.* 90
- on Cæsarean section, *Obst.* 59
- on case of inversion of uterus, *Obst.* 49
- of large fibroid of cervix developing after subtotal hysterectomy, *Obst.* 13
- on myomectomy for uterine fibroids, *Obst.* 23
- on urgent need for education in control of cancer, *Occ. Lect.* 36
- on value of ergot, *Obst. & Therap.* 5
- ruptured unilateral solid cancer of ovary, ovariectomy, no recurrence six years later, *Obst.* 105
- sarcoma in ovarian dermoid tumour, case, *Obst.* 101-105
- stretching of epithelium of tubal rugæ by blood effused into them in torsion of pedicle of ovarian tumour, *Obst.* 106
- Spencer, W. G.**—Hirschsprung's disease, partial relief following plication of sigmoid flexure, *Clin.* 31
- Spicer, F.**—Swelling on posterior wall of pharynx, *Laryng.* 57
- Spilsbury, Sir B.**—Heart conditions found, post-mortem among fatalities connected with anaesthesia, *Anæsth.* 30
- report on specimen of sarcoma of uterus, *Obst.* 66
- Spinal** anaesthesia with tropacocaine, Cæsarean section under, two cases (B. W. Whitehouse and H. Featherstone), *Obst.* 55-58
- cord, lesions of, in pernicious anæmia, features of, *Med.* 42
- sclerosis of, anæmia with, blood changes in, *Med.* 36
- blood changes in, compared with those presented in cases of pernicious anæmia, *Med.* 33, 34
- in pernicious anæmia, features of, cases, *Med.* 11, 22, 23, 30
- in severe anæmia, *Med.* 10, 11, 23
- sclerosis of, subacute combined, special connexion with pernicious anæmia considered, *Med.* 25
- corset, wearing of, in treatment of traumatic spondylitis, *Orth.* 7
- nerve roots, posterior, sensory fibres of, degeneration of, *Baln.* 12, 15
- inflammatory effect of mustard oil applied to skin, not present after, *Baln.* 12
- separation from spinal cord, by section, effect, *Baln.* 12
- Spine**, compression of, or disseminated sclerosis, case for diagnosis (F. M. R. Walshe), *Neur.* 48
- fracture-dislocation of, *Orth.* 2
- see also *Spondylitis*, traumatic
- osteo-arthritis of (C. Goulesbrough), *Med.* 63-70
- frequency of, *Med.* 63
- types of, *Med.* 64
- X-ray pathology of, *Med.* 67
- X-ray appearances of, advantages of use of Potter-Bucky diaphragm, *Med.* 69
- Spirochæta eurygrata* and *Spirochæta stenogyrata*, comparison of, *Trop.* 46, 47
- Spirochætal** dysentery, case of (W. Broughton-Alcock), *Trop.* 46
- Spirochæte**, special form of, as cause of neurosyphilis, possibility considered, *Neur.* 66
- Spleen** and liver, enlargement of, with pneumococcal septicæmia, case (R. C. Jewsbury), *Child.* 26
- rare case of congenital non-familial jaundice without enlargement of, in otherwise healthy man aged 56 (F. Parkes Weber), *Med.* 81-83
- enlargement of, with anæmia accompanying congenital family cholæmia, *Med.* 80
- increased size of, in Indian kala-azar, *Trop.* 1.
- Splenectomy** in treatment of hæmolytic jaundice, *Med.* 77
- Splenic** fever in cattle and horses, *Med.* 52
- Splenomegaly**, chronic, of uncertain origin, case of (F. Parkes Weber), *Child.* 64
- Spondylitis**, traumatic (T. H. Openshaw), *Orth.* 1-10
- injuries in cervical region, *Orth.* 2
- methods of treatment, *Orth.* 3
- in dorsal and lumbar regions, diagnostic difficulties, *Orth.* 3, 7
- prognosis of, *Orth.* 10
- quadriplegia with, case (D. McAlpine), *Neur.* 83
- simulation by malingerers, *Orth.* 4
- treatment of, *Orth.* 9, 10
- by wearing of spinal corset, *Orth.* 7
- X-ray appearances in cases of, *Orth.* 4-10
- Spondylose rhizomélisque**, case (J. Collier), *Neur.* 47
- Sponge**, swallowed during anaesthesia in dental surgery, case, *Anæsth.* 19
- Sponges**, etherized, use in dental surgery, *Anæsth.* 15, 16
- Sprawson, E., M.C.**—Discussion on case of multiple dentigerous cysts, *Odont.* 47
- vascular supply of enamel organ of *Felis domestica*, *Odont.* 47-54
- Stanley, Hon. Sir Arthur.**—Discussion on urgent need for education in control of cancer, *Occ. Lect.* 36
- Stannus, H. S.**—Case of (?) pellagra, *Derm.* 27
- discussion on cases of pellagra, *Child.* 64
- on cases of splenomegaly, *Child.* 67
- Staphylococcal** infection in dental sepsis, *Odont.* 8
- Steadman, F. St. J.**—Discussion on anaesthesia in dental surgery, *Anæsth.* 22
- Steatorrhœa**, congenital, case (D. Paterson), *Child.* 27
- Stereoscopic** sense, loss of, early sign of visual fatigue, *War* 51
- Sterilization**, separation of carbohydrates from proteins during, new fermentation tube for (C. Dukes), *Path.* 13-16

- Stethoscope**, introduction of, *Electr.* 2
- Stevens**, T. G.—Squamous epithelioma of vagina, *Obst.* 26
- Stewart**, T. Grainger.—Case of scleroderma, *Neur.* 31
- Still's disease**, specimen of (C. B. Dansie), *Child.* 43
- Stimulants**, decreasing use of, for medical purposes, *Med.* 48
- Stimulus**, response to, time relations of, *Psych.* 6
verbal, forms of, *Psych.* 5
- Stoddart**, W. H. B.—Discussion on genius and insanity, *Psych.* 37
- Stoffel's operation** for spastic contracture, *Orth.* 35
- Stolkind**, E.—Case of acromegaly in girl, aged 16, with congenital heart disease (aortic stenosis), *Clin.* 22
of adiposis dolorosa (Dercum's disease), *Clin.* 45
of Graves' (Parry-Graves-Basedow) disease in woman, aged 69, without goitre, *Clin.* 44
of post-encephalitic paralysis agitans, *Clin.* 47-49
two cases of erythremia (Vaquez disease) treated by Röntgen-therapy, *Clin.* 35-38
- Stomach**, antrum of, pressure upon, of pathological gall-bladder containing stones, X-ray appearances, *Electr.* 88
pressure upon, due to pathological gall-bladder, *Electr.* 79
spasm of, in diagnosis of pathological gall-bladder, *Electr.* 81
cancer of, partial gastrectomy for, case (R. P. Rowlands), *Clin.* 2
diseases of (non-malignant), mortality from, 1901-1920 statistics, *Surg.* 8
lesions of, diagnosis, value of radiographs in, *Electr.* 36
mucosa and submucosa of, lesions of, in case of pernicious anemia, *Med.* 13
normal, radiograms of, comparing folds of mucous membrane, *Electr.* 92
opaque meal-examination of, method for (abstract, (S. G. Scott), *Electr.* 35-41
apparatus for, *Electr.* 39
clinical history, *Electr.* 36
composition of meal, *Electr.* 37
examination essential, *Electr.* 38
importance of concentration of attention on, *Electr.* 38
method of recording statistics, *Electr.* 37
number of examinations, *Electr.* 38
position of patient, *Electr.* 39-41
rapidity of method, *Electr.* 37
stages in, described, *Electr.* 40
systematic radioscopic palpation, *Electr.* 36
technique, *Electr.* 37
sore, in pernicious anemia, *Med.* 7
- Stone** in diverticulum complicating vesical diverticula, operative treatment in, *Urol.* 67
see also *Calculus*, *Calculi*, *Gall-stone*
- Stowers**, J. H.—Discussion on case of dermatitis repens, *Derm.* 99
- Straus's levulose test** in case of enlarged liver with persistent acetonuria and diaceturia, *Child.* 58
- Streptococcal infection** of colon, of dental origin, *Odont.* 11
- Streptococcal toxæmia**, other foci of infection than teeth or gums, *Odont.* 10
- Streptococci** found in dental infections, varieties of, *Odont.* 8
kinds of, found in mouth, *Odont.* 17, 18
Streptococcus salivarius, *Odont.* 18, 19
- Strickland - Goodall**, J.—Discussion on blood pressure, *Baln.* 5
- Strychnine**, effects of, upon animals and man compared, *Therap.* 47
- Stuart-Low**, W.—Discussion on case of multiple foci of growth in palate and tonsil, *Laryng.* 19
discussion on case of œdema of septum in association with nasal polypi, *Laryng.* 27
on infections of teeth and gums in their relationship to nose, throat, and ear, *Odont.* 41
- Subcapsular pyelotomy**, see *Pyelotomy*, subcapsular
- Subglottic stenosis**, complete laryngostomy for, case (W. Howarth), *Laryng.* 48
- Subhyaloid hæmorrhage** in a girl, case (M. S. Mayou), *Ophth.* 31
- Subluxation** of inner end of right clavicle (P. M. Heath), *Orth.* 12
- Submaxillary gland** containing large salivary calculus (Dan McKenzie), *Laryng.* 7
with calculi (H. B. Tawse), *Laryng.* 22
- Suggestibility**, McDougall's definition of (quoted), *Psych.* 7
- Suggestion**, blistering effects of burn, prevented by, *Baln.* 15
- Sulpho-conjugation test** in case of enlarged liver with persistent acetonuria and diaceturia, *Child.* 58
- Sulphonal** and derivatives, poisoning by, acute hæmatoporphyria following, *Neur.* 25
- Sulphuric acid**, accidental swallowing of, by patient with syphilitic laryngitis, case showing results of (C. Gill-Carey), *Laryng.* 67
- Summa Perfectionis* of "Geber," Jābir ibn Hāyan possible author of, *Hist.* 47
- Sunlight treatment** of tuberculosis of larynx, demonstration of instrument for (Sir J. Dundas Grant), *Laryng.* 12
- Suprarenal gland medication** in organotherapy, *Therap.* 16
- Surgery**, changes in, due to bacteriology, *Med.* 44
effects of late war upon, *Surg.* 9
progress of, and rise and fall of surgical operations (J. Berry), *Surg.* 1-11
- Surgical malpraxis**, alleged, early case of (1424), *Hist.* 8
registrar, post of, value of, to young surgeons, *Surg.* 4
- Sutcliffe**, W. G.—Two large calculi removed from perineum of male, aged 62, in Margate Cottage Hospital, *Urol.* 36
- Sutherland**, G. A.—Discussion on cases for diagnosis, *Child.* 19, 21
- Swan**, R. H. Jocelyn.—Discussion on subcapsular pyelotomy in treatment of renal calculi, *Surg.* 41
incidence of malignant disease in the apparently benign enlargement of prostate, *Urol.* 71-77

- Swan, R. H. Jocelyn.**—Multiple cystic formation in lower pole of kidney, *Urol.* 41
prostate removed by prostatectomy, weight 12 oz. or 340 grm., *Urol.* 42
pyonephrosis due to kinking of ureter by aberrant renal vessels, *Urol.* 41
- Swine fever**, vaccine for, Pasteur's discovery of, *Occ. Lect.* 13
- Sydenham, F., and McKenzie, Dan.**—Epidemic cerebro-spinal meningitis associated with acute suppuration of middle ear, case, *Otol.* 51
- Sylvester-Bradley, Lieut.-Col. C. R.**—Discussion on physical training, *War* 39
- Symbiosis** in relation to dental sepsis, *Odont.* 10
- Syme, W. S.**—Discussion on case of epithelioma of soft palate, *Laryng.* 88
discussion on skiagrams showing fibrous strictures of oesophagus in child, *Laryng.* 42
on treatment of malignant disease in upper jaw, *Laryng.* 87
- Symonds, Sir C., K.B.E.**—Gonorrhoeal stricture of rectum, *Proct.* 12-19
nephrostomy for relief of inoperable rectovesical fistula, *Proct.* 90
- Symonds, C. P.**—Case of encephalitis lethargica, showing late results, *Child.* 32
case of muscular atrophy of "peroneal" type apparently commencing in, and confined for some time to, the right hand, *Neur.* 90
of myasthenia gravis in which throat symptoms were an early sign, *Laryng.* 17
of unilateral affection of cranial nerves, 7, 9, 10, 11 and 12, *Neur.* 52
of unilateral affection of cranial nerves, 9-12 (Tapia's syndrome) associated with chronic otitis media, *Neur.* 53
discussion on case of disseminated sclerosis, *Neur.* 52
on late effects of encephalitis lethargica, *Child.* 38, 40
on treatment of neuro-syphilis, *Neur.* 74
note on nervous system in case of erythraemia, *Med.* 84
- Sympathetic and vagus nerves**, and their relation to climate and hydrology, discussion on, *Baln.* 7-17
afferent impulses to nerve centres through, routes for, *Baln.* 8
in man and lower animals compared, *Baln.* 8, 9, 10
nerve and endocrine system, balance of, asthma in relation to, *Therap.* 3
ganglia of, in man and mammals, compared, *Baln.* 10
relation of hyperthermal mud baths to, *Baln.* 15
nervous system, same effect on, produced by physical or psychical cause, *Baln.* 15
- Syncope** (chloroform), *Anaesth.* 30, 31
- Syndactyly**, unusual form of (H. A. T. Fairbank), *Orth.* 29
- Syngamus trachealis*, life history of, *Trop.* 44
- Synostosis** of phalangeal joints, specimen of, (?) congenital in origin (W. H. Ogilvie), *Orth.* 51
- Syphilis**, cases of, infected with yaws, *Trop.* 40
causing polyneuritis, *Neur.* 17
congenital, cases of (D. Nabarro), *Child.* 42; (J. H. Thursfield), *Child.* 41; (R. H. Norgate), *Child.* 80
early and late, effects of intensive treatment on Wassermann reaction, *Neur.* 65, 66
in India, incidence, *Trop.* 15
in man, case (W. Fox), *Derm.* 16
in Scotland, incidence of, *Epid.* 81, 82
inoculated in patients suffering from yaws, *Trop.* 40
introduction of, into Europe, *War* 15
lichen planus and, case (S. E. Dore), *Derm.* 18
of internal ears, complete nerve-deafness due to: caloric and rotation tests negative, galvanic positive, case (Sir J. Dundas-Grant), *Otol.* 16
quinidine administration contra-indicated in old subjects of, with cardiac trouble, *Therap.* 41
secondary, results of treatment by trepol, case showing (J. H. Sequeira), *Derm.* 21
tertiary, thyroidal hormones in, *Therap.* 15
treatment of, at Middlesex Hospital, details of, *Neur.* 65
Wassermann reaction of cerebro-spinal fluid before and after treatment in series of cases, *Neur.* 67
- Syngomyella**, case of (S. A. K. Wilson), *Neur.* 49
showing pain of central origin, case (A. G. Duncan), *Neur.* 83
with much sensory and motor impairment and little wasting, case (C. M. Hinds Howell), *Neur.* 50
- Tabes dorsalis**, early, case of (J. P. Lockhart-Mummery), *Proct.* 90
early diagnosis combined with continued anti-syphilitic treatment, favourable results of, *Neur.* 63
mesenterica, incidence of, statistics, *Urol.* 1
- Tabo-paresis**, "juvenile," case of (C. Worster-Drought), *Neur.* 81
- Tachycardia**, paroxysmal, treatment by administration of quinidine, *Therap.* 39, 42
- Tailors**, mortality from phthisis per 1,000 living at various age periods, *Epid.* 90
- Talipes**, tendon transplantation for (E. Laming Evans), *Orth.* 14
- Tapia's syndrome** (unilateral affection of cranial nerves 9 to 12), associated with chronic otitis media, case (C. P. Symonds), *Neur.* 53
- Tartar emetic**, tolerance to, in animals, idiosyncrasies of, *Therap.* 43
- Tartaric acid**, Pasteur's researches on, *Occ. Lect.* 16, 17, 18
- Tawse, H. B.**—Case for diagnosis, *Laryng.* 21
submaxillary gland with calculi, *Laryng.* 22
thyroid tumour from base of tongue, *Laryng.* 21
ulceration of left tonsil, case for diagnosis, *Laryng.* 70-72
- Taylor, J.**—Case of disseminated sclerosis, *Neur.* 48
discussion on cases of congenital neurosyphilis in brother and sister, *Neur.* 50
on dystrophia myotonica, *Neur.* 43

- Tay-Sachs** disease, case of (L. Mandel), *Child.* 55
- Teeth** and gums, clinical macroscopical signs of unhealthy condition of, *Odont.* 10
- infections of, in their relationship to nose, throat and ear, discussion on, *Odont.* 35-42
- and jaws in puppies, effects on, of diet sufficient in amount but deficient in quality, experiments showing, *Odont.* 75, 76
- apices and roots of, granuloma about, *Odont.* 25, 31
- calcification of, influence of ductless glands on, *Odont.* 3, 4
- caries of, factors in, causation of, *Odont.* 3
- in relation to structure, *Odont.* 76, 77
- congenital absence of all except two, case (A. T. Pitts), *Child.* 45
- in three members of a family, case (A. T. Pitts), *Child.* 44
- crowning, careless and improper, *Odont.* 7, 30, 33
- dead, apical infection in, treatment, *Odont.* 23
- deciduous, imperfect structure influencing progress of caries, *Odont.* 76, 77
- effect of antiscorbutic vitamin on, *Odont.* 76
- eruption of, diet before and after, effects of, in relation to caries, *Odont.* 77
- extraction of, causing disappearance of migraine complicating pyorrhoea, *Odont.* 24
- in dental sepsis, and its complications, *Odont.* 7, 17, 23, 26-32
- skin diseases abating after, *Odont.* 28
- wholesale, not sole cure of dental sepsis, *Odont.* 7, 18
- Hutchinsonian, case of (A. T. Pitts), *Child.* 45
- influence of habits, mode of life and environment upon, ancient customs contrasted with modern conditions, *Odont.* 1, 2
- normal structure and hypoplastic structure, caries in, compared, *Odont.* 77
- of children, importance of sufficiency of sleep in regard to, *Odont.* 5
- radiographs of, in dental sepsis, importance of correct interpretation, *Odont.* 22, 23
- resistance of, to caries, effect of diet on (May Mellanby), *Odont.* 74-82
- factors acting indirectly through pulp and other tissues of dominant importance, *Odont.* 75
- summary, *Odont.* 79-82
- reaction of, to external stimuli, *Odont.* 77
- structure of, in relation to caries, *Odont.* 76, 77
- Tegmen**, ossification of incus to (S. Scott), *Otol.* 20
- Teleröntgenography** in cardiac diagnosis, *Electr.* 7
- Temperature**, effect upon prognosis of eclampsia, *Obst.* 3
- curves in influenza, trench fever and sand-fly fever, graph comparing, *War* 8
- Temporal** bone from case of tuberculous lateral sinus thrombosis and extra-cerebellar abscess (E. D. D. Davis), *Otol.* 5
- left, necrosis of, involving facial nerve and labyrinth, following triple infection of scarlet fever, measles and diphtheria, in child aged 7, case (J. F. O'Malley), *Otol.* 29
- Temporo-sphenoidal** abscess, left: amnesia for names of objects, case (S. Scott), *Otol.* 55
- operation for, epileptiform seizures subsequent to, case, (D. McKenzie), *Otol.* 52
- Temporo-sphenoidal** abscess, right, without clinical signs, in case of brain abscess due to otitic infection (T. H. Just), *Otol.* 54
- Tendon** transplantation, case of (B. W. Howell), *Orth.* 50
- for talipes (E. Laming Evans), *Orth.* 14
- Testicles**, atrophy of, in myotonia atrophica, *Neur.* 10, 11
- Testicular** medication in organo-therapy, *Therap.* 13, 17
- Tests** for hearing, attempt to standardize (S. Hastings and Major W. S. Tucker), *Otol.* 1-5
- Thermostability** of bactericidal body in rat serum, *Path.* 5, 6, 7
- Thigh**, gunshot fracture of, comminuted, showing displacement of fragments, *Path.* 34
- Thompson**, A. R.—The propriety of attempting to secure primary union after operations upon bladder and prostate, *Urol.* 47-53
- Thompson**, C. J. S.—Discussion on medical services of Henry V's campaign of Somme in 1415, *Hist.* 10
- Thomsen's** disease, atypical, cases described as, *Neur.* 36, 37
- distinction of dystrophia myotonica from, *Neur.* 41
- Thomson**, J.—Discussion on cases of duodenal obstruction in infants, *Child.* 13
- Thomson**, Sir StClair.—Bismuth and glycerine gauze, *Laryng.* 29
- discussion on case of extensive lupus of palate, pharynx and larynx, *Laryng.* 51
- on improved antrum-exploring trocar and cannula, *Laryng.* 53
- on laryngeal case for diagnosis, *Laryng.* 51
- on oedema of septum in association with nasal polypi, *Laryng.* 28
- on tuberculous ulcer of dorsum of tongue, *Laryng.* 51
- remarks at Pasteur Centenary Meeting, *Occ. Lect.* 27
- healed tuberculosis of lung and larynx, *Laryng.* 64
- laryngeal case apparently of epithelioma (possibly syphilis) completely healed and arrested under X-ray treatment without operation, *Laryng.* 60
- tuberculosis of larynx cured seven years ago by silence and galvano-cautery, *Laryng.* 64
- two cases of laryngo-fissure for intrinsic cancer of larynx, *Laryng.* 59
- Thomson-Walker**, Sir John.—Case of aberrant prostatic nodule, *Urol.* 32
- of cyst of prostate, *Urol.* 31
- of malignant growth of renal pelvis, with calculi, *Urol.* 85-87
- of myosarcoma of epididymis, *Urol.* 31
- of vesico urethral calculus, *Urol.* 87
- malignant changes in simple benign enlargement of prostate (quoted), *Urol.* 76
- origin of renal calculi (quoted), *Surg.* 21
- relation of calcified abdominal glands to urinary surgery, *Urol.* 1-17
- and **Barrington**, F. J. F.—Case of malakoplakia, *Urol.* 32-34
- Thoracoscope**, cauterization of adhesions under guidance of, critical survey of operations, *Electr.* 54-60

- Thoracoscope**, technique, *Electr.* 47-48
adhesions of collapsed lung as seen by, appearances of, *Electr.* 52
- Thoracoscopic** control, artificial pneumothorax treatment of pulmonary tuberculosis under, cauterization of adhesions in (H. C. Jacobæus), *Electr.* 45-60
- Thoracoscopy**, value of, for differential diagnosis, *Electr.* 45
- Thorax**, normal, instantaneous radiograms showing, *Electr.* 16
- Thornhill**, W. H., and **Kelson**, W. H.—Case of ulceration of palate and fauces, *Laryng.* 13
- Throat**, nose and ear, infections of, teeth and gums in their relationship to, discussion on, *Odont.* 35-42
symptoms, early sign in case of myasthenia gravis (C. P. Symonds), *Laryng.* 17
- Thrombo-angitis obliterans**, cases (G. Evans), *Clin.* 12-14; (St. J. D. Buxton), *Clin.* 14, 15
- Thrombopenia**, in relation to effects of exposure to radium, *Electr.* 44
- Thrombosis**, see *Lateral sinus thrombosis*
- Thumbs**, both, absence of, with other deformities of upper extremities, in infant (B. Myers), *Child.* 72
- Thursfield**, J. Hugh—Case of congenital syphilis, *Child.* 41
case of polyarthritis, *Child.* 41
discussion on case of acquired chronic hæmolytic (acholuric) jaundice, *Med.* 80
- Thymol**, use in expulsion of *Gastrodiscoides hominis*, *Trop.* 13
- Thyroid**, dry, in treatment of chilblains, *Therap.* 15
gland, diseases of, effect on position and shape of trachea, *Laryng.* 82
incidence of laryngeal paralysis in, *Laryng.* 81
in their relation to laryngology (F. Holt Diggle), (abstract), *Laryng.* 81
hormones of, use of, *Therap.* 14, 15
influence on nutrition, *Psych.* 24
instability, Graves' disease and, in the cow, and its relation to ovarian disease (L. P. Pugh), *Otol.* 92-99
medication in organo-therapy, *Therap.* 11, 16
reaction of, to noxious stimuli, *Psych.* 25
tumour from base of tongue (H. B. Tawse), *Laryng.* 21
- Thyro-toxic** conditions, auricular fibrillation associated with, action of quinidine in cases of, *Therap.* 27, 28
- Thyroxin**, administration in myxœdema, in relation to effect on basal metabolic rate, *Therap.* 15
- Tibia**, right, acute osteomyelitis of, in girl aged eight years, case (H. T. Gray), *Child.* 44
- Tidy**, H. L.—Discussion on case of acquired chronic hæmolytic (acholuric) jaundice, *Med.* 80
glandular fever and infective mononucleosis (abstract), *Med.* 70-72
- Tilley**, H.—Case of myotonia atrophica with implication of left crico-arytænoid muscle, *Laryng.* 18
discussion on case of chronic laryngitis of long standing, *Laryng.* 24
- Tilley**, H.—Discussion on case of tuberculoma of pharynx, *Laryng.* 52
discussion on case of multiple papillomata of larynx, *Laryng.* 46
of sarcoma of nose cured by radium, *Laryng.* 50
on diseases of thyroid gland in their relation to laryngology, two cases described, *Laryng.* 82, 83
on improved antrum-exploring trocar and cannula, *Laryng.* 53
on infections of mouth and gums in their relationship to nose, throat and ear, *Odont.* 40
on lingual tonsil, *Laryng.* 76
on specimen of cyst of larynx, *Laryng.* 54
microscopical section of a benign, pedunculated tumour of left tonsil, *Laryng.* 20
skiagram showing paper fastener in left bronchus of child, *Laryng.* 20
- Tin and lead miners**, mortality from phthisis per 1,000 living at various age periods, *Epid.* 90
- Tin-miners**, mortality among, from various causes, *Epid.* 94
- Tinnitus** associated with facial spasms (G. J. Jenkins), *Otol.* 8
- Tirard**, Sir N.—Discussion on value of ergot, *Obst. & Therap.* 5
- Todd**, A. H.—Case of myeloma of outer condyle of femur, showing result of bone-grafting, *Clin.* 3, 4
- Toes and fingers**, clubbing of, case (O. C. M. Davies), *Child.* 81
great, both, involved by double hammer-toes, (A. E. M. Woolf), *Clin.* 1
- Tongue**, base of, thyroid tumour from (H. B. Tawse), *Laryng.* 21
dorsum of, tuberculous ulcer of (W. Howarth), *Laryng.* 50
lesions of, in pernicious anæmia, intense neuritis and muscle changes in, *Med.* 6, 8, 9, 10
lymphangioma circumscriptum of, case (G. Petit), with discussion, *Derm.* 58
- Tonsil and palate**, growth in, multiple foci of, case (W. M. Mollison), *Laryng.* 19
cartilage formation in, unusual tonsillar appendage and its relation to (abstract), (A. L. Turner), *Laryng.* 16
summary of observations recorded, *Laryng.* 16
left, benign pedunculated tumour of, microscopical section of (H. Tilley), *Laryng.* 20
sarcoma of, case (Sir W. Milligan), *Laryng.* 88
ulceration of, case for diagnosis (H. Bell Tawse), *Laryng.* 70-72
lingual, clinical observations on (abstract), (J. Arnold-Jones), *Laryng.* 74
right, and surrounding faucial region, sarcoma of, case (Sir W. Milligan and F. Wrigley), *Laryng.* 91
- Tonsillar** appendage, unusual, and its relation to cartilage formation in tonsil (abstract) (A. Logan Turner), *Laryng.* 16
- Tonsillitis**, mortality from, 1901-1920, statistics, *Surg.* 8
- Tonsils**, region of, sarcoma of, treated by X-rays after partial removal, case (Sir J. Dundas Grant and D. McKenzie), *Laryng.* 69

- Tooth** follicle, in relation to enamel organ of *Felis domestica*, *Odont.* 51
 variability in various stages of growth, *Odont.* 52, 53
- Tooth-Marie-Charcot** type of muscular atrophy, case (E. C. Williams), *Child.* 79
- Torticollis**, ocular, case of (Paul B. Roth), *Orth.* 46
- Tournay** reaction (abstract) (P. Doynes), *Ophth.* 47
- Toxæmia** as cause of changes in retina in renal retinitis, *Med. & Ophth.* 2, 3, 4, 5
 dental sepsis causing, *Odont.* 12
- Toxæmic** attacks in pernicious anæmia, *Med.* 21
 kidney, definition of, *Urol.* 20
 origin of "renal" retinitis, *Med. & Ophth.* 15, 16
- Toxic** factors in relation to genius and insanity, *Psych.* 36
- Toxins**, selective effect on nervous tissues, *Urol.* 20
- Trachea** and œsophagus, threepenny piece impacted in, perforation between, in baby aged 3 months, mounted specimen showing (H. J. Banks-Davis), *Laryng.* 55
 obstruction of, due to (?) arrest of development of trachea (C. A. S. Ridout), *Laryng.* 58
 parts removed post mortem in case of (C. A. S. Ridout), *Laryng.* 10
 of child aged 6 months, foreign body removed from (H. Smurthwaite), *Laryng.* 66
 papillomata of, case (Sir J. Dundas Grant and J. J. Perkins), *Laryng.* 7
 position and shape of, effects of disease of thyroid gland on, *Laryng.* 82
 two foreign bodies in, one movable and the other fixed, in case of child aged 3, mounted specimen showing (H. J. Banks-Davis), *Laryng.* 55
- Trachoma**, suppression among Chinese Labour Corps, *Epid.* 64
- Tragulus napu*, description of *Gastrodiscoides hominis* from (M. Khalil), *Trop.* 9-14
- Transference**, *Psych.* 14, 15, 17, 18, 19
 building up of, *Psych.* 17
 Jung's views on, *Psych.* 19
 negative, *Psych.* 14, 17, 18
 positive, *Psych.* 14
- Trauma** in ætiology of myopathy, cases reported, *Neur.* 6, 7
 muscular dystrophy and, *Neur.* 3, 7
- Traumatic** spondylitis (T. H. Openshaw), *Orth.* 1-10
- Tremor**, case of, for diagnosis (A. Feiling), *Neur.* 27
 post-encephalitic, localization of lesion in brain, *Ophth.* 42
- Tremors**, progressive macular changes associated with, case (H. M. Joseph), *Ophth.* 39
- Trench** fever, influenza and sand-fly fever, temperature and leucocyte curves compared, *War* 8
 mouth, deficiency of vitamin C associated with, *Odont.* 4
- Trepol**, results of treatment by, case showing (J. H. Sequeira), *Derm.* 21
- Trethowan**, W. H.—Discussion on operative treatment of dislocated hips, *Orth.* 24
- Trevan**, J. W. and Boock, E.—Effect of light on response of frogs to drugs, *Therap.* 8
- Trichomycosis** axillaris, varieties of, and fungi causing, *Derm.* 97, 98
 rubra, case of (A. Castellani), *Derm.* 97
- Trichorrhæxis** nodosa, case (J. L. Bunch), *Derm.* 74
- Trocar** and cannula, antrum-exploring, improved form of (H. M. Wherry), *Laryng.* 53
- Trochanter**, small, fracture of (W. T. G. Pugh), *Orth.* 12
 traction fracture of, case (P. M. Heath), *Orth.* 12
- Tropacocaine**, spinal anæsthesia with, Cæsarean section under, two cases (B. Whitehouse and H. Featherstone), *Obst.* 55-58
- Tropical** climate, effect of, on physical and mental efficiency (T. S. Rippon), *War* 46-54
 ill-effects on blonde subjects, *Baln.* 13
 psychoneurosis and, *War* 53
- Tropics**, Antirabic Institute in, establishment of (A. E. Hamerton), *Trop.* 49-55
 commoner continued fevers occurring in, *War* 2
- Trotter**, G. C.—Discussion on enteric fever due to carriers of infection, *Epid.* 10
- Trotter**, W.—Surgical treatment of eighth nerve tumours (abstract), *Otol.* 37
- Tubercular** choroiditis, *Ophth.* 2
 conjunctivitis, *Ophth.* 4
 iritis, *Ophth.* 3
 keratitis, *Ophth.* 6
 scleritis, *Ophth.* 6
- Tuberculides**, association of folliculitis decalvans with, unique case of, *Derm.* 102
- Tuberculin** in treatment of tubercular conjunctivitis, dosage, *Ophth.* 4, 5
- Tuberculoma** of pharynx, case (N. Patterson and G. C. Cathcart), *Laryng.* 51
- Tuberculosis** as cause of polyneuritis, rare, *Neur.* 18
 economics and (R. J. Ewart), *Epid.* 11-16
 free meals provided to school children, 1912-1922, in relation to, *Epid.* 15
 inherited diathesis in relation to, *Epid.* 15, 16
 wage movements and cost of living in relation to, table showing, *Epid.* 14, 15
 intraventricular, case (Sir J. Dundas-Grant), *Laryng.* 18
 laryngeal, case (J. A. K. Renshaw), *Laryng.* 93
 case of, with demonstration of instrument for sunlight treatment (Sir J. Dundas-Grant), *Laryng.* 12
 cured seven years ago by silence and galvanocautery (Sir StClair Thomson), *Laryng.* 46
 mental state of geniuses suffering from, *Psych.* 36
 miliary, instantaneous radiogram showing, *Electr.* 15
 mortality statistics in Vienna 1906-1916, *Epid.* 11
 ocular, some aspects of (A. L. Whitehead), *Ophth.* 2-7
 frequency of latent tuberculosis in glands and, *Ophth.* 5
 references, *Ophth.* 7
 of mesenteric glands, usually an isolated infection, *Urol.* 4
 of retina, primary, *Ophth.* 3
 prevalence of, and poverty, relationship between, *Epid.* 11

- Tuberculosis**, pulmonary, active, blood pressure low in, *Baln.* 2
 active, variation in systolic blood pressure in *Baln.* 3
 and laryngeal, healed case (Sir St.C. Thomson), *Laryng.* 64
 as shown by X-rays, but without physical signs (S. Melville), *Electr.* 31-35
 death-rate from, food-supply in relation to, *Epid.* 15
 consumption of food and real wages compared with (1851-1920), *Epid.* 12, 13, 14
 in relation to real wages, chart indicating, *Epid.* 14
 dispensary population of, *Epid.* 16
 instantaneous radiogram showing, *Electr.* 15
 outgrowth of ventricle in subject of, case (Sir J. Dundas-Grant), *Laryng.* 55
 under thoracoscopic control, cauterization of adhesions in artificial pneumothorax treatment of (H. C. Jacobæus), *Electr.* 45, 60
 with laryngeal symptoms, two cases (P. Franklin), *Laryng.* 25
 see also *Phthisis*
 renal, relation of, to tuberculous abdominal glands, *Urol.* 4, 5
Tuberculous disease of lacrymal sac, *Ophth.* 3
 infection, pulmonary, fibrosis following, case (E. C. Williams), *Child.* 79
 silicosis, low infectivity of, *Epid.* 99
 ulcer of dorsum of tongue (W. Howarth), *Laryng.* 50
Tucker, J. H.—Founder of Epidemiological Society of London, *Epid.* 35
Tucker, W. S., Major R.F., and **Hastings**, S.—An attempt to standardize tests for hearing, *Otol.* 1-5
Tumour, calcified, of recto-vaginal septum (L. C. Rivett), *Obst.* 81
Turbinal, inferior, pedunculated angioma of (bleeding polypus), case (S. Hastings), *Laryng.* 25
Turnbull, H. M.—Report on case of chorion-epithelioma of uterus showing very extensive growth in uterine wall, *Obst.* 68
 report on microscopical appearances in case of squamous carcinoma of face, *Derm.* 52, 53
Turner, A. Logan.—Discussion on case of extensive lupus of palate, pharynx and larynx, *Laryng.* 52
 of sarcoma of nose cured by radium, *Laryng.* 50
 on tests for hearing, *Otol.* 4
 unusual tonsillar appendage and its relation to cartilage formation in tonsil (abstract), *Laryng.* 16
 and **Fraser**, J. S.—Labyrinthitis as a complication of middle-ear suppuration (abstract), *Otol.* 15
Turner, F. M.—Age and sex distribution in scarlet fever, *Epid.* 19-30
Turner, G. Grey.—Case in which adenoma weighing 2 lb. 3 oz. was successfully removed from liver, with remarks on partial hepatectomy, *Surg.* 43-56
Turner, J. G.—Discussion on dental sepsis, *Odont.* 24
Turner, Philip.—Case of bilateral calculous pyonephrosis ten years after double nephro-lithotomy, *Clin.* 40
 case of excision of adenoma of liver, which had ruptured spontaneously causing internal hæmorrhage, *Surg.* 60
 hydronephrosis of a single kidney; spontaneous rupture into peritoneal cavity, *Clin.* 24.
Turner, W. Aldren.—Case of disseminated sclerosis, *Neur.* 52
Turpentine, injection of, in treatment of case of sciatica in woman suffering from lead poisoning, *Clin.* 8
 intra-uterine dressing of, in treatment of hæmorrhage due to functional causes, *Obst.* 62
Twin pregnancies, eclampsia and, *Obst.* 2
Twining, E. W.—Pirie's method of radiographing the mastoid cells, *Laryng.* 96
Twort-d'Herelle phenomenon, *Epid.* 74, 77, 78
Tylosis, hereditary, case (J. D. Rolleston), *Child.* 24
Tympanic membrane and meatus, laceration of, produced by knitting-needle (H. J. Banks-Davis), *Otol.* 30
Typhoid and paratyphoid infections, asymmetrical neuritis in, *Neur.* 15
 fever, carriers of infection, administrative treatment, *Epid.* 9, 10
 circumscribed outbreak in Northern Scottish county, *Epid.* 4
 due to carriers of infection (abstract) (F. Dittmar), *Epid.* 1-10
 factor of intermittency, *Epid.* 5, 6
 in Scottish poorhouse, details of cases, *Epid.* 1-4
 mortality in 1880 and 1920 compared, *Med.* 47
 outbreak in mental hospital, *Epid.* 7-9
 traced to milk infected by carriers, *Epid.* 6
 outbreaks traced to working housekeeper, *Epid.* 4, 5
 Scottish Board of Health, regulations as to treatment, *Epid.* 9
 urinary carrier of, *Epid.* 6
Typhus fever, mortality in 1880 and 1920 compared, *Med.* 47
Tyramine in ergot preparations, *Obst. & Therap.* 3
Ulcer, duodenal, see *Duodenal* ulcer
 rodent, see *Rodent* ulcer
 tuberculous, of dorsum of tongue (W. Howarth), *Laryng.* 50
Ulcerative colitis, discussion on, *Proct.* 91-110
 see also under *Colitis*
Ulcus rodens erythematosides, case of (L. Savatard), *Derm.* 65
Ultraviolet viruses considered from epidemiological point of view (Sir W. Hamer), *Epid.* 65-76
Uræmia, factors in (W. Langdon Brown), *Urol.* 19-27
 and uræmia, symptoms contrasted, *Urol.* 19, 20
 causes of, summary and conclusions, *Urol.* 26, 27

- Uræmia**, convulsions and amaurosis in, significance and causation of, *Urol.* 23
 chronic parenchymatous nephritis and, *Urol.* 22
 existence of two definite groups of cases, *Urol.* 26
 focal embolic nephritis and, *Urol.* 22
 in relation to nephritis, *Urol.* 20
 to acute nephritis, *Urol.* 21, 22
 paroxysmal dyspnoea in, significance and causation of, *Urol.* 23
 pressure as factor in causation of, *Urol.* 29
 urea retention in, *Urol.* 19
- Uræmic asthma**, restriction of use of term, *Urol.* 24
- Urea**, in body fluids, distribution of, *Urol.* 25
 percentage of, in blood and in vomit, compared, *Urol.* 19
 retention in uræmia, *Urol.* 19
 marked, in cases of retinitis with gross disturbance of renal function, *Med. & Ophth.* 17, 18
- Urea-concentration test** in cases of retinitis with gross disturbance of renal function, *Med. & Ophth.* 17, 18
 without gross disturbance of renal function, *Med. & Ophth.* 19
- Ureter**, kinking of, by aberrant renal vessels, pyonephrosis due to, *Urol.* 41
 left, deformity of, with absence of right kidney, case (W. Girling Ball), *Urol.* 35
 relationship of, to neck of vesical diverticulum, *Urol.* 67
- Urethroscopy**, air embolism occurring during, case (R. Ogier Ward), *Urol.* 54
- Urinæmia and uræmia**, symptoms contrasted, *Urol.* 19, 20
 syndrome of, *Urol.* 20
- Urinary fistula**, suprapubic. See under *Fistula*
 surgery, relation of calcified abdominal glands to (Sir J. Thomson-Walker), *Urol.* 1-17
- Urine**, analysis of, twenty-two months after hepatectomy, *Surg.* 48
 bacillus typhosus in, in case of enteric "carrier," *Epid.* 6
 condition of, in retinitis due to local vascular disease, *Med. & Ophth.* 10
 examination of, in diagnosis of sand-fly fever in Malta, *War* 3, 8
 fractional analysis of, method for, *Urol.* 83
 residual, in vesical diverticula, *Urol.* 68
- Urticaria pigmentosa**, cases of (H. W. Barber), *Derm.* 94; (J. A. Drake) *Derm.* 73; (J. M. H. MacLeod) *Derm.* 73; in an adult (E. G. G. Little), *Derm.* 72
- Uterus**, body of, cancer of, necrotic fibro adenoma in patient aged 74, simulating (J. S. Fairbairn), *Obst.* 45
 cancer of, mortality, decline in, *Occ. Lect.* 36
 cervix, cancer of, simulated by adenoma of vaginal fornix (H. R. Spencer), *Obst.* 27
 treated by radium before operation, two cases (T. W. Eden and A. Goodwin), *Obst.* 32, 34, 35
 large fibroid of, developing after subtotal hysterectomy (A. E. Giles), *Obst.* 12
 prolapsed, carcinoma of, in woman aged 77 (H. R. Andrews), *Obst.* 109
 spontaneous partial extrusion of pair of hæmostatic forceps left in peritoneal cavity through, case, *Obst.* 36
- Uterus**, chorion-epithelioma of, showing very extensive growth in uterine wall (S. G. Luker), *Obst.* 67
 cornu of, cyst of, due to dilatation of interstitial portion of tube (J. S. Fairbairn), *Obst.* 45
 curettings of, section of (H. Briggs), *Obst.* 61
 fibroids of, treatment by myomectomy, indications for, and results of, *Obst.* 13-21
 hæmorrhage from, severe and persistent, treatment by radium, with report on 45 cases (S. Forsdike), *Obst.* 69-78
 treatment by radium, X-rays and operation, relative advantages, *Obst.* 77
 inertia, in first stage of labour terminating in inversion, *Obst.* 49
 inversion of, occurring at third week of puerperium (W. R. White-Cooper and Griffith, H. K.), *Obst.* 48, 49
 inverted, septic, danger of replacing, *Obst.* 49
 method of opening, in lower segment, in Cæsarean section, advantages and disadvantages, *Obst.* 54
 ovary and breast, cancer of, mortality compared, (1906, 1915, 1920), *Occ. Lect.* 36
 removed for carcinoma of cervix after treatment by radium (A. H. Richardson), *Obst.* 31
 sarcoma of, two specimens of (J. D. Barris), *Obst.* 65-67
 sepsis of, controlled by Carrel's tubes, *Obst.* 50
 wall of, very extensive growth of chorion-epithelioma in, (S. G. Luker), *Obst.* 67
- Uveitis and parotitis**, polyneuritis with, *Neur.* 17
- Uvula**, cyst of (T. J. Faulder), *Laryng.* 25
- Vaccination**, Edward Jenner's experiments leading to introduction of, *Occ. Lect.* 4, 5
 in Japan, history of (Mr. Miyajima), *Hist.* 23-26
 spread of practice of, throughout Europe, in 1799-1800, *Occ. Lect.* 6
- Vaccine**, antirabic, manufacture of, *Trop.* 52; see also under *Antirabic Institute*
- Vaccines** in treatment of ulcerative colitis, *Proct.* 95, 101
- Vagal trauma**, effects of, on anæsthetized patient, (C. L. Hewer), *Anæsth.* 7
- Vagina**, carcinoma of, primary, specimen (E. Holland), *Obst.* 25
 epithelioma of, squamous, specimen (T. G. Stevens), *Obst.* 26
 fornix, adenoma of, simulating cancer of cervix, (H. R. Spencer), *Obst.* 27
 wall of, angioma of (H. Briggs), *Obst.* 61
- Vago-sympathetic plexuses**, comparative anatomy of, *Baln.* 11
- Vagus and sympathetic nerves**, and their relation to climate and hydrology, discussion on, *Baln.* 7-17
 afferent impulses to nerve-centres through, routes for, *Baln.* 8
 in man and lower animals compared, *Baln.* 8, 9, 10
 nerve, trauma of, effects on anæsthetized patient, *Anæsth.* 7
- Valentine, J. A.**—Cyst of retina, *Ophth.* 1
 night blindness: retinitis pigmentosa sine pigmento, *Ophth.* 17
- Van den Bergh's test** in case of congenital non-familial jaundice without enlargement of liver or spleen, results of, *Med.* 82, 83

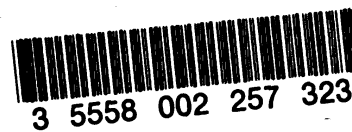
- Yaquez-Osler's** disease, treated by Röntgen-therapy, two cases (E. Stolkind), *Clin* 35 with cerebral hæmorrhage, case of (J. A. Ryle), *Med.* 83
- Yaquez** and Bordet, on value of precise radiological methods (quoted), *Electr.* 11, 12
- Yascular** supply of enamel organ of *Felis domestica* (E. Sprawson), *Odont.* 47-54
- Yaso-constrictor** nerves, origin of, *Baln.* 12
- Yaso-dilator** nerves, origin of, *Baln.* 12
- Yasomotor** nerve fibres, sensory, stimulation of, effects, *Baln.* 12
- Yater**, ampulla of, filling of, by barium, significance of, in relation to pathological gall-bladder, *Electr.* 79
- Venereal** disease, ante-natal mortality from, *War* 21
 as a war casualty (Surg. Rear-Admiral W. Bett), *War* 15-25
 incidence at various periods, *War* 15
corrigendum, *War* 44
 discussion on, *War* 25-29
 historical review of, *War* 15, 16
 history of, and observations on various Contagious Diseases Acts, *War* 22-25
 in American Civil War, *War* 16
 in civil population, incidence of, *War* 20, 21
 in Crimean War, *War* 16
 in Franco-Prussian War, *War* 16
 in late war (1914-18), incidence of, *War* 17
 in Peninsular War, *War* 16
 in Scotland, incidence of (T. F. Dewar), (abstract), *Epid.* 81-84
 conclusions regarding, *Epid.* 83
 notification, "conditional," suggested, *Epid.* 84
 prevention of, measures for, in the Services, *War* 20
 by encouragement of games and sport, *War* 20
 prophylactic measures, *War* 18, 19
 relation of alcoholic habits to, *War* 20
 treatment at clinics, statistics, *War* 21
- Venesection**, treatment of eclampsia by, results, *Obst.* 9
- Ventricle**, cerebral, fourth, floor of, ependymal glioma growing from, simulating cerebellar abscess, in case of bilateral chronic suppurative otitis media, section of (T. H. Just), *Otol.* 62
 laryngeal, outgrowth from, in subject of pulmonary tuberculosis, case (Sir J. Dundas-Grant), *Laryng.* 55
 stripping of lining membrane of, in treatment of stenosis of larynx caused by bilateral abductor paralysis, *Laryng.* 33
 and sacculus, difference between, in horse and man, *Laryng.* 34
- Ventricular** band, infiltration of (intraventricular tuberculosis), case (Sir J. Dundas-Grant), *Laryng.* 43
 right, swelling of, case (Sir J. Dundas-Grant), *Laryng.* 43
 bands, valvular action of, case illustrating (Sir J. Dundas-Grant), *Laryng.* 43
- Ventriculectomy** in treatment of stenosis of larynx caused by bilateral abductor paralysis, *Laryng.* 33
- Ventriculo-chordectomy** in treatment of double abductor paralysis, case (W. Howarth), *Laryng.* 47
 in treatment of stenosis of larynx caused by bilateral abductor paralysis, *Laryng.* 35
- Vernon**, H. M.—Views on enzymes (quoted), *Epid.* 69
- Verrall**, P. Jenner.—Discussion on operative treatment of spastic paralysis, *Orth.* 41
- Vertebral** column, fracture-dislocation in cervical region, methods of treatment, *Orth.* 2, 3
 symptoms of, *Orth.* 2
 in dorsal and lumbar regions, diagnostic difficulties, *Orth.* 3
- Vertigo**, cured by opening the external semicircular canal, case of (W. M. Mollison), *Otol.* 60
 simulating Ménière's disease, with anomalous nystagmus reactions, case (Sir J. Dundas-Grant), *Otol.* 20
 with fixation of ossicles, cured by ossiculectomy, case (Sir J. Dundas Grant), *Otol.* 18
- Vesical** diverticula. See *Bladder*, diverticula of
- Vesico-urethral** calculus, case (Sir J. Thomson-Walker), *Urol.* 87
- Vestibular** activity, almost complete loss of, in case of absolute bilateral deafness (A. Ryland), *Otol.* 7
- Yetch**, J., on purulent ophthalmia (quoted), *Epid.* 49, 50
- Yeyers**, G. M.—Lung flukes of genus *Paragonimus*, *Trop.* 43, 44
- Vienna**, total death-rate and death-rate from phthisis, 1912-1920, *Epid.* 11
- Vincent**, Swale.—Discussion on Graves' disease and thyroid instability in the cow, and its relation to ovarian disease, *Obst.* 100
 discussion on present position of organo-therapy, *Therap.* 9
- Vines**, H.—Discussion on present position of organo-therapy, *Therap.* 23
- Viruses**, ultraviolet, considered from epidemiological point of view (Sir W. Hamer), *Epid.* 65-76
 diseases possibly due to, *Epid.* 78
 presumptions regarding, *Epid.* 66
- Viscera**, effects on, of foreign bodies left in peritoneal cavity, *Obst.* 38, 39
- Visceroptosis**, associated with duodenal ulcer, high acidity, relief after gastro-enterostomy, case (H. S. Souttar), *Clin.* 5
 with low acidity and duodenal ulcer, case (H. S. Souttar), *Clin.* 5, 6
- Vision**, standards of, for scholars and teachers in Council Schools (abstract) (N. B. Harman), *Ophth.* 7
- Visual** disorder as cause of psychoneurosis, *War* 53
 fatigue in air pilots, effect of experience upon, *War* 51
 in India, investigations into, *War* 45, 46
 symptoms of, *War* 46
 loss of stereoscopic sense, early sign of, *War* 51
- Vitamin A**, deficiency of, in diet, effects on teeth and jaws in puppies, experiments showing, *Odont.* 75, 76
 antiscorbutic, deprivation of, effect on teeth of guinea-pig, *Odont.* 76
 C, deficiency of, association with "trench mouth," *Odont.* 4

- Vitamins**, absence from diet, favouring infection of colon, *Proct.* 92
association with preventive dentistry, *Odont.* 4
- Vitiligo**, morphea associated with, case (H. W. Barber), *Derm.* 106
- Vlasto**, M., anatomical differences between ventricle and sacculus in horse and man (quoted), *Laryng.* 34
case of dentigerous cyst, *Laryng.* 43
discussion on cases of otosclerosis with unusual symptom, *Otol.* 10
on lingual tonsil, *Laryng.* 75
- Vocal** cord, antero lateral transplantation of, in treatment of stenosis of larynx caused by bilateral abductor paralysis, *Laryng.* 35
evisceration or ablation of, with soft parts lining larynx, in treatment of stenosis of larynx caused by bilateral abductor paralysis, *Laryng.* 35
left, and left half of palate, paralysis of, two cases (Sir J. Dundas-Grant), *Laryng.* 68
outgrowth from, ? fibroma or prolapse, case (Sir J. Dundas-Grant), *Laryng.* 69
right, epithelioma of, case (F. Holt Diggle), *Laryng.* 95
epithelioma of, treatment by laryngo-fissure, case (Sir W. Milligan), *Laryng.* 89
cords, point of origin of, *Laryng.* 86
- Voice**, conditions of, following operative procedures for stenosis of larynx, *Laryng.* 38
- Wages**, real, and phthisis death-rate, chart showing relation between, *Epid.* 14
food consumption and death-rate from phthisis, compared, *Epid.* 13
- Wales**, lead miners in, mortality among (1908-1918), *Epid.* 95
see also *England and Wales*
- Walker**, C.—Case of spring catarrh, *Ophth.* 34
- Walker**, Kenneth M.—Serous cyst of kidney, *Urol.* 45
- Wallace**, W.—Discussion on genius and insanity, *Psych.* 37
- Walshe**, F.M.R.—Acoustic tumours, *Otol.* 32
case for diagnosis: spinal compression or disseminated sclerosis, *Neur.* 48
and **Cleminson**, F. J., case of acoustic tumour (right); operation by Sir V. Horsley in 1912; removal of tumour; recovery, *Otol.* 31, 32
- Walton**, A. J.—Cases of hepatectomy, *Surg.* 62
- War** casualty, venereal disease as (Surg. Rear-Admiral W. Bett), *War* 15-29
effect of scientific progress on, *War* 32
human factor in, *War* 32
physical training of Army during, *War* 31
(the late) (1914-18), effects of, on surgery, *Surg.* 9
venereal disease in, incidence of, *War* 17
- Ward**, R. Ogier. Case of air embolism occurring during urethroscopy, *Urol.* 51
case of large renal calculus, *Urol.* 38
- Wassermann** reaction, application of, as basis for estimate of incidence of syphilis, *Epid.* 82
in diagnosis of sandfly fever in Malta War 3 5
- Wassermann** reaction, in early and late syphilis, effects of intensive treatment on, *Neur.* 65, 66
negative, in presence of definite clinical signs of syphilis, *Neur.* 63, 73, 74, 75, 76
of cerebro-spinal fluid before and after treatment in series of cases of syphilis, *Neur.* 67
- Watson-Williams**, E.—Acute nasal sinus disease in young children, *Child.* 81-84
- Watson-Williams**, P.—Discussion on infection of teeth in their relationship to nose, throat and ear, *Odont.* 35
discussion on lingual tonsil, *Laryng.* 76
method of exploration of nasal accessory sinuses, *Laryng.* 96
- Waugh**, G. E. and **Pitts**, A. T., D.S.O.—Case of ankylosis of jaw, *Child.* 44
- Weber**, F. Parkes — Argyll-Robertson pupils with mydriasis, *Child.* 68
case of acquired chronic hæmolytic (acholuric) jaundice, seen fifteen years ago, with a blood picture at that time resembling one of pernicious anemia, *Med.* 73-77
of chronic splenomegaly of uncertain origin, *Child.* 64
of exophthalmos probably caused by non-suppurative cavernous sinus thrombosis, *Clin.* 41
congenital hæmolytic jaundice, *Child.* 66
discussion on action of quinidine in case of cardiac disease, *Therap.* 40
on cases for diagnosis, *Derm.* 57, 58, 90
on case of enlarged liver with persistent acetonuria and diaceturia, *Child.* 59
of erythremia, *Med.* 84
of generalized scleroderma with nodules, *Derm.* 108
of hereditary tylosis, *Child.* 25
of Hirschsprung's disease, *Clin.* 32
of parakeratosis variegata, *Derm.* 105
of patent ductus arteriosus and mitral disease, *Child.* 49
of pellagra, *Child.* 63
of thrombo angiitis obliterans, *Clin.* 15, 16
of unusual localization of ichthyosis, *Derm.* 95
of xanthoma (? diabetorum), *Derm.* 93
on endocrine factor in mental disease, *Psych.* 31
on factors in uræmia, *Urol.* 28
on late effects of encephalitis lethargica, *Child.* 35
on osteo-arthritis of spine, *Med.* 69
rare case of congenital non-familial jaundice, without enlargement of liver or spleen, in an otherwise healthy man aged 56, *Med.* 81-83
- Raynaud's** syndrome in non-syphilitic infant, with remarkable family history (abstract), *Child.* 47
- Westmacott**, F. H., C.B.E.—Case of cicatricial contraction of right vocal cord, *Laryng.* 96
case of growth in anterior commissure, *Laryng.* 96
of growth in anterior commissure and on adjoining extremity of right vocal cord, *Laryng.* 96
of growth in anterior commissure of larynx and on right vocal cord, *Laryng.* 96

- Westmacott, F. H., C.B.E.**—Case of laryngeal growth, *Laryng.* 96
of lupus of right nasal fossa, *Laryng.* 96
of ulceration of soft palate and left tonsil, *Laryng.* 96
discussion on treatment of large foreign bodies impacted in gullet, *Laryng.* 80
extensive osteomyelitis of frontal bone, case, *Laryng.* 93
sarcoma of maxilla and malar and frontal bones, *Laryng.* 92
two cases of chronic œdema of orbit, *Laryng.* 91
- Whale, H. Lawson**—Discussion on case of multiple papillomata of nose, *Laryng.* 46
on case of vertigo cured by opening external semicircular canal, *Otol.* 61
papilloma of septum nasi, *Laryng.* 12
- Wharry, H. M.**—Improved antrum-exploring trocar and cannula, *Laryng.* 53
- White, C.**—Discussion on case of carcinoma of vagina, *Obst.* 27
instruments left in peritoneal cavity; effects and results as shown by analysis of 44 hitherto unpublished cases, *Obst.* 36-43
- White, H. P. W.**—Closure of suprapubic urinary fistula following suprapubic prostatectomy: observations on 68 cases, *Surg.* 119-125
- White-Cooper, W. R., and Griffith, H. K.**—Case of obstructed labour, *Obst.* 50
inversion of uterus occurring in third week of puerperium, *Obst.* 48, 49
- Whitehead, A. L.**—Discussion on case of ectopia lentis, *Ophth.* 12
discussion on case of endothelioma of orbit, *Ophth.* 37
on primary band-shaped opacity of both corneæ, *Ophth.* 32
on progressive macular degeneration with tremors, *Ophth.* 42
on standards of vision for students and teachers, *Ophth.* 10
on treatment of conical cornea, *Ophth.* 25
on unusual results of operations for cataract, *Ophth.* 24
some aspects of ocular tuberculosis, *Ophth.* 2-7
- Whitehouse, B.**—Adenomatosis vaginae, case, and treatment, *Obst.* 46, 47
discussion on cases of instruments &c., left in peritoneal cavity, *Obst.* 44
and **Featherstone, H.**—Two cases of Cesarean section under spinal anæsthesia with tropacocaine, *Obst.* 55-58
- Whitelocke, R. H. Anglin.**—Treatment of fractures of patella, *Surg.* 111-119
- Whitfield, A.**—Acarus from case of mange in human being infected by a dog, *Derm.* 75
case for diagnosis, *Derm.* 75
demodex impetigo, *Derm.* 28
discussion on case for diagnosis, *Derm.* 57
on case of leishmaniasis of skin, *Derm.* 10
resembling lupus vulgaris, *Derm.* 49
of multiple superficial rodent ulcer, *Derm.* 26
of xanthoma, (?) diabetorum, *Derm.* 93
- Whittingham, H. P.**—Life history of sand-fly, *Phlebotomus papatasi*, *Trop.* 45
sand-fly fever in Malta, *War* 1-14
- Whooping-cough**, mortality in 1880 and 1920 compared, *Med.* 47
- Widal, hæmoclastic test** in case of enlarged liver with persistent acetonuria and diaceturia (quoted), *Child.* 58
- Wigley, J. E. M.**—Case for diagnosis, *Derm.* 108
lichen spinulosus, *Derm.* 108
- Willcox, Sir W., K.C.I.E.**—Discussion on dental sepsis, *Odont.* 7
- Williams, A. W.**—Case for diagnosis, *Derm.* 71
- Williams, Everard.**—Discussion on treatment of dysmenorrhœa, *Obst.* 116
- Williams, E. C.**—Case exhibiting the Tooth-Marie-Charcot type of muscular atrophy, *Child.* 79
case of consanguinity, *Child.* 79
of pulmonary fibrosis, following tuberculous infection, *Child.* 79
- Williams, L.**—Discussion on the vagus and sympathetic nerves and their relation to climate and hydrology, *Baln.* 12
- Williamson, H.**—Discussion on adenomyomata of female pelvic organs, *Obst.* 91
discussion on radium treatment before operation for cancer of cervix, *Obst.* 35
- Williamson-Noble, F. A.**—Angeoid streaks of retina, *Ophth.* 1
atrophic patches at macula (?) tuberculosis, (?) cyst, *Ophth.* 32
endothelioma of orbit, *Ophth.* 35
a plane glass retinoscope, *Ophth.* 1
- Wilson, J. Leitch.**—Endocrine factor in mental disease, *Psych.* 21-30
- Wilson, S. A. Kinnier.**—Case for diagnosis, possibly amyotonia congenita, *Neur.* 49
case of syringomyelia, *Neur.* 49
congenital neurosyphilis in brother and sister, cases, *Neur.* 50
pathological laughing and crying, *Psych.* 39
- Wise, R.**—Glycosuria, resulting in birth of dead child, treated with success in subsequent pregnancy, *Obst.* 35
- Woodman, E. M.**—Case of sarcoma of nose cured by radium, *Laryng.* 49
demonstration illustrating certain pathological and surgical points in treatment of malignant disease in upper jaw, *Laryng.* 87
discussion on cases of intrinsic cancer of larynx treated by laryngo-fissure, *Laryng.* 62
on case of sarcoma of maxilla, malar and frontal bones, *Laryng.* 92
of ventriculo-chordectomy for double abductor paralysis, *Laryng.* 47
- Woods, W. W.**—Post-mortem report on case of pulmonary embolism and recurrent leiomyosarcoma, *Obst.* 63
- Woolf, A. E. Mortimer.**—Case of bilateral hammer great toes, *Clin.* 1
(?) epithelioma of penis, *Clin.* 1
traction lesion of right brachial plexus, involving 5th and 6th groups, *Clin.* 1
- Woolsorter's disease**, *Med.* 52
- Worster-Drought, C.**—Case of birth injury to brachial plexus; all cords of plexus originally involved; recovery of function in outer and posterior cords; paresis now of infraclavicular or Klumpke type, *Child.* 73
case of encephalitis lethargica, showing late results, *Child.* 32, 33

- Worster-Drought, C.**—Case of enlarged liver with persistent acetonuria and diaceturia, *Child.* 56
 of Huntington's chorea, *Neur.* 82
 of "juvenile" tabo-paresis, *Neur.* 81
 of juvenile general paralysis of the insane, *Neur.* 82
 discussion on birth injuries, *Child.* 78
 and **Dundas-Grant, Sir J.**—Nasal stenosis, mainly subjective, in case of Parkinson's disease, *Laryng.* 23
- Wright, A. J. M.**—Case of œdema of septum in association with nasal polypi, *Laryng.* 27
 lipoma of larynx removed by operation, *Laryng.* 11
- Wright, G.**—Primary carcinoma of liver excised by operation, *Surg.* 56
- Wrigley, F. G.**—Case of malignant disease (?) sarcoma of left antrum, *Laryng.* 96
 of paralysis of right vocal cord, *Laryng.* 96
 proptosis of right eye: with post-nasal catarrh, adenoids and enlarged tonsils, *Laryng.* 96
 and **Milligan, Sir W.**—Orbital cellulitis: invasion of frontal sinus: osteo-myelitis of frontal bone, case, *Laryng.* 90
 sarcoma of right tonsil and surrounding faucial region, *Laryng.* 91
- Wyard, S.**—Discussion on case of persistent jaundice in infant, *Child.* 18
- Wylie, A.**—Cystic laryngeal growth, *Laryng.* 44
 discussion on cases of intrinsic cancer of larynx treated by laryngo-fissure, *Laryng.* 62
 epithelioma of nasal septum, floor of both nostrils, alveolar surface upper jaw, and left side lower jaw, case, *Laryng.* 30
- Xantho-erythrodermia perstans**, cases of (J. L. Bunch), *Derm.* 81; (H. C. Semon), *Derm.* 103
 type of parapsoriasis, case (W. Fox), *Derm.* 91
- Xanthoma**, case of, (?) diabetorum (L. A. Savatard), *Derm.* 93
 diabetorum, case (J. H. Sequeira), *Derm.* 30
- X-ray appearances after operations for treatment of congenital dislocation of hips**, *Orth.* 20, 21
 in case of artificial pneumopericardium, *Electr.* 72-74
 in cases of congenital stenosis of duodenum, *Child.* 10, 11, 12
 of duodenal obstruction in infants, *Child.* 51-53
 of traumatic spondylitis, *Orth.* 4-10
 of adhesions of collapsed lung, difficulty of interpretation of, *Electr.* 51
 of bones in case of scorbutic infantilism, *Clin.* 18, 21
 of gall-stones, *Electr.* 82, 83
 of injuries in cervical region of vertebral column, *Orth.* 2
 of normal stomach comparing folds of mucous membrane, *Electr.* 92
 of pathological gall-bladder, *Electr.* 82-90
 of spine, advantages of use of Potter-Bucky diaphragm, *Med.* 68, 69
 doses, recording of, apparatus for (G. E. S. Phillips), *Electr.* 30
- X-ray examination of heart**, essentials of, *Electr.* 21
 pathology of osteo-arthritis of spine, *Med.* 67
 therapy in treatment of carcinoma of prostate, *Urol.* 78
 treatment, without operation, laryngeal case of epithelioma (possibly syphilis) completely healed and arrested under (Sir StClair Thomson), *Laryng.* 60
- X-rays in cardiological diagnosis**, *Electr.* 6
 composite apparatus for, *Electr.* 20-23
 mechanism for moving plate work, *Electr.* 25
 moving film mechanism, *Electr.* 26
 orthodiagraphy, *Electr.* 7
 radiography, instantaneous, *Electr.* 8
 radioscopy, *Electr.* 6
 recording apparatus, *Electr.* 24
 records by slit-diaphragm method, showing moving edge of heart in normal and abnormal conditions, *Electr.* 28, 29, 30
 screen observation, *Electr.* 6
 standard chart for measuring heart shadow, *Electr.* 23
 teleröntgenography, *Electr.* 7
 X-ray cardiogram, *Electr.* 19
- in diagnosis of calcified abdominal glands**, *Urol.* 8
 density and uniformity of shadow, *Urol.* 10
 difficulties attending, *Urol.* 8
 effect of respiration and change of position, *Urol.* 12
 grouping of shadows under, *Urol.* 10
 position of shadow in renal and gall-bladder areas, *Urol.* 8, 9
 size and shape of shadows, *Urol.* 9
 in diagnosis of fracture-dislocations of vertebral column, stereoscopic photographs necessary, *Orth.* 4
 introduction of, changes in medicine due to, *Med.* 46
 negative value of, in diagnosis of pathological gall-bladder, *Electr.* 81
 pulmonary tuberculosis as shown by, but without physical signs (S. Melville), *Electr.* 31-35
 radium and operation, relative advantages of, in treatment of severe and persistent uterine hæmorrhage, *Obst.* 77
 sarcoma of tonsillar region treated by, after partial removal, case (Sir J. Dundas-Grant and D. McKenzie), *Laryng.* 69
- Yaws**, history of introduction into India; with personal observations on over 200 initial lesions (A. Powell), *Trop.* 15-42
 absence of prodromal furfuraceous eruption in, *Trop.* 34
 auto-inoculation in, *Trop.* 31
 bibliography and references, *Trop.* 42
 cases of, in which syphilis was inoculated on patients suffering from, *Trop.* 40
 clinical description, *Trop.* 19, 20, 21, 22 *et seq.*
 general eruption in, *Trop.* 34
 heredity in reference to, *Trop.* 41
 in India, cases reported by various authorities, *Trop.* 17, 18

- Yaws**, inoculation on to sloughing or septic ulcer, results of, *Trop.* 26, 27
infection of, on syphilis, cases of, *Trop.* 40
introduction into India from Ceylon in 1887, *Trop.* 15
lymphatic glands and, *Trop.* 31
misleading descriptions often given to, *Trop.* 31
original lesions in, Numa Rat's description of, *Trop.* 31
plantar granulomata, type of, *Trop.* 35
plantar hyperkeratosis as sequel of, *Trop.* 41
primary, cases of (photographs), *Trop.* 21, 22, 23 *et seq.*
on scar of herpes, case, *Trop.* 24
primary lesion of, *Trop.* 18
a general eruption in 43 cases, *Trop.* 18
- Yaws**, rapidity of spread of, cases illustrating, *Trop.* 28, 29
satellite type of, *Trop.* 23
secondary eruption in, *Trop.* 19
spread of infection of, *Trop.* 15, 16, 17
tertiary sequelæ of, *Trop.* 36
views of various observers quoted, *Trop.* 31, 32, 33
- Yealland, L. R.**—Case of myotonia congenita, *Neur.* 45
hysterical fits, with some reference to their treatment, *Neur.* 85 94
- Young, W. A.**—Microscopical report on case for diagnosis, (?) leukæmia cutis, *Derm.* 13
- Young's** operation of total prostatectomy, *Urol.* 78



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